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S. HRG. 119–326

SANCTUARY CITIES: THE COST OF UNDERMINING LAW AND ORDER

HEARING

BEFORE THE

COMMITTEE ON THE BUDGET UNITED STATES SENATE

ONE HUNDRED NINETEENTH CONGRESS

SECOND SESSION

March 10, 2026

Printed for the use of the Committee on the Budget

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SANCTUARY CITIES: THE COST OF UNDERMINING LAW AND ORDER

TUESDAY, MARCH 10, 2026

COMMITTEE ON THE BUDGET,
U.S. SENATE,
Washington, DC.

The hearing was convened, pursuant to notice, at 10:00 a.m., in Dirksen 608, Hon. Lindsey O. Graham, Chairman of the Committee, presiding.

Present: Senators Graham, Grassley, Johnson, Marshall, Cornyn, Lee, Kennedy, Moreno, R. Scott, Merkley, Whitehouse, Kaine, Van Hollen, Luján and Padilla.

Also present: Republican Staff: Nick Myers, Majority Staff Director; Erich Hartman, Deputy Staff Director; Lillian Meadows, Senior Counsel; Meghan Green, General Counsel.

Democratic Staff: Ben Ward, Minority Staff Director; Melissa Kaplan-Pistiner, General Counsel; Joshua Smith, Budget Policy Director.

Witnesses:

The Honorable Chad Wolf, Former Acting Secretary U.S. Department of Homeland Security (2019–2020) and Homeland Security and Immigration Chair, America First Policy

The Honorable Michael L. Chapman, Sheriff of Loudoun County, Virginia

Ms. Jessica M. Vaughan, Director of Policy Studies, Center for Immigration Studies

Mr. David J. Bier, The Selz Foundation Chair in Immigration Policy, Cato Institute

Mr. Brendan Duke, Senior Director, Federal Budget Center of Budget and Policy Priorities

OPENING STATEMENT OF CHAIRMAN GRAHAM

Chairman GRAHAM. Good morning. Welcome to the Republican hearing on Sanctuary Cities. Are our Democratic friends on the way? Should we wait? Do we know if they're coming? Could somebody find that out?

I will go ahead and make sure our Democratic colleagues can have their time and ask their questions. So we're the Budget Committee, the mighty, mighty Budget Committee that did reconciliation and thank you very much. Right on cue. I would hope this Committee would take round two and see if we can do something on reconciliation round 2. But that'd be up to a group of us and talk to the leadership about that.

But this hearing today, I think it's timely. We're trying to look at the budget impact of a policy called Sanctuary Cities. Now there are 200, I think cities, 14 States—We got a chart here somewhere—that engage in the practice of Sanctuary Cities.

And what is that practice? Local officials, state local officials, basically have adopted a policy of not turning over to the federal government illegal immigrants subject to being turned over, because they don't like the policy. They don't want to engage in deporting these people.

Now, under President Obama, I think three million people were deported and I don't believe any Republican refused to work with the federal government to deport these people. And when President Obama deported three million people, it was called being strong and good government. When we try to deport people as a result of being invaded by illegal immigrants for four years under the Biden administration, we're not seizing bad guys.

I don't buy that at all. I think most Americans would like to get control over our country and sovereignty over our nation. Immigration done right can be a tremendous benefit to the country. When you obliterate the borders for four years and people from all over the world flood in, you got a problem and it needs to be fixed and cleaned up.

As to United States Immigration and Customs Enforcement (ICE), if we can make ICE more professional, if we can do things at Department of Homeland Security (DHS) that make it more professional, count me in. I think Markwayne Mullen will be a great Secretary to do that. But to my Democratic colleagues, we'll never fix illegal immigration until you get to the source of the problem.

And one of the problems we have is magnets to encourage future illegal immigration. I got 12 Sanctuary states, 200 cities. There are 25,000 people that ICE had a detainer on that were refused to be turned over.

Next one. We believe it endangers public safety, 10,000 criminals released by Sanctuary policies who went on to be arrested for additional crimes. They should have been turned over. They weren't. They stayed in the country and they committed crimes, $9 billion in fraud that we know of and probably growing. 59 percent of illegal immigrant households are on government welfare programs costing taxpayers 42 billion a year.

And that's some real, real money. Let me give you a real life example of what happens when you don't do this, right? Fairfax. I think we're going hear here in a minute from one of our sheriffs. Fairfax police warned prosecutors about a repeat offender months before deadly bus stop stabbing.

And this gentleman here, Jalloh, what's his name? Jalloh, was in custody and had a pretty bad rap sheet. People in the prosecution business say, don't release this—I mean deport this guy, turn him over. Don't release him. That was good advice.

It was not taken.

This young lady here, Stephanie Minter, was murdered by this man. And we had this man in detention in Fairfax County and they refused to turn him over for deportation. He was allowed to stay in the county and the rest is history. A young mom is dead because of that decision.

And who was this guy? He came to the country in 2012. He was arrested 30 times, rape, malicious wounding and assault. 2020, ICE submits a formal detainer to deport him. The request is blocked by Virginia's attorney for Fairfax County, a Soros-backed prosecutor and defender of Sanctuary Cities who ignored the detainer.

On May, 2025, he was arrested once more for stabbing and the individual in the leg, but was—but again released. 30 arrests. May 2025 stabbed somebody in the leg. Let him go again. February 2026, just months later, he murders Ms. Minter.

I'm not okay with that. I think that's offensive to who we are as a rule of law nation to the people in Fairfax County to this district attorney. I wish we could find a way for these people to sue you and make you pay a heavy price for ignoring your legal obligations under federal law and showing such poor judgment.

How many arrests is necessary? How many people need to be stabbed before a guy like that is deported? And what's the upside of keeping them here? Is this the model citizen we want to keep in America? Now there's an ideology around Sanctuary City that needs to be broken and we're here to break it.

I've got legislation designed to change this dynamic. Going forward if a local official or a governor, anybody refuses to cooperate with federal government in turning over people subject to deportation under federal law, they can be prosecuted because they should be prosecuted. You got to make it so that those who hang on to these illegal immigrants in defiance of federal law, that they feel the pain, not just the public.

So my hope is that we can find a way forward to break the cycle of the damage being done from Sanctuary Cities, the cost incurred to the taxpayer and what happened to Stephanie, just like never happens again. We're not going to quit on this. If you want to defend it, you're welcome to do it.

And this is not going away. You'll never have rational immigration as long as you have 12 states saying if you get here, you never leave. You're never going to fix this if you have 200 cities that practice Sanctuary policy, because the illegal immigrant believes I get there and never leave.

I've tried to work with Democrats like many over here to find a rational solution to our illegal immigration problem. We're not going to ignore four years. So for Joe Biden obliterated our border. People came in from all over the world by tens of millions, flown into the interior of the country and only God knows where they're at.

Look at the number of people on the terrorist watch list and how it's gone up 2,700 percent in '21, '22, '23. Where are these people? They don't know. I asked. Nobody knows where they're at. So in this invasion during the Biden administration, we had 11,000 murderers. Look at the number of people on the terrorist watch list. They're in our country and we don't know where they're at.

So what we're going to do is be willing to reform ICE, try to fix problems that we've seen, but we're not going to give up on the idea that we need to right wrongs here and deport people that present danger and try to regain control over our immigration system.

To my Democratic colleagues, what is the answer to Sanctuary Cities? Is it to continue the policy? Is it to eliminate the policy? All of us say eliminate it. I think the public is with us by like 80 percent. Until we eliminate the policy, you're going to see more and more stories like I just described and it is now time to call the question on Sanctuary Cities.

Will it be the norm going forward in America or will it change? To my colleagues on this side of the aisle thank you for supporting this effort. We're fully committed to eliminating Sanctuary Cities for the good of the country. To my Democratic colleagues, we'd love to join with you in a rational way to do some things to make ICE more professional.

But what you need to do in return is work with us to eliminate one of the biggest magnets to future illegal immigration, which is Sanctuary City policy. With that, Senator Merkley.

OPENING STATEMENT OF SENATOR MERKLEY [1]

Senator MERKLEY. Thank you very much Mr. Chairman. It's been a year since we've had a meeting of this Budget Committee and a lot has happened in that last year. Of course, one of the big things that happened was a piece of legislation that Trump called his Big Beautiful Bill, but most of us consider it a big ugly betrayal of the American citizen.

What did it proceed to do? Slash nutrition for hungry children, gutted health care for millions of families, enormously increased the wealth of the already wealthiest in our country. That's certainly not government by and for the people; that's government by and for the powerful. And it massively increased our national debt, estimated to be a $30 trillion increase over the next 30 years.

And if interest rates are one percent higher than used in the Congressional Budget Office (CBO) model, call that a $50 trillion increase in national debt over the next 30 years and it also massively increased the budget for ICE. I don't think you'll see many changes in budgets that look like this. You won't see a leap like this for housing in America, a fundamental need for families.

You won't see this for health care in America. In fact, health care was savaged by the Big Ugly Bill. You won't see this for education in America, but you certainly see it for ICE. Now these issues of the impact on nutrition budgets and health care budgets and housing budgets and deficit and debt and the budget for ISOs are all very good topics for this Committee to hold a hearing on, but we haven't held a hearing on that.

Now Trump has turned ICE into secret police. I can't believe that we now have secret police in America, but we do. I well recall studying secret police back when I was in college and the basic mark of them was they'd show up in unmarked vans, unmarked uniforms, no judicial warrant, knock down doors without a judicial warrant, grab people, detain them and often prevent them from having access and communication with families or lawyers.

All of that's happening right now in America. We now have a secret police called ICE and it's a tremendous black spot on our na-

[1] Prepared statement of Senator Merkley appears in the appendix on page 47.

tion that this has happened under this administration. Freedom and privacy, laws, norms and our Constitution all shredded.

Heartbreakingly, three American citizens have been killed by ICE. Renee Good, Alex Pretti in Minnesota and Reuben Ray Martinez in Texas. Now when ICE killed Reuben Ray Martinez, they hid the facts. So most Americans aren't aware that a third individual was killed. But recent reporting by CBS News obtained the body cam footage showing him being killed by an ICE officer in March of 2025.

Now ICE's budget has been increased by a factor of seven. The way that it operates has been dramatically changed from the norms of law and justice in a democratic republic to the strategy of secret police in an authoritarian regime. That would be a very good topic for a hearing, given how much ICE's budget has increased and we should be holding one in an examining that question.

Wars come with enormous costs. We know that the wars in the Middle East and Afghanistan and Iraq were staggering in terms of blood and treasure. More than 7,000 Americans killed, more than 50,000 Americans injured, often life-altering injuries, more than $6 trillion of national treasure expended.

So here we are. America's at war. That'd be a good topic for a budget hearing. Seven U.S. service members already killed. The Pentagon estimating a billion dollars a day. And in fact, some of my colleagues have said it's $2 billion a day, a big change in expenditures and revenues.

But none of those are the topics of today's hearing, the first we've had in a year. But we do have this topic that my colleague has pointed out, Sanctuary Cities. Well, Sanctuary is a bit of misnomer. Sanctuary states and cities are not places that are a safe haven where immigrants cannot be detained or deported.

It refers to the decision that local police will serve as local police and not be commandeered to be assistant ICE agents. These policies are rooted in the 10th amendment of the Constitution and my colleagues across the aisle like to lean on state's rights. It's certainly a state's right to refuse to have its local police commandeered to be assistant ICE agents, assistance now to ICE operating as a secret police in the United States of America.

So the idea that local police do local police work and ICE agents do ICE work is protected by our Constitution. But there's also a public safety rationale behind it. We know that when local police serve as local police, they build relationships with the community and decrease crime.

Research shows that jurisdictions that choose for local police to be local police and not assistance to a secret police, refusing to be commandeered as a federal agent, they have a higher median household income, they have less poverty, they have less reliance on public assistance, they have lower unemployment. Those all sound like pretty good things to me and they have lower crime rates.

A 2022 study found that allowing local police to be local police, not commandeered to be assistant ICE agents, results in a decrease in crime rates in those communities. And why? Well, because in places where they do get commandeered to be assistant ICE

agents, they get less information, less trust, less ability to prosecute and crime goes up.

So if my colleagues are all for crime, then persist in trying to violate the 10th amendment and the wise wisdom of those states that see the wisdom in separation with different roles. And there's nothing about local police being local police that hampers federal immigration enforcement. In fact, when people are arrested, fingerprints, data, crime are all put into a national database.

So the federal government has all it needs and if it wants to make sure that a person is handed over at the time that a person is completing their sentence in a local or state institution, all they have to do is get a judicial warrant, a judicial warrant. That's all that's required.

We've already seen the harm that happens when the executive branch tells the executive branch that it's okay to override Americans' civil rights. Reason: we have separation of powers in our Constitution, something being seriously eroded under this administration.

So as a reminder, each of these issues, slashing nutrition assistance, gutting health care, increasing the wealth of billionaires, exploding deficits and debts, transforming ICE into a secret police, launching a war, are all appropriate issues this Budget Committee should be focused on. But the decision by cities and states to enhance public safety by having local police focus on local police issues is not a new change.

And so it's puzzling to me why that's the decision for this first hearing in a year. What we know is that states and cities that let local police be local police do pretty damn well.

With that in mind, I've invited two expert witnesses to join us today, Mr. David Bier from the Cato Institute, an expert on Sanctuary policies and the broader concerns about the Department of Homeland Security and its $170 billion for immigration enforcement, with little accountability or oversight.

Joining him is Mr. Brendan Duke from the Center on Budget and Policy Priorities, which focuses on critical budgetary concerns, including how the big ugly betrayal law exploded our debt and raised the costs for working families. So I appreciate both of you joining us today.

And while he is not able to join us for today's hearing, Oregon Attorney General Dan Rayfield has written a statement and it explains how the home states, my home state Oregon, how their policy is to let police be police, has assisted and helped him and the law enforcement officers keep Oregon communities safe.

As he notes in his statement, Oregon's police policies have peacefully coexisted with seven presidential administrations. Mr. Chair, I ask unanimous consent to enter his statement into the record.[2]

Chairman GRAHAM. Without objection.

Senator MERKLEY. And I also asked unanimous consent to submit a statement for the record from the Law Enforcement Action Partnership,[3] which states "When local departments are pressured to prioritize federal immigration objectives, officer time and re-

[2] Statement submitted by Senator Merkley appears in the appendix on page 122.
[3] Statement submitted by Senator Merkley appears in the appendix on page 125.

sources are diverted from serious threats such as violent crime, domestic violence and community safety concerns.

"The result is a policing environment that is less effective and less trusted. Creating a less effective and less trusted local public safety environment should not be the objective of the federal legislature." Thank you, Mr. Chairman.

Chairman GRAHAM. Thank you very much. So the novel approach, the 10th Amendment protects Sanctuary Cities, I hadn't heard that one, but we'll talk about that later.

So our witnesses are Secretary, former secretary Chad Wolf, Acting Secretary of the U.S. Department of Homeland Security from 2019 to 2020 and the current Homeland Security Immigration Chair of the America First Policy Institute.

Sheriff Michael Chapman has been the sheriff of Loudoun County, Virginia since 2012. I think you're next door to Fairfax County. You can tell us about that.

Ms. Jessica Vaughan is Director of Policy Studies at the Center for Immigration Studies, where she has worked since 1992. Welcome to y'all. If you'd rise and raise your right hand, please. Do you solemnly swear the testimony you're about to give this Committee is the truth, the whole truth and nothing but the truth, so help you God?

PANEL. I do.

Chairman GRAHAM. Thank you. Welcome to y'all. Mr. Wolf.

STATEMENT OF THE HONORABLE CHAD WOLF, FORMER ACTING SECRETARY, U.S. DEPARTMENT OF HOMELAND SECURITY AND HOMELAND SECURITY AND IMMIGRATION CHAIR, AMERICA FIRST POLICY INSTITUTE [4]

Hon. WOLF. Well thank you, Chairman Graham and Ranking Member Merkley for the opportunity to testify before the Committee today.

What we now commonly call "Sanctuary Cities" was born nearly 50 years ago in California and since then has exploded nationwide. According to the Department of Justice, 12 states, the District of Columbia, as well as 22 other jurisdictions for now are officially considered Sanctuary jurisdictions.

But as the Chairman said, there are likely hundreds more. According to a 2024 report, nearly eight million illegal aliens resided in those jurisdictions and one in every three Americans or 120 million Americans live in Sanctuary jurisdictions.

To be clear, this is not a localized phenomenon. Ultimately, Sanctuary policies affect every American. And so while not all Sanctuary jurisdictions have the exact same policies, they all share a core belief that it is acceptable and even justified to ignore illegal immigration and violations of our immigration law.

But our law is clear. Title VIII, Section 1227 clearly states that aliens who entered the U.S. illegally or otherwise in the U.S. without lawful authorization are in fact removable. That is the mission of ICE. The law goes on to address those who harbor illegal aliens. Title VIII, Section 1324 states that any person who encourages or induces an alien to come enter or reside in the U.S. knowing or in

[4] Prepared statement of Hon. Wolf appears in the appendix on page 50.

reckless disregard of the fact of such coming to, entry or residence will be a violation of law.

Now due to the supremacy clause and a long list of judicial opinions interpreting that, states do not have the authority to pick and choose which federal laws are recognized and enforced. And they do not have the authority to nullify federal laws by ignoring them or proactively creating obstacles to their effectiveness.

Tragically, Sanctuary policies are unleashing devastating consequences for the American people, particularly those that live in those jurisdictions. In California, again the birthplace of the Sanctuary movement, local politicians released 4,641 criminal aliens from local jails in 2025, rather than handing them over to ICE for removal.

Those criminals included aliens that had committed Homicide, assault, sexual predatory offenses and burglary. And just weeks ago, as the Chairman indicated in Fairfax County, just miles from where we are today, a criminal illegal alien with more than 30 arrests fatally stabbed a woman in the neck and that county, Fairfax County, has been a Sanctuary for almost a decade and this is the tragic result.

Despite the dangerous nature of Sanctuary jurisdictions, there are some that will not stop until we've become a Sanctuary nation, largely free from all border enforcement and immigration enforcement. Calls for ICE-free zones, detention bans and even an outright abolishment of ICE are becoming increasingly common.

The evidence is clear to restore the rule of law, common sense and public safety, the era of Sanctuary jurisdictions need to come to an end permanently. Thankfully, President Trump and numerous states are taking action, but legal challenges persist and Sanctuary jurisdictions are still largely able to exist under current law. Congress should take action.

In my written testimony, I've outlined several recommendations to address this issue, including the need to hold local officials accountable for knowingly violating the law. Let us heed the lessons that we've learned from tragedies unfolding in cities like New York, Los Angeles, Chicago, Seattle, San Francisco and others. The American people are safer when state and local governments cooperate with federal immigration agencies. State and local governments should give our agents access to their jails and prisons, so that federal agents can screen prisoners, determine their immigration status, process removable aliens and transfer custody to federal authorities once an alien is released.

This requires little effort from local governments, yet under Sanctuary policies, state and local officials often slam the door shut on immigration officers.

I'd like to close with a simple question. For those who continue to support Sanctuary policies, how many more Americans need to die at the hands of illegal aliens released by Sanctuary jurisdictions? Sanctuary cities must become an artifact of American history, rather than the ongoing safe haven for drug dealers, narcoterrorists, murderers and dangerous people who threaten the safety and the security of the American people.

And let me just end on a comment made by the Ranking Member, talking about secret police. I think that's an outrageous com-

ment. I think the men and women of ICE are sworn law enforcement officers, they do their job, they're trained to do their job. They have a very dangerous job in what they do and to continue to denigrate their job and how they go about it in such a dangerous environment I think is a disservice to them.

Senator MERKLEY. Mr. Chairman, since the testimony involves the comment I made, I will just reply that if you don't want to be secret police, don't act like secret police. And I laid out the factual basis on exactly all the qualities that they are utilizing now that are the hallmark of secret police around the world. Thank you.

Chairman GRAHAM. Mr. Chapman.

STATEMENT OF THE HONORABLE MICHAEL L. CHAPMAN, SHERIFF, LOUDOUN COUNTY, VIRGINIA[5]

Hon. CHAPMAN. Chairman Graham, Ranking Member Senator Merkley and other Members of the Committee. I am Loudoun County Sheriff Mike Chapman and lead Virginia's most complex and largest full-service sheriff's office, employing over 900. Loudoun County sits just outside of Washington, D.C. and is the home to Leesburg, Middleburg and the Dulles International Airport.

In Loudoun County, we celebrate a diverse community, have an extraordinarily diverse workforce. We welcome everyone here that's here legally and within our organization speak over 25 different languages. I have served 47 years in this profession as a local police officer, a career Drug Enforcement Administration (DEA) special agent and for the past 14 years as an elected sheriff.

I am honored to sit before you today to discuss our mutual concern of Sanctuary cities and counties. I am fortunate that Loudoun County is part of a traditional form of government that enables a sheriff to conduct all law enforcement, jail and court operations under one roof, essentially making me both a sheriff and a police chief.

This differs significantly from our surrounding jurisdictions where appointed county, city and town police chiefs are the norm. Loudoun's form of government has enabled me to make decisions in the best interest of our citizens, free of mandated political agendas. It has also allowed me to be the only Republican law enforcement leader in all of Northern Virginia.

That responsibility is a double-edged sword. While I don't answer to our 7–2 Democrat Board of Supervisors for policy decisions, I do receive my budget from them. This dichotomy causes significant friction, making this a very challenging area. Our system of government, however, provides our citizens with a check and balance.

We endured a two year battle with our Board of Supervisors who pursued rather converting our sheriff's office into a police department several years ago, to exercise complete power and control over our agency. We defeated this aggressive action because our citizens ultimately decided that they prefer having a direct voice in who leads their public safety through the election process.

This system and our common sense approach to problems putting citizens first is why the Sheriff's office has been awarded favor,

[5] Prepared statement of Hon. Chapman appears in the appendix on page 55.

government service for two years running in our local newspaper, and why I've been selected as favorite public servant for the past five years.

Regarding immigration, it is important to distinguish criminal aliens who pose a threat to our community from immigrants here legally. Consequently, while our surrounding jurisdictions choose to be sanctuaries, not make this distinction and not honor ICE detainers, we do just the opposite. We have always cooperated with all of our state, local and federal counterparts including ICE, and have routinely notified ICE of those in our custody for criminal offenses who also had active detainers issued by ICE regardless of the presidential administration.

We honor all detainers and warrants from other law enforcement jurisdictions, just as I would expect them to honor ours. These practices likely contribute to why our agency has the lowest crime rate in the entire Washington D.C.–Northern Virginia area, and is one of the safest counties in the entire United States.

That doesn't mean we're exempt from problems related to illegal immigration. In 2024, we experienced five homicides, three of which were committed by people here illegally. Two of the three were committed by an Mara Salvatrucha 13 (MS–13) gang member and in a neighboring county, one that refuses to contact ICE regarding the criminals they arrest, two homicides were committed in just over the past two months.

One occurred within 24 hours after the criminal who had an outstanding ICE detainer on him was released from jail without any notification to ICE. The other, police questioned why the subject had been released and warned the prosecutor of danger, stating not if, but when he would assault again, resulting in a grandmother being stabbed to death at a bus stop. This exemplifies what happens with Sanctuary counties. Law enforcement's hands are tied by politicians and political agendas.

Last year, I signed a 287(g) warrant service officer program agreement, which enabled us to hold a person arrested for a criminal violation an additional 48 hours for ICE, in the event ICE couldn't pick them up immediately. This enabled us to safely turn over criminal illegal aliens to ICE who otherwise would have been released into our community and could pose an additional threat to our residents.

This simple process has kept ICE from having to conduct street operations in our area. I have told my community that not turning over criminal aliens to ICE, who could endanger our residents, would be irresponsible and dangerous to the very people I've sworn an oath to protect. This process, which has worked well for us, is likely to become far more challenging considering the legislation currently in motion in Richmond.

Our governor rescinded the previous administration's executive order, which enabled jurisdictions to cooperate with ICE. This was followed by legislation proposed by both houses of our general assembly, which if passed and signed by the governor, will likely require judicial warrants from ICE for local law enforcement to act on anything.

Considering illegal immigration is a civil offense, this action will no doubt force us to release detainees incarcerated by us for crimi-

nal offenses unrelated to immigration back into our communities, where they could pose an additional threat to our citizens.

If this Committee can exercise any leverage to influence a different outcome, I am confident that law enforcement officials throughout Virginia and other locations who are familiar with these challenges would be grateful. We take our jobs seriously and want to keep all of our citizens safe. Thank you for your attention and, I look forward to answering any questions.

Chairman GRAHAM. Ms. Vaughan.

STATEMENT OF JESSICA VAUGHAN, DIRECTOR OF POLICY STUDIES, CENTER FOR IMMIGRATION STUDIES [6]

Ms. VAUGHAN. Good morning and thank you. More than half of removable aliens live in those jurisdictions that have Sanctuary policies intended to hinder immigration enforcement and prohibit cooperation between state and local law enforcement and federal immigration enforcement, which is almost always a violation of federal law and imposes enormous costs on taxpayers.

My organization has been tracking these policies since 2015, and we maintain a list and map of jurisdictions and policies on our website. Those who promote Sanctuary policies often claim that they're needed to build trust with immigrants, so that they'll report crimes or to avoid distracting local law enforcement agencies with immigration enforcement.

These claims are disproven and disingenuous. In fact, Sanctuary policies are an irrational political response based on disagreement with federal immigration policy and opposition to enforcement of immigration laws. The motivations for Sanctuary policies are ideological and political, but the cost of Sanctuary policies is unfortunately very tangible.

First, the public safety costs, and these are the most significant and problematic. Between October of 2022 and February of 2025, Sanctuary jurisdictions refused more than 26,000 detainers for criminal aliens. Half of these were in California and large numbers also were released in Illinois, Virginia, Massachusetts and Connecticut.

The individual jails that released the most were Santa Clara County, California, Cook County, Illinois and Fairfax County, Virginia. In 2024 alone, the Fairfax County sheriff declined to honor 744 detainers for criminals that she had in custody. That's more than two a day. Some of these are gang members with long rap sheets who get arrested over and over, costing county taxpayers, untold sums in repeated crimes, prosecution and incarceration. Others are child predators and sex offenders.

Oregon is another place that protects criminal aliens with strict Sanctuary policies. Last fall, ICE actually had to issue subpoenas just to try to get four Oregon sheriffs to share information on violent felons who had been released despite ICE detainers. They had convictions for rape, assault, child exploitation and kidnapping, among other crimes.

What possible reason can there be for not cooperating with ICE for their removal? It's certainly not a public safety reason, and it

[6] Prepared statement of Ms. Vaughan appears in the appendix on page 57.

seems as if they actually want to make immigration enforcement as costly and labor intensive as possible. Besides blocking law enforcement agencies from cooperating with ICE Sanctuary states typically have other policies that attract illegal settlement and thus burden taxpayers with support of illegal immigrants.

Besides funding emergency health care and schooling for all, a number of Sanctuary states go farther and choose to provide Medicaid subsidized health insurance, nutrition assistance, housing and much more. Illegal immigrants use these welfare programs in large numbers. My organization has found that over 60 percent of illegal alien-headed households are using at least one form of welfare program and usually more than one, at a cost of tens of billions of dollars to taxpayers.

This is why any resources spent on immigration enforcement actually save taxpayers money, taxpayer money in the long run. It is my repeated observation that the politicians who support Sanctuary policies are not moved by the costs or the pain their policies have caused American families.

Victims like Stephanie Minter, Denny McCann, Drew Rosenberg, Matthew Denice, Dominic Durden, Laken Riley and Kate Steinle and so many others. These are priceless lives lost to Sanctuary policies, and it's wrong to trivialize their loss as a statistical blip.

Some states like South Carolina, Florida and Texas, have banned Sanctuary policies. The Trump administration is making progress through outreach and litigation, but while the federal government likely cannot force Sanctuary jurisdictions to do the right thing, it can create consequences for sanctuaries. Congress needs to create additional consequences like disqualification from certain federal funding, so that taxpayers are not subsidizing sanctuaries.

Congress should also clarify that state and local police have the authority to cooperate with immigration enforcement, so that they can't be blocked from doing it by state or local laws. In addition, those who are seriously harmed as a result of Sanctuary policies should have some resource in the courts. Thank you.

STATEMENT OF DAVID J. BIER, THE SETZ FOUNDATION CHAIR IN IMMIGRATION POLICY, CATO INSTITUTE [7]

Mr. BIER. Chairman Graham, Ranking Member Merkley and distinguished Members of the Committee, thank you for the opportunity to testify.

For half a century, the Cato Institute's research has shown that people, whatever their ancestry, background or birthplace, can thrive in a free society. Immigrants come to work for us and with us to make our lives better. They work at higher rates and are nearly twice as likely to start businesses here.

Immigrants have reduced government deficits by 14 and a half trillion dollars over the last 30 years, and yet Congress has not updated the legal immigration system during that entire time. So just three percent of all legal immigrant applicants were approved in 2024.

Rather than fix this broken system, the new administration banned half of all the legal immigrants who had made it to the end

[7] Prepared statement of Mr. Bier appears in the appendix on page 67.

of that process, including spouses and children of U.S. citizens. The fallout from this flawed system is left to our cities to manage. In many areas one in five residents lives with someone here illegally.

Mass deportation is not an option for these places. It is an attack on them. It is taking Americans, Americans away from their spouses, parents, pastors, parishioners, employees, employers, customers and friends. It's not us Americans versus them immigrants. Cities know immigrants are part of us. They also know that immigrants are here to build, not destroy.

A mountain, literally a mountain of research proves that immigrants and reasonable limits on participation with ICE do not increase crime rates. Immigrants, even illegal immigrants, are half as likely to be incarcerated for crimes in the United States. According to DHS, only about three percent of illegal immigrants have any criminal convictions.

Immigrants reduce crime rates, which means your less likely to be a victim of a crime. Let me be clear. When a non-citizen does victimize someone, they should be convicted, imprisoned and deported. But it is DHS that has ignored this formula. Just 28 percent of the people detained by ICE right now were convicted of any crime at all.

This is no surprise. DHS has abandoned its public safety mission. It has stripped millions of vetted immigrants of their legal status, just so that it could deport them. Instead of protecting us, DHS violates the Constitution. Its massed agents invade our homes without warrants. They detain without evidence, and they deport without due process. They intimidate, beat and even kill Americans who protest. They arrest legal immigrants for writing opinion articles. They lie to the courts and then they ignore them.

With all this unconstitutional mayhem, Congress should not itself violate the Constitution by threatening local law enforcement, not for interfering with ICE, but rather for following their own local laws that they swore an oath to uphold.

Under our system of federalism and the Constitution, Congress can't force cities to do its bidding, whether on immigration, guns or the environment. If Congress wants cities to help DHS, it'll need those cities to trust DHS. But to get trust you need to be trustworthy, and this DHS is not.

Consider this. Before this administration, non-citizens were more likely to report serious crimes to the police than the U.S.-born population. Now, DHS is arresting immigrant crime victims. For example in Iowa, a shooting victim who helped bring to justice violent robbers was arrested by ICE before he had even healed.

But since he had a pending charge for an unpaid traffic ticket, ICE considers him a criminal. This has caused filings for U visas for crime victims working with police to drop by 63 percent. This is not making Americans safer. Congress must require DHS to focus on real threats, and it must empower Americans to sue federal agents for violating the Constitution. We need accountability and constitutionality to restore trust. Thank you.

STATEMENT OF BRENDAN DUKE, SENIOR DIRECTOR, FEDERAL BUDGET, CENTER ON BUDGET AND POLICY PRIORITIES [8]

Mr. DUKE. Thank you. Chair Graham, Ranking Member Merkley and Members of the of the Budget Committee. My name is Brendan Duke, and I'm Senior Director for Federal Budget Policy at the Center on Budget and Policy Priorities.

The Center on Budget is a non-partisan research and policy institute that advances federal and state policies to help build a country where everyone has the resources they need to thrive and share in the nation's prosperity.

Let me first begin with an overview of our fiscal situation for the Budget Committee. Analyses typically focused on government spending and revenue as shares of the economy. In 2025, revenue was 17 percent, which is around the same level as in 1985. Programmatic spending on the other hand grew seven-tenths of a percentage point since 1985.

But Social Security and Medicare are responsible for more than 100 percent of the increase in spending, because the country has gotten older. That is nobody's fault. I'm sure everyone on this Committee wishes they had the same knees or back they did 40 years ago. Similarly, we all wish we had the demographics and ratio of working age Americans to retirees that we had 40 years ago.

The combination of population aging in basic arithmetic means we will face a choice between raising revenue past the 17 percent of Gross Domestic Product (GDP) level set when the country's demographics were more favorable, or making drastic cuts to important programs.

The retirement of the baby boom was a predictable and predicted development. But at the beginning of the 21st century, we had a fiscal system that would deliver adequate projected revenue to meet our needs. Rounds of unpaid†for tax cuts ruined that. Take a look at projections for 2025 that were made right before most of the Bush tax cuts were made permanent, and before anyone even thought of the Trump tax cuts and compare them to the actual 2025 numbers.

The 2025 deficit is much higher than projected in 2012, despite programmatic spending being lower than projected. The reason the deficit roses revenues come in far lower than projected as a result of those unpaid-for tax cuts. Importantly, Social Security, Medicare and other health programs such as Medicaid and premium tax credits also came in below CBO's 2012 projections.

What all of this ultimately shows is that it's entirely possible to meet our commitments to seniors, while ensuring millions of low and moderate income Americans have health care and food if we are willing to raise the revenue to do so. Instead, Congress made a different choice last summer. The giant reconciliation bill adds $3.4 trillion to the deficit, as it extends tax cuts tilted to the wealthy despite cutting Supplemental Nutrition Assistance Program (SNAP) and Medicaid by about 20 percent by 2034.

CBO more recently estimated this figure at $4.7 trillion over the new budget window and including interest. Notably, this $3.4 tril-

[8] Prepared statement of Mr. Duke appears in the appendix on page 84.

lion cost is higher than the cost of extending all of the expiring tax cuts for all families, including for millionaires, but without the cuts to SNAP and Medicaid, and it would have cost far less than, to extend the tax.

That's for the 98 percent of families making under $400,000 without those cuts to SNAP and Medicaid. In other words, the cuts to health care and food assistance in the reconciliation bill were entirely the result of Congress's choice to load the bill with tax cuts beyond the 2017 tax cuts, along with large increases for defense and immigration enforcement.

The result is millions of low income families will lose the ability to afford the high cost of groceries or go to the doctor when they're sick, cuts that a vast majority of the public staunchly oppose. Moreover, the entire cost of the tax cut was essentially offset by the Trump administration's decision to raise taxes on imported goods.

Cutting income taxes financed by taxes on imported goods is a poor trade. It is both economically inefficient and further pushes the burden of financing the government from high income to low and middle income families.

In fact, the combo of the Trump administration's tariff regime and the spending cuts on the reconciliation bill will leave households in the bottom 70 percent worse off. The Supreme Court last month handed the Trump administration an opportunity to save face by ruling that many of these sweeping tariffs were illegal.

Instead of using the Court decision to back down on a failed policy, Trump has instead replaced those struck down tariffs with a sweeping 10 to 15 percent across the board tariff. I think we can all agree that the steps required to significantly improve the outlook, whether through revenue increases, program cuts or both can be politically challenging.

Immigration can reduce the need for those difficult measures. Immigrants contribute to our communities and the U.S. economy in many ways. In particular, they improve our fiscal outlook by boosting our national birth rate and they boost our workforce. Last month, CBO evaluated the economic and fiscal effects of the administration's anti-immigrant policies. CBO cut in half the projection of the annual growth rate of the U.S. population over the next few 10 years.

CBO now estimates that the 2035 working age population will be 2.4 million people smaller than in its previous projection. This has important fiscal costs too. CBO projects that the administration's actions to reduce immigration will increase deficits by $500 billion over ten years, principally by reducing the number of taxpayers.

Those higher deficits will make deficit reduction that much harder, adding to the damage from H.R. 1. Financing income tax tilted to the wealthy by cutting programs and raising import taxes while discouraging and disparaging immigrants is no way to fix our country's fiscal imbalances.

Thank you.

Chairman GRAHAM. Thank you all for your testimony.

We'll do five minute rounds.

Mr. Bier, is it accurate that Barack Obama deported three million people when he was president?

Mr. BIER. Those are total removals, not people deported from the interior.

Chairman GRAHAM. What does that mean?

Mr. BIER. Well, there are people deported from the borders who never lived in the United States.

Chairman GRAHAM. Were there people deported inside the United States?

Mr. BIER. Well, I mean I think it's a little bit unfair to compare people deported at the border——

Chairman GRAHAM. Oh, hey, hey, hey.

Mr. BIER [continuing]. Versus people in the interior. I think Jessica would agree to this.

Chairman GRAHAM. So hey. Hey listen, listen. You don't want to answer a question, that's fine, but don't tell me what's fair and unfair. Bottom line. Did he use judicial warrants to deport anybody? When he deported somebody interior, did he have a judicial warrant?

Mr. BIER. No.

Chairman GRAHAM. No. Why? Because it's stupid. Mr. Wolf, tell me what would happen if we adopted his approach. You can't deport anybody unless you have a judicial warrant. Would that be effectively the end of deportation?

Hon. WOLF. I think it would. I mean if you just look at——

Chairman GRAHAM. Don't you think that's what they want to do?

Hon. WOLF. In part, absolutely.

Chairman GRAHAM. These people over here want to stop deportation.

Hon. WOLF. So if you just look at the 1.5 or 1.6 million individuals here with a final order of removal, right? They've been through the judicial process. They've been to an immigration judge, probably an Immigration Board of Appeals, and every time the judge says, you don't have a legal right to be here.

If you had to get a judicial warrant for each one of those, you'd be here for the next 20 years trying to remove those 1.6——

Chairman GRAHAM. But when a Democrat says you need a judicial warrant to deport somebody, they're turning—Obama didn't do it, did he?

Hon. WOLF. Not that I'm aware of, no.

Chairman GRAHAM. No. No, he didn't because it's impossible. You just stop the whole system. You're not trying to reform ICE. You're trying to stop deportations. If it were up to you, we'd have more illegal immigrants. I hope people are paying attention to what the hell is going on in this country.

Sheriff, tell me about what it's like to live next to a Sanctuary county.

Hon. CHAPMAN. Well, we're the only ones that actually work with ICE and are able to get people out based on our 287(g). We turn them over to ICE. We arrest them for criminal violations outside of their immigration status. They come and they pick them up and they're gone. And I'll tell you, it's made a big difference. We had——

Chairman GRAHAM. Well, those laws that you're enforcing, were they on the books before Trump became president?

Hon. CHAPMAN. Yes.

Chairman GRAHAM. So all these laws were there before Trump became president and Obama used them. Now, when we use them, all of a sudden we're Nazis. I think people see through this. Do you believe Sanctuary City policy is bad for crime, Mr. Chapman?

Hon. CHAPMAN. I do. I think, you know, the ability—if you look at us and compare it to our next door neighbor there they, I think, maybe have notified ICE maybe about seven times last year, where it was 184 times for us.

Those are people that could have been released back to our community, that could have committed other crimes, and we don't need that here. It's irresponsible of me to allow that to happen and let these people back in our community.

Chairman GRAHAM. Ms. Vaughan, how would you—what's your takeaway about Sanctuary City policy? What damage has it done to our country, if any?

Ms. VAUGHAN. Well, there's the cost of the lives lost to illegal aliens who were released by Sanctuaries. That's priceless. There's the fiscal——

Chairman GRAHAM. This case behind me, Jalani. Is that his name? He should have been deported. Do you agree Ms. Vaughan?

Ms. VAUGHAN. Absolutely, yes.

Chairman GRAHAM. You know, you're talking about suing somebody who's been abused by the government. Why don't we sue local prosecutors who put this in motion? I'd love to do that.

So I'm going to try to find a way to give victims a chance to go to court when a local prosecutor puts back on the streets somebody with 30 priors. That's some—do you support that idea, empowering people?

Ms. VAUGHAN. I do. I think that that would be an effective deterrent to Sanctuary policies, because they're adopted——

Chairman GRAHAM. Make these counties and cities pay.

Ms. VAUGHAN. Right. They're adopted for political reasons, not public safety reasons.

Chairman GRAHAM. Yeah. What are—what's the upside of ignoring federal law and keeping people like this out of federal custody? It's a political choice.

So let me ask you this question. There was a clerk of court, Mr. Wolf, in Kentucky that did not want to issue a marriage license to a gay couple, in spite of the fact Supreme Court said that gay marriage was legal, and she went to jail. Do you think that's a good outcome? Should she have gone to jail for refusing to obey the law?

Hon. WOLF. I think it's important to obey the law.

Chairman GRAHAM. Yeah, I do too.

Hon. WOLF. The law is the law.

Chairman GRAHAM. And when I suggested that the clerk of court who had a religious view, that she could not in good conscience issue a marriage license to a gay couple, even though the law required, she needed to get out of her job. I was—boy, what a great guy I was then.

Now the shoe's on the other foot. All of a sudden, that's not—that's not a good idea. You can—what you want to do is just ignore the laws you don't like. Is that where we're headed, Mr. Wolf?

Hon. WOLF. I believe it is.

Chairman GRAHAM. Well, let's stop it now. Let's end this madness. There is no upside to Sanctuary Cities. It's all downside. Senator Merkley.

Senator MERKLEY. Well, no upside other than, higher median household income, less poverty, less reliance on public assistance, lower unemployment and safer communities, all of which seem pretty much on the upside from my point of view.

Mr. Duke, what effect did the Big Beautiful Bill, as Trump called it, have on debt over the ten year budget window?

Mr. DUKE. CBO shows that it adds $4.7 trillion to the deficit.

Senator MERKLEY. That's a pretty whopping sum.

Mr. DUKE. It's enormous.

Senator MERKLEY. For one piece of legislation, and over 30 years?

Mr. DUKE. It comes to $30 trillion with a T.

Senator MERKLEY. Wow.

Mr. DUKE. Yeah.

Senator MERKLEY. What's our national debt right now?

Mr. DUKE. As a share of GDP, it's around 100 percent.

Senator MERKLEY. And that's about $30 trillion?

Mr. DUKE. Yeah.

Senator MERKLEY. So another $30 trillion. But if interest rates are one percent higher, how much more does that add to the debt?

Mr. DUKE. I can't believe I'm saying these numbers, because they sound like they're from Star Trek, but $50 trillion.

Senator MERKLEY. Yeah, $50 trillion. Well, thanks a lot folks for passing a bill that's going to add $30 to $50 trillion to our national debt, suppressing our investments in health care, housing, education, public safety, transportation, all important things for the welfare of America.

And Mr. Duke, how did that bill affect the size of the working age population?

Mr. DUKE. Yeah. So basically the combination of that—so first that bill funded ICE a lot, as you showed, and CBO says that it is going to shrink the number of people who are in the workforce, because they are going to leave the country or be in detention camps. That directly shrinks our tax base.

Senator MERKLEY. Okay. So that has an overall effect on the GDP of our nation?

Mr. DUKE. Fewer working people means smaller GDP.

Senator MERKLEY. Okay. Mr. Bier, in states or cities that have policies referred to as Sanctuary policies, but more accurately that dedicate local police to doing local police work, do those locations fully cooperate in detaining somebody when there's a judicial warrant for them?

Mr. BIER. Yes, absolutely. They turn over anyone subject to a criminal matter upon notice by the federal government that they have a warrant.

Senator MERKLEY. So those jurisdictions have entered into the Federal Bureau of Investigation (FBI) federal database fingerprints, arrest record, what that person's being detained for, how long they're being held. Federal government has all that information.

Mr. BIER. Exactly, yes. So they could go and get those individuals anytime they want. And in this case in Virginia, for example, the Laken Riley Act, which they all supported, directed the Department of Homeland Security to get those people.

And instead of getting those people, they're out on the streets profiling random Hispanic workers going to their jobs. So it's a total misdirection to blame the county, when the Department of Homeland Security isn't even following the law that they passed.

Senator MERKLEY. Why not, since they could get that warrant and pick up those individuals who have committed violent crimes?

Mr. BIER. Because the White House has created an arrest quota that's the only metric for success for them. How many arrests can you make today? And the fastest way to make arrests is indiscriminate raids, indiscriminate profiling of people on the street.

That's what's led to this huge jump in the number of people, and the frankly chaos that we're seeing in cities across the country.

Senator MERKLEY. So there was a chart in your longer written testimony showing that only five percent of the individuals booked by ICE have ever had a violent criminal conviction.

Mr. BIER. Yeah. And not only that, if you look at my written testimony as well, I talk about how the Department of Homeland Security has its own internal threat assessment that they assign to each individual who comes into their custody.

And those people have to prioritize. They actually have a mandate. They can't be all political. They have to figure out who is actually a threat. And of those people, only ten percent received the highest threat level and 70 percent were deemed by the Department of Homeland—this Department of Homeland Security, to not be a threat at all.

Senator MERKLEY. So how does that match the campaign rhetoric about we're going to go after the worst of the worst, which I think 100 percent of Americans supported the idea that anyone who had committed a violent crime should be—should be investigated, should be prosecuted, should be if convicted and incarcerated should be deported. But those numbers don't match that.

Mr. BIER. Great campaign messaging, great messaging for, you know, a $200 million ad campaign by the DHS Secretary. Not based in reality at all. On day one of this administration, they rescinded all policies that forced ICE to focus on violent offenders and people with criminal convictions.

Senator MERKLEY. They did the exact opposite of what they told the citizens they were going to do during the campaign.

Mr. BIER. It was a lie, just like it was a lie about all the things they said about Renee Good and Alex Pretti and all the other people that they've killed over the last year.

Senator MERKLEY. And you recommend—do I have time for the last question, or my time has expired?

Chairman GRAHAM. Sure. Go ahead.

Senator MERKLEY. You recommend in your testimony that there be a private right of action when individuals' civil rights are violated. Now my understanding is that if local police or state police violate your civil rights, you have a cause of action against that; is that correct?

Mr. BIER. Yeah. Under Title 42, Section 1983, there's a private right of action against state and local, but not against the feds.

And this is a critically important issue, because right now they're recruiting all these state and local officers into essentially the ICE framework, which gives them immunity against all constitutional violations that they carry out while working in conjunction with the federal government.

So we need to expand that to cover federal officers as well, to make sure we don't have this immunity creep into every police department in America, which is their goal, including through this bill that we're talking about today.

Senator MERKLEY. So when the Vice President said ICE agents have absolute immunity in violating people's civil rights, he was right?

Mr. BIER. Effectively, yes. Right now, as the law stands.

Senator MERKLEY. Thank you, Mr. Chairman.

Chairman GRAHAM. Thank you. Just one point of clarification. Is it your position that a non-violent illegal immigrant shouldn't be deported?

Mr. BIER. My position is that we should give people who are peaceful people a path to citizenship. You endorsed that principle for many years, Senator.

Chairman GRAHAM. So no matter how many people come?

Mr. BIER. No. It should be based on whether people have a demonstrable basis to remain in the United States, which is whether they contribute to this country or whether they're a threat to it.

Chairman GRAHAM. I guess——

Mr. BIER. We should have all laws——

Chairman GRAHAM. I guess what you don't understand, and I don't want to, I'm familiar with the Gang of Eight bill, shut down illegal immigration. We did away with the magnets. We're going to give people a chance who've been here for a long time.

Mr. BIER. Your bill had the exact opposite approach of what this Department of Homeland Security is carrying out.

Chairman GRAHAM. Listen, listen. For four years, Biden obliterated the border. 11 million people came in and the bill I was working on would have prevented that. He took everything we were trying to do to stop illegal immigration, be fair to the ones here and blew a hole in it, and now we got 11 million more and you want to give them amnesty. They'll never stop. Senator Grassley.

STATEMENT OF SENATOR GRASSLEY

Senator GRASSLEY. Yeah. The United States is a country based on the rule of law, and it's against the law to enter our country without our permission.

And I compliment the Chairman for having this hearing, and his strong statements and his strong questioning. We all know that our country benefits from legal immigration and entering our country. This is a Budget Committee, and my questions will basically be based upon budget impact and cost to our government.

Mr. Wolf, there are some legal limitations that prevent states from completely excluding illegal immigrants from access to some of their government programs. What are those limitations and what immediate steps considering those limitations can the federal

government take to minimize the fiscal burden of Sanctuary jurisdictions?

Hon. WOLF. Yeah. I think when we talk about public benefits, whether they're federal or state benefits, I think and the Chairman I believe mentioned this earlier, these are strong magnets of why folks decide to come to the United States illegally.

Now there's a variety of different reasons for them to do so. But if they're able to stay in a jurisdiction that shields them from law enforcement, that provides public benefits, particularly at the state level, but also at the federal level, both need to be looked at, that continues to incentivize those individuals to come and to stay.

And so when you look at how to secure the country, it's it not only border security, it's a big part of it, but it's also getting at these pull factors, and those public benefits are a big part of that. I think the American people are generous. I think they want to help people that need true help.

But to have folks that come here to the country illegally choose to stay illegally, have no protections under U.S. law and continue to get public benefits, I think that's where the American people draw the line.

Senator GRASSLEY. Ms. Vaughan, Sanctuary jurisdictions frame their policy as squarely a matter of local policy. But we know these policies have both broader public safety and fiscal effects on areas beyond their border. So what are some examples of the cost to both the federal government and the state government caused by Sanctuary policies that are often forgotten or overlooked, with emphasis upon forgotten or overlooked?

Ms. VAUGHAN. Well, I think too many of the victims of these policies have been forgotten. I named a few of them, but these families are still suffering silently. Another forgotten cost is the cost of providing benefits, taxpayer-funded social services to illegal alien households.

And federal law currently allows states to choose to do that if their legislature approves it, at enormous costs, and some of those dollars are federal dollars. So all of us as Americans share the cost burden of that arrangement, and Congress can do something about that.

But there's also the cost of the erosion of the integrity of our immigration system, when individual states and jurisdiction and local jurisdictions can obstruct the enforcement of federal laws that are passed by Congress without consequence. Even though they are—many of these Sanctuary policies are clearly in violation of Section 1373 and 1644 of the Immigration Act.

And so, you know, it's a slap in the face to legal immigrants to have—say allow these policies that shield individuals from immigration enforcement, most of whom are criminal aliens. Immigrants don't want to live with these policies either. Something like 70 percent of the American public is opposed to Sanctuary policies for all of these reasons.

Senator GRASSLEY. Mr. Wolf, if you can answer this question. It's in regard to the 11 million people that came here under Biden and Harris. Over that period of time, did illegal immigrants seek out Sanctuary jurisdictions for safe harbor? And is there any evidence to back up your statement?

Hon. WOLF. Well, I think if you look historically at where illegal aliens reside now, they reside all across the United States. But if you were to do a heat map, you would see them in—the majority of them in places like New York City, Chicago, Los Angeles and others that have these Sanctuary jurisdictions.

So, yes. I do believe over time that illegal aliens do tend to, go to these types of jurisdictions.

Senator GRASSLEY. I got six second left. Sheriff Chapman, what are some examples of state and local cost increases those jurisdictions incur?

Hon. CHAPMAN. Well, I look at the funding that goes to our state and local, the folks that surround us and folks that do not support, you know, the immigration policies here and the funding that they get. And I think if that was curtailed, maybe they might look at this a lot more seriously.

But it is a cost to us. It's a cost of putting people out there. You're talking about more homicides, you're talking about—we've had our share locally here and certainly as a Chairman brought up, and we've had them in Loudoun County. And it's a human factor that is really important here. We want to keep our people safe and keep them alive.

Chairman GRAHAM. Senator Whitehouse.

Senator WHITEHOUSE. Thank you, Chairman.

Chairman GRAHAM. Van Hollen. I apologize.

STATEMENT OF SENATOR VAN HOLLEN

Senator VAN HOLLEN. Thank you. Thank you, Mr. Chairman. Mr. Wolf, if I could start with you. You would agree that in America, people have the right to peacefully protest under the First Amendment, right?

Hon. WOLF. Correct.

Senator VAN HOLLEN. You would agree that people have the Constitutional right to due process, right?

Hon. WOLF. Correct.

Senator VAN HOLLEN. Would you agree with Kristi Noem, who characterized Renee Good as a domestic terrorist? Is Renee Good a domestic terrorist in your view?

Hon. WOLF. I've been very clear on this point when that incident occurred, as well as, the Pretti incident as well, as to let that investigation play out before any comments are made about the motives of any of the individuals involved, including the law enforcement officers.

Senator VAN HOLLEN. So clearly it was wrong to label them domestic terrorists, right?

Hon. WOLF. Again, I don't—I'm not at the department. I don't know what the information she had. If you're asking me my opinion, I would have wait till the investigation play out before I rendered any decision.

Senator VAN HOLLEN. So you think it's still an open question, as to whether or not they're domestic terrorists?

Hon. WOLF. Again, I'm going to let the investigation play out.

Senator VAN HOLLEN. It sounds like you do, which I think, you know, the country has seen the videos, okay. So I find it outrageous that you would sit here today and hold open the idea that they

were domestic terrorists. Their parents and their loved ones deserve an apology from the U.S. government. And I find it outrageous that you're taking the position that you are here today.

Hon. WOLF. I'm not taking a position.

Senator VAN HOLLEN. Yes, you are. By taking no position and leaving on the table the notion that they're domestic terrorists, you are taking a position.

Mr. Bier, thank you for emphasizing the big lie, that what the Trump administration is doing is going after the worst of the worst. As Senator Merkley pointed out and as you've shown, that's just not true. I mean nobody wants people who are here illegally committing violent crimes, but the overwhelming majority of people who are in detention right now, by ICE's own admission, pose no risk to public safety, is that right?

Mr. BIER. That's right. Over 70 percent were given no threat level at all. They could be released right now without a threat to the public.

Senator VAN HOLLEN. Yeah. So this is just such a big lie. I mean that's what we're seeing playing out in this forum and out of the White House from people like Stephen Miller and the President of the United States.

Could you talk about another big lie? We do know that the so-called Big Beautiful Bill, as Republicans called it, increased the debt massively, like over 4 trillion bucks, right? They gave tax cuts to billionaires permanently. They cut health programs, they cut food safety, food programs.

They had a walloping increase for ICE, like seven times its regular budget. And at the same time, we hear people like Stephen Miller and others claiming that it's actually immigrants who are the cause of increasing our deficits and debt. In fact, they've said that immigration is the cause of the growing national debt, and immigrants take more from the country than they contribute.

Now, Mr. Bier, you've looked at this question very closely. Could you just respond to what I think is the second big lie?

Mr. BIER. Yeah. Over the last 30 years, we looked at all the government budgets, federal, state and local combined.

Immigrants reduced deficits by 14 and a half trillion dollars, which is about a third of all of the total deficits during that period of time. And it's easy to understand why, because they work at 12 percentage points higher than the national average.

They use less benefits because they're subject to constraints, unique barriers to applying for those benefits in particular Social Security and Medicare, where those are by far our largest programs and they're not eligible for those at all if they're here in the country illegally, or if they came legally and they don't have a sufficient work history to qualify.

So you have clear reasons for believing that they are reducing the deficits and debt. They are a benefit to this country, and we need more people who are going to contribute in the future as our population ages.

Senator VAN HOLLEN. Could you amplify a little more on the issue of Medicare and Social Security, because those, you know, trust funds in just a couple years there won't, you know, be enough to pay out full on the dollar for Social Security and Medicare?

Mr. BIER. That's right.

Senator VAN HOLLEN. But as you pointed out, there are many immigrants that are paying into that, but not paying out. So could you just elaborate on that?

Mr. BIER. According to the Social Security Administration, we need about 35 million more workers in order to keep revenues equal to expenses by the middle of the 2030's.

So we are at a position right now where immigration is not going to solve it. Obviously, it's not going to solve it. But it is moving us in the right direction. These are people who are showing up. They're ready to work. They're often prime age individuals who are ready to enter the labor force.

So it's a huge benefit fiscally to the United States to have these people who want to contribute to our country.

Senator VAN HOLLEN. Thank you. And thank you for exposing two of the big lies coming out of the Trump administration.

Mr. BIER. Thank you.

Senator VAN HOLLEN. Thank you.

Chairman GRAHAM. The information we have is that 64 percent of the people encountered by ICE are either convicted or pending criminal charges. So if somebody's wrong, we'll find out who's lying.

STATEMENT OF SENATOR JOHNSON

Senator JOHNSON. Mr. Chairman. Yeah. Let's talk about some realities here. The Ranking Member talked about the funding for ICE rose dramatically this first year of the Trump administration. Well, this is why, because for the previous four years, President Biden and the Democrats opened up the border. Now I like this chart. First of all, saved the President's life.

But the other reason I like this chart is it shows cause and effect. And the catalyst for this explosion in illegal immigration really was deferred action on childhood arrival. That's what sparked this. That created the first incentive for people to come this country illegally. That started the first wave.

Now back in 2014, these were massive numbers. I remember President Obama calling it 2,000 a day. It was, you know, a humanitarian crisis. I think under Biden we had over 14,000 a day average. So Mr. Wolf, you were Acting Secretary trying to clean up the Obama mess.

You know, I remember leading a delegation down to McAllen, Texas during the Obama administration, where we looked at the facility they set up and everybody, Democrat, Republican, were singing the praises of DHS for kind of skirting the rules and establishing a facility that would keep the children safe, as they were being encountered and detained across the border, because of the incentive of Deferred Action for Childhood Arrivals (DACA).

A couple years later in the Trump administration, that exact same facility, all of a sudden now the Democrats are calling that kids in cages. I mean first of all, will you just confirm the fact that that was the exact same facility that we praised in 2014, and all of a sudden 2017, those are kids in cages?

Hon. WOLF. That's correct. Those were Border Patrol facilities.

Senator JOHNSON. I think out of your Department, last year we got a memo that said of the millions, and nobody knows exactly

how many millions came in here, the wide open border. I mean this is—I mean it's so out of whack. 650,000 people that are at large in America have committed crimes. 15,000 murderers, 20,000 rapists. Mr. Bier, you're shaking your head.

Mr. BIER. They're not at large. The murderers are in prison. They're in prison, where they should be.

Senator JOHNSON. They're not detained.

Mr. BIER. They're detained by the state. They're not detained by ICE. They're in prisons ready to be deported. Now I don't——

Senator JOHNSON. Ms. Vaughan, have you kept track of how many people have been murdered, raped, victims of vehicular homicide at the hands of somebody that the Biden administration let in this country?

Ms. VAUGHAN. I wish there were a way to do that. But so many law enforcement agencies do not keep track of the immigration status of people in their custody. They don't want to know.

Senator JOHNSON. Would you say it's in the hundreds?

Ms. VAUGHAN. Yes.

Senator JOHNSON. Thousands?

Ms. VAUGHAN. Yes.

Senator JOHNSON. Tens of thousands?

Ms. VAUGHAN. One data point that I do know is that at a certain point a few years ago, ICE determined that there were 10,000 deportable criminal aliens who were released by Sanctuary policies, who subsequently re-offended within 18 months.

Senator JOHNSON. So where is the outrage on the other side of the aisle to those victims and their families? You know, we had a hearing and in Homeland Security, and they were talking about the trained activists. The number was very large I mean, but I couldn't believe it.

Trained activists that are trained to basically interfere and obstruct with the legal law enforcement actions. And I made the comment, I can't imagine encouraging my supporters, first of all, to get trained to obstruct justice much less than go ahead and do it. Put yourself in harm's way, knowing people are going to potentially get hurt.

Well, people did get hurt. Three people lost their lives, and now the other side exploits those martyrs while they completely ignore the thousands. Let's face it, it's thousands of the Laken Rileys and those individuals, you know.

Where's the outrage to the crimes committed by that massive wave? And of course, I didn't want to have to spend money on ICE. But what are you going to do and trying to clean up this mess? I mean it is difficult enough for you to clean up Obama's mess. Try and clean up Biden's mess, and rather than getting help from the other side, no.

They trained activists to obstruct trying to clean up that mess. That's the reality of the situation. That's why I agree and I appreciate this hearing. We have to end Sanctuary Cities, because it's just another incentive, a massive incentive for more illegal immigration. And the next time Democrats get in power, they'll open it up again. Thank you, Mr. Chairman.

Chairman GRAHAM. Senator Whitehouse.

STATEMENT OF SENATOR WHITEHOUSE

Senator WHITEHOUSE. Thank you, Chairman. Let me first observe for my colleagues here that "Sanctuary City" is a political term. It's not a technical term as your own witness has acknowledged, and it's important to get things right.

So I want to just describe what's happening in Rhode Island. In Rhode Island, our federal district court has decided that local and state police officers can be held liable if they hold a person under an ICE detainer without a proper warrant, that the warrant requirement actually applies. And particularly when you're dealing with someone's freedom, detention. And that decision is not a fluke. It has been upheld by the United States Court of Appeals for the First Circuit.

So in my state, the district of Rhode Island from a federal point of view, and in my circuit the United States Circuit Court of Appeals for the First Circuit, it is binding law that state and local officials who hold somebody under an ICE detainer where ICE hasn't bothered to get a warrant, can be held civilly liable. So we need to face that fact, because that's actually binding federal law.

The second comment I want to make is about the behavior of ICE agents when they operate in Rhode Island. Those of you on the Judiciary Committee will remember me describing the set of affairs to then Secretary Noem. So masked agents appear in the Rhode Island Superior Court courthouse. They're operating in violation of an agreement that they have with the Court not to operate in that courthouse.

They are operating with cameras, in violation of a courthouse rule that forbids taking photographs. They're pursuing a 30 plus year-old individual who they have mixed up with a high school intern who is interning for one of our state court judges. The state court judge is going to take the intern back to high school, so they walk out to the judge's car.

If the ICE agents hadn't bothered to figure out who the judges were in the courthouse, when they got to the car, parked in a judge's only spot, they had the information necessary to deduce that this might be a judge. But rather than accept the judge's representations that he was returning an intern to the high school, I'll take out the full F-bomb because they used the actual full word.

They told the judge "We are going to smash your effing windows and we're going to drag you out of your effing car if you don't turn over this intern to us." And then they grabbed the intern, and they took the him into another car for half an hour and figured out that the high school intern was not the 30 plus year-old person they purported to be looking for.

Why would regular law enforcement, why would those sheriffs in the courthouse want to cooperate with an agency whose legal—whose law enforcement behavior is so wildly irresponsible and unprofessional? It's hard to imagine. And when I asked Secretary Noem about the problem of deconfliction, I think pretty much everybody in law enforcement understands what deconfliction is. Even if the state police aren't cooperating with the Bristol Police, when they run in to do a raid in Bristol, they will let the Bristol police know that they're going to do a raid so that when calls come

in about cars in the street or people with guns or whatever, the local police department know what's going on.

Deconfliction is elementary stuff that is taught to law enforcement, and yet ICE refuses to deconflict and the secretary didn't know the difference between deconfliction and cooperation. So clearly there's a massive hole in the way in which DHS thinks about this stuff, and it is in that environment that I think a lot of sensible decisions are being made by local police chiefs, about whether they want to cooperate with an organization that behaves in unreasonable and unprofessional ways, that threatens judges with violence and that refuses to even give them the courtesy of deconflicting when they're operating in their jurisdiction. Thank you, Chairman.

Chairman GRAHAM. Thank you. Senator Cornyn.

STATEMENT OF SENATOR CORNYN

Senator CORNYN. Well Mr. Chairman, I think President Trump has done it again, exercising almost magical powers, turning Democrats into budget hawks.

It's an amazing thing to behold, And of course they reflexively oppose anything and everything that he proposes, whether it's standing to honor law enforcement and public safety and not illegal immigrants, whether it's eliminating the threat of nuclear weapons and the number one state sponsor of terrorism. They take the side of the state sponsor of terrorism. It's just amazing to me to behold.

Sheriff, I was struck by the Ranking Member's introduction to his comments. He said let the police be the police, and I assume that would extend to other law enforcement officers like you and the sheriffs and other folks who maintain public safety. But is the collective role and job of law enforcement officers like yourself to protect public safety?

Hon. CHAPMAN. It is, and I think we all need to work together. One of the things that was just left out of the testimony just provided is the fact that communication is important to this, and we work very well with our counterparts.

And a lot of times it's because of the chiefs are driven by the politicians above them, that don't allow them to actually work properly and cooperatively with our federal and other local counterparts. When you have that level of communication, you can work these issues out in advance before you have a problem like, you know, what the Senator was just saying.

Senator CORNYN. Well, a lot of it boils down to information-sharing; correct?

Hon. CHAPMAN. That's correct.

Senator CORNYN. And even where in the Senator Whitehouse's Circuit, the judges said that maybe local law enforcement can't keep people beyond their jail sentence without some other additional warrant.

Let me ask you to go out on a limb here a little bit. If the two individuals who tragically died in Minneapolis, who interfered with law enforcement operation by ICE, if there had been the kind of cooperation that you demonstrate in Loudoun County and the sort of cooperation that you've advocated for here, do you think it's like more likely than not that those two individuals will still be alive?

Hon. CHAPMAN. Well, I can't really speak directly to that. What I can tell you is that the cooperation that we've had actually works well, and I do believe that that would have had an impact there. When you're talking about 287(g) and again, Senator Whitehouse's information, we have an agreement with ICE and our people are trained in order to hold onto people for a little bit up to 48 hours after they're incarcerated for a completely separate offense with us, okay.

Senator CORNYN. That's a memorandum of understanding between the feds and the local law enforcement?

Hon. CHAPMAN. That's right. And that way they're already in custody. All we got to do is turn them over. They don't only have to come to the community to do—very often to do any enforcement operations. So it actually works out well.

And to get on the front side of this and to have that level of communication prevents that kind of thing. But if you have politicians that are directing you to act in a particular way it really, you know, really messes up the system here, and I think that's largely the problem.

We all want to work together. We work with everybody. All of our federal counterparts, all of our state and local counterparts, which I believe is one of the reasons we got one of the lowest crime rates in the entire country.

Senator CORNYN. And you have—as a result of that cooperation, you haven't experienced any of the tragic consequences that we saw in Minneapolis?

Hon. CHAPMAN. We've actually had—before that, we actually had people that were—had not been arrested by us that committed crimes. But anybody that's been arrested by us and turned over to ICE is not a problem. They're out of our hands.

Senator CORNYN. And Mr. Wolf, you were of course former Secretary of Homeland Security, and our Democratic colleagues say that they won't fund the Department of Homeland Security and alleviate these long waiting periods that citizens are experiencing at airports all around the country. They won't fund the Federal Emergency Management Agency (FEMA), until federal warrants are required.

Now will you explain how a final order of deportation is a civil matter, and that requiring after that due process has already been exhausted why requiring a criminal warrant would be not only unnecessary, but also destructive of ICE's ability to actually enforce immigration law?

Hon. WOLF. Absolutely. An alien will go before an immigration judge through—towards the end of their pendency of their hearing. They will go before an immigration judge and they will rule on their case.

If they want to appeal that, they'll go to the Immigration Board of Appeals and they could perhaps even appeal that outside of that. And in every case a judge, through the due process, they'll have an attorney with them through the due process, says you don't have a legal right to remain in the United States and it's time for you to self deport. It's time for you to leave.

And we know, as I indicated earlier, about 1.6 million of those individuals have said "No. I'm going to defy that order. I'm going

to stay here in the United States. I don't want to go back to my home country."

And then ICE is coming in at the very end of this process, enforcement removal officers, to effectuate that judge's order, to understand and to remove that individual from the country.

And in some cases, they are in their house, and so they have a final order of removal. They have reasonable suspicion that that individual is in that house. They have likely observed that individual go in and out of that house, and they are there to remove that individual.

It's very different than a judicial warrant, where you're just going into arrest someone who's then going into the due process system, has to go before a judge, have their case heard, all the facts. That is not as what is occurring when you have a final order of removal.

Senator CORNYN. Thank you.

Chairman GRAHAM. Senator Luján.

STATEMENT OF SENATOR LUJÁN

Senator LUJÁN. Thank you, Mr. Chairman. Everyone, thank y'all for being here today. And I just want to begin by reminding everyone this is the first hearing of the Budget Committee in this Congress.

This is the first one, 13 months into this Congress. Oh, you're saying second, sir? I appreciate that, Chairman. Second.

VOICE. What was the first one?

Senator LUJÁN. Look. When we talk about what's happening to constituents across the country right now, you'd think there'd be some hearings about the high cost of everything. They're detainers, 287. I certainly hope we get to that place.

But since we're here today and we're not talking about the high cost of groceries or how expensive it's to get a birthday cake for your kids, gas prices through the roof, housing unaffordable, my Republican colleagues want to have a conversation about the One Big Beautiful Bill that they passed, which by the way we've now learned from the Congressional Budget Office that the legislation increased the federal deficit by $3.4 trillion. Trillion.

That's a lot of money, while failing to provide real relief to middle and low income families. So, look. The proposal's tied to the package and its broader agenda included cuts to programs like food, like SNAP, providing support to millions of working families just to put food on the table.

So as we sit here in the Senate Budget Committee, yes or no, and my question is to Mr. Duke. Does focusing federal attention on punishing so-called Sanctuary Cities meaningfully address the affordability crisis Americans are facing?

Mr. DUKE. No.

Senator LUJÁN. Does it lower health costs for a child with diabetes?

Mr. DUKE. No.

Senator LUJÁN. Does it make the gas that people put at a gas station into their vehicles more affordable to get to work, or does it make it more expensive?

Mr. DUKE. It has nothing to do with gas.

Senator LUJÁN. Does it help a single mom afford to put food on the table?

Mr. DUKE. No.

Senator LUJÁN. Does it address the medical solvency issues to make sure seniors can count on their benefits?

Mr. DUKE. It addresses them by making them worse.

Senator LUJÁN. Does it help a young couple who wants to buy a home and start a family maybe afford that house?

Mr. DUKE. It does not help.

Senator LUJÁN. Well, what the hearing discusses does not include anything that I just asked about, and the constituents I've been talking to at home are saying when's Congress going to get off their rears and do something about the prices that we're hitting every day, as opposed to just protecting the Epstein class and not demanding that these documents and everything else get released?

So Mr. Wolf, you had responsibilities in a few of these places, and I appreciate the responses you were providing to one of my colleagues now about the process that you believe is fair, that allows ICE agents to go into someone's house without a judicial warrant. I just—it's hard for me to continue to listen to justification on how American citizens can have their homes broken into without a judicial warrant. I don't understand this, that American citizens, it's okay for an American citizen to have their windows on their car bashed in.

That's not okay, and with all these conversations about Artificial Intelligence (AI) and some of these companies now that are working with ICE for facial recognition, which I have concerns about, what I don't understand now is how the Department of Homeland Security has access to the best facial recognition, AI.

DHS says they know who the violent criminals are. Their words. They say they know who they are. And if they only had access to facial recognition, they could go find all the violent criminals. Well, why aren't they doing it? It's one unanswered question after another. It's one American right after another that is being torn apart, and there's justification now to say we can pound in your door.

We can go in your house if you're an American citizen, or we can bust out your windows and drag your butt out of a car as an American citizen. I just don't get it, Mr. Chairman, and I hope we have some hearings about getting answers to that, because I think there's agreement between Democrats and Republicans on this issue.

We don't want American citizens being treated this way. None of us. We want to go after the violent criminals. We both agree with this. I certainly hope that we can have a thorough and thoughtful hearing and get some answers to that as well, and that everyone agrees, stop doing that. Mr. President, tell your folks to stop doing that.

I hope now that Senator Mullin, who's going to be Secretary of Health or Homeland Security, maybe he'll get these people to stop all the bad shit and to go after the bad people. Then we'll be in a better place. Thank you for the time, Mr. Chairman.

Chairman GRAHAM. Thank you. Yeah. We'll have a hearing on affordability and Ms. Vaughan, what percentage of a legal immi-

grants in Sanctuary Cities or just in general receive federal benefits?

Ms. VAUGHAN. I don't have numbers for specific Sanctuary jurisdictions.

Chairman GRAHAM. Yeah, just in general.

Ms. VAUGHAN. but nationally, it's more than 60 percent——

Chairman GRAHAM. So more than 60 percent of illegal immigrants receive federal benefits, and it has absolutely no budget impact. Well, that's interesting. Senator Lee.

STATEMENT OF SENATOR LEE

Senator LEE. Thank you Mr. Chairman for holding this hearing. Thanks to each of you for being here. I want to say at the outset a couple of things. First, I'm not familiar with many countries that are asked to simply ignore their own immigration laws, separate and apart from their criminal laws, just their own immigration laws.

Those countries that do exist or may have existed over time don't remain countries very long, because things happen when you don't enforce your own immigration laws. We don't have to travel very far in our own continent or throughout Europe or Asia or Africa, to see what would happen if one of us entered one of those countries, took up residence there unlawfully. It likely wouldn't end well for us.

I also want to observe that there's been several mentions here today about the difference between state authority and federal authority in reference to state's rights. I don't like using the term state rights, not only because it conjures George Wallace's image, also because it's inappropriate.

States don't have rights. Individuals have rights. States have authority. It's the inverse polar opposite of a right. We have parallel authority with parallel sovereign jurisdictions in this country. The U.S. government, the one headquartered in this city where we sit today, has authority over our immigration laws, laws governing naturalization.

Who may enter the country, who may stay, under what terms and conditions or for what period of time. We do need our federal government to be able to undertake those efforts, to perform those efforts well. We've adopted a number of techniques to procure and encourage the cooperation of state and local jurisdictions.

Sometimes those have been ignored, and that is unfortunate. I'll get more to that in a moment. But look, we're exploring something today that is not without cost. The cost of an unsecured border, an unprecedented illegal immigration during the four years of the Biden administration, remain both stunning and debilitating.

The Trump administration, for its part, quite fortunately has done a very effective job at securing the border and beginning the process of deportations on a very large scale. But there's a lot of work left to be done to deport the many millions who remain in this country, for us to be able to restore law and order, people who if they did immigrate to almost any other country on Earth, would be in a whole lot of trouble and would be facing those things.

And yet we're made to feel as if we would be doing the wrong thing by enforcing our immigration laws as such. That important

work that's being done carried out every day by brave men and women in federal law enforcement, continues to be obstructed across the country, as radical leftists, who are proudly, aggressively supporting Sanctuary Cities, are trying to try to undermine the enforcement of federal law.

These Democrat-controlled state and local governments have, in many instances, been working actively, aggressively through these Sanctuary City policies to subvert not just federal law, but the enforcement of federal law by duly sworn and authorized federal law enforcement officials.

And they are trying to do one thing by enforcing the law, which is to prioritize the well-being of American citizens and the safety of the American homeland. This terrible reality that we face is highly offensive to the citizens of the United States, especially those who have done the work to immigrate to this country legally and to do it the right way.

Now believe it or not, not that long ago, Democrats supported and President Clinton himself signed into law the Illegal Immigration Reform and Immigrant Responsibility Act, commonly known as IIRIRA. That Act in expanded border enforcement capabilities and helped bring immigration under control at a time when illegal immigration to the United States was far less frequent, far less of a large problem than it has been in recent years.

In 2016, President Trump ran for President and was elected on a promise to stop illegal immigration. Democrats intending to oppose the President at every turn decided to depart in that moment from their previous, more common sense position, and to go all in on the belief that our nation should effectively have no border at all, that borders are sort of aspirational suggestions that we can look the other way on.

That brings us to the topic of today's hearing, the cost of Sanctuary Cities. So what is that cost? Well, the cost is clear. It is waste, fraud and abuse by illegal aliens who shouldn't be within our borders. We saw this in Minnesota and we've seen it elsewhere. The cost is clear more than anything else.

It's the loss of human life, innocent human life. Innocent American lives at the hands of illegal aliens, who have perpetrated some of the most heinous attacks on innocent American lives in recent memory. And these are entirely preventable, entirely avoidable crimes, crimes that would not have happened but for our open border policies facilitated by Sanctuary Cities.

The cost is clear. It's also the increased additional avoidable tax burden on state, local and federal taxpayers, who are expected to subsidize and in fact are forced to subsidize problematic social welfare programs that illegal aliens have obtained eligibility for through fraudulent means or various loopholes, to say nothing of the education system, which because of a Supreme Court ruling in Plyler v. Doe four and a half decades ago, states are also forced, by no choice of their own, to supply that additional benefit as well.

So, look. The cost is clear. It's all over the place. We can't ignore it, and by continuing to ignore it, we create additional problems. Let's not dress this up as something that it is not and never can be. Thank you.

Chairman GRAHAM. Mr. Padilla.

STATEMENT OF SENATOR PADILLA

Mr. PADILLA. Thank you, Mr. Chair. I want to echo the sentiment that several of my colleagues have mentioned earlier in this hearing, in more than just being surprised that this is well, I guess the second hearing for this Congress, but the first of this calendar year.

It is not about the federal budget, not about an economic outlook. But I understand why the Republican majority wouldn't want to talk about that, not while gas prices are spiking because of an unauthorized war against Iran. Instead, we're here to discuss how off base this whole Sanctuary conversation really is.

Now I know it's an area of passion for the Chairman. We've debated Sanctuary policies on the floor of the Senate. As I've objected to the legislation he has presented that would promote federal overreach, in my opinion. I think part of my frustration comes from the fact that there's a fundamental misunderstanding about what Sanctuary policies are and what Sanctuary policies are not.

Sanctuary policies do not mean that there are no laws in Sanctuary cities or states. They do not keep ICE and other federal agencies from entering a city or a state to arrest violent criminals as they're released from prison. Sanctuary policy simply affirm that immigration enforcement is the job of the federal government, and that the federal government cannot unilaterally decide to co-opt state and local authorities or their resources, precious resources in carrying out these responsibilities.

True, there's some states and local jurisdictions who may choose to coordinate, collaborate, but there's many who don't for their justifiable reasons. It means carrying out immigration enforcement activities is ICE's purview, while respecting state and local law enforcement agencies to make their own decisions about how to focus and direct their limited resources that they deem best protects and serves their constituents.

It means prohibiting federal overreach through policies that have been upheld by the courts. And so I'm glad several of my Republican colleagues are here to listen me to me quote Justice Scalia in the case Prince v. United States.

"The federal government may neither issue directives, require the states to address particular problems, nor command the states" officers or those of their political subdivisions to administer or enforce a federal regulatory program."

Look, folks. We can have disagreements on policy, but the fundamental misunderstanding about what Sanctuary policies are has led to the promotion of enforcement actions across the country that have become damaging to our communities and to our local economies.

So it seems to me that this debate has not really been about public safety, because if it were, we could also be talking about how the data shows that Sanctuary policies actually make communities safer, healthier and more prosperous.

That's right. The evidence shows, the research shows Sanctuary jurisdictions have lower crime rates, higher meeting household income, less poverty, less reliance on public assistance, higher labor force participation and lower unemployment.

That's right. Seems like Sanctuary cities are helping to make America great. I said it. So let's stop perpetuating the lies. Let's ensure that ICE and United States Customs and Border Protection (CBP) agents are accountable and conduct their jobs in accordance with the law and the Constitution. Let's allow local law enforcement leaders to do what they deem is best for public safety.

And Mr. Chairman, just so you know that I'm not exaggerating in my claims, I'd like to submit for the record articles and reports from researchers at the University of Texas-Austin, from the Center for American Progress, from the University of Arizona, University of North Carolina and the American Immigration Council.[9]

Chairman GRAHAM. I have no objection.

Senator PADILLA. Thank you. Thank you, Mr. Chairman.

Chairman GRAHAM. Senator Kennedy.

STATEMENT OF SENATOR KENNEDY

Senator KENNEDY. Secretary Wolf, thank you for your service. Why in your opinion do so many, not all, but so many of my Democratic colleagues believe in open borders?

Hon. WOLF. Well, that's a good question. I think it's a couple of different fronts. One, it would be they don't believe in immigration law. They don't like enforcement, they don't like removing of individuals.

Senator KENNEDY. What else?

Hon. WOLF. And they believe that—there are some that believe that anyone and everyone should be able to come to the U.S., whether they apply for protections under what else U.S. law or not.

Senator KENNEDY. What else?

Hon. WOLF. And then there are some probably that would like to see more voters in the U.S. as well.

Senator KENNEDY. Ms. Vaughan, some of my colleagues for whom I have great respect, believe that the 10th Amendments, the 10th Amendment allows local governments to ignore federal immigration law and implement Sanctuary policy.

The Supreme Court has ruled that racial quotas represent a pernicious form of racial discrimination. Can local governments ignore that ruling under the 10th Amendment?

Ms. VAUGHAN. Well, the 10th Amendment is about——

Senator KENNEDY. And can they—answer my question, please.

Ms. VAUGHAN. The 10th Amendment is about commandeering, and so it's not really appropriate to this because——

Senator KENNEDY. Is the answer yes or no, ma'am?

Ms. VAUGHAN. Could you ask me again because I don't understand the relevance to—I'm sorry, because ICE is not asking them——

Senator KENNEDY. Secretary Wolf, you heard my question. Under the guise of the 10th Amendment, can local governments ignore Supreme Court ruling?

Hon. WOLF. I don't believe they can.

Senator KENNEDY. Okay. Women have the right to vote in America by statute and by Constitutional interpretation. Under the

[9] Documents submitted by Senator Padilla appears in the appendix on page 136.

guise of the 10th Amendment, can local governments just ignore that and prohibit women from voting?

Hon. WOLF. No.

Senator KENNEDY. Mr. Duke, you worked for President Biden and Vice President Harris, didn't you?

Mr. DUKE. Yes.

Senator KENNEDY. And I listened carefully to your testimony today. You expressed—well, you offered the opinion that the One Big Beautiful Bill is terrible in part because it will cause deficits. Is that, is that right?

Mr. DUKE. Yes.

Senator KENNEDY. Isn't it a fact that the profligacy of President Biden caused nine percent inflation in America?

Mr. DUKE. No.

Senator KENNEDY. Okay. Isn't it a fact that if President Biden had discovered life on Mars when he was president, he would have immediately sent it money?

Mr. DUKE. I don't believe so.

Senator KENNEDY. Oh, you don't? Okay. Well, I appreciate your newly found fiscal conservatism.

Mr. Bier, you seem to be a very smart guy and you seem to be a nice enough guy.

Mr. BIER. Thank you, Senator. You seem smart as well.

Chairman GRAHAM. I object. [Laughter]

Senator KENNEDY. Don't, don't you——

Mr. BIER. That was the Chairman, not me.

Senator KENNEDY. Don't you think your hyperbolic statements undermine your credibility?

Mr. BIER. What hyperbolic statement did I make?

Senator KENNEDY. On February the 11th, 2026, you posted on Bluesky, quote—my words or your words, not mine. "The Democrats" video telling service members to ignore illegal orders didn't go far enough. They should have also urged them to refuse unethical orders, whether illegal or not. There are many things deemed legal that are still obviously unethical and everyone should hold themselves to this higher law." Did I read that correctly?

Mr. BIER. I believe you did. Do you disagree with it? Do you think people should do unethical things in their capacity as——

Senator KENNEDY. On December the 31st of 2025, you posted to in front of——

Mr. BIER. But wait, wait. Do you disagree with——

Senator KENNEDY. But wait, let me finish my question, sir. In front of God and country. You said "They," referring to Republicans, "think they can troll their way into us accepting ethnic cleansing." Your words, not mine. Did I read that correctly?

Mr. BIER. That was in regard to a Department of Homeland Security——

Senator KENNEDY. Did I read that correctly?

Mr. BIER [continuing]. Post about advocating 100 million deportations. That is what DHS has tweeted from their own account.——

Senator KENNEDY. Truth hurts, doesn't it?

Mr. BIER [continuing]. 100 million deportations would be ethnic cleansing. You would be removing——

Senator KENNEDY. Truth hurts, doesn't it?

Mr. BIER [continuing]. One-third of the country. So yes, there are people within the Department of Homeland Security——

Senator KENNEDY. Truth can just gut you like a fish. And you don't think this is hyperbolic? Give me 30 more seconds.

Mr. BIER. I think advocating 100 million deportations is ethnic cleansing.

Senator KENNEDY. I'm having fun, Mr. Chairman. On February 5th, you said the following on Bluesky.

Mr. BIER. Your time is up.

Senator KENNEDY. "These are all good. But there's no enforcement mechanism." I'm sorry. Let me try a better one.

On March 2nd, you said "If you rule against Trump's population purge agenda," no hyperbole there, "The nativists will name you, threaten you and come after you. These judges are much braver than the ICE agents who hide behind masks while violating the Constitution."

Mr. BIER. They are much braver. They put their names on their rulings, and they stand behind their Constitutional rulings. When I talk about population purge, I'm talking about the fact that they're trying to deport U.S.-born citizens, people born here. They are trying to deport them as well.

So it's not a mass deportation agenda. It is also an agenda intended to reduce the population of the United States, including U.S.-born people——

Chairman GRAHAM. Thank, thank you.

Mr. BIER [continuing]. So these are not hyperbolic statements. I appreciate you reading my account. I appreciate your concerns.

Senator KENNEDY. What planet did you just——

Chairman GRAHAM. Wait, wait, wait.

Senator KENNEDY. Wait a minute, Mr. Chairman. What planet did you just parachute from?

Chairman GRAHAM. No, no, no. Hey guys.

Senator KENNEDY. You trigger my gag reflex, Mr. Bier.

Chairman GRAHAM. So we got a process here, and it's good to know you don't have an agenda. Senator Kaine.

STATEMENT OF SENATOR KAINE

Senator KAINE. Mr. Chair, thanks for doing this, and Sheriff, welcome. I've got a Virginian at the table and that's why I wanted to come. My sheriff in his testimony said "It is important to distinguish criminal aliens who pose a threat to our community from immigrants here legally."

All of us agree with that. We should try to find that balance. I don't think we're balanced right now. I've been here 13 years. I wish we'd have a hearing about the positive impact immigrants make on the United States. We've never had that hearing in this Committee or the Health, Education, Labor and Pension (HELP) Committee or any others. When we have hearings, it's about immigrants doing bad things rather than about——

Chairman GRAHAM. When y'all were in charge, why didn't you have them?

Senator KAINE. Well, can I take my time? Can I take my time? Because I think what happens, I think what happens——

Chairman GRAHAM. You get a whole minute more.

Senator KAINE [continuing]. When the only hearings about immigrants are about crime, and we have a President who calls people's countries shithole countries. I got a lot of immigrants in the United States and in Virginia who feel like they're being targeted.

And let's talk about positives. I'm going to use Loudoun as an example. The sheriff is rightly proud of Loudoun County. 420,000 people in the last census. It's probably coming up to 450 now. It is not only one of the safest jurisdictions in the United States, it's also by recent count the wealthiest county in the United States.

And guess what? 25 percent of the population and climbing is immigrant. 25 percent. Safe, economically successful immigrant. And I also applaud this Sheriff's Department. If you have something to do or if you don't have something to do on Halloween sometime, go to the Halloween parade in Leesburg and you'll see the Sheriff's deputies helping order and they all wear uniforms with their names on them. None of them wear masks.

The county's been an innovator in body camera, in body camera use. Wow. What a novel concept. I like the professionalism that I see with your Sheriff's Department, and I think we could use it at the federal level.

Here's an issue about Sanctuary Cities in the Trump administration. DHS—Mr. Chair, you might like this one. DHS in May put out a list of Sanctuary cities and they included 34 Virginia communities, cities and counties on that list. It provoked laughter in Virginia.

There were cities that were not cities but counties. There were counties that were not counties, but cities. There were jurisdictions that have virtually no immigrants whatsoever listed as Sanctuary communities. Here was my favorite, the city of Duffield, Virginia.

Now, if you don't know where Duffield is, you're not alone. Scott County, deep in Appalachia, 0.6 square miles, 76 people, not a single immigrant, not even a police department. Sanctuary City. Virginia laughed the DHS out of the office. And so in October, this was in August. I'm sorry, this was in May.

In August, United States Department of Justice (DOJ) came back and did a new list, number of Sanctuary Cities in Virginia, zero. They redid the list in October. Number of Sanctuary Cities in Virginia, zero. Mr. Chair, I'd like to introduce DHS's bogus Sanctuary Cities list into the record.[10]

Chairman GRAHAM. Without objection.

Senator KAINE. And then Mr. Chair, my last question is, look. I'm not on Judiciary, and the bill that you have about imposing liability on local officials would suggest that the only fault in this area lies on local officials.

Now if you have a markup, it'll be in Judiciary. And I'm not on it, but my question is sort of for you. Isn't what's good for the goose, good for the gander? What about all the times when I was governor of Virginia, 2006–2010, and my DOC, Department of Corrections or local law enforcement, told ICE and the federal immigration agencies, three years under President Bush, one year under President Obama that "Hey, we've got somebody here who is not lawfully here. Please come pick them up, and ICE didn't show up."

[10] Document submitted by Senator Kaine appears in the appendix on page 132.

And ICE still is not showing up. So often the bad examples, including one of the ones in Fairfax recently, repeated instances of ICE having this person in their purview, in their grasp and then letting them go. So wouldn't it be—I mean if you're going to do a bill and and blame cities for this, and try to impose liability on their local taxpayers?

What about letting people sue ICE, when hard-working local officials and sheriffs let folks know "Hey, we got folks here that, you know, are here unlawfully. Come pick them up." And when they don't come pick them up, shouldn't they be liable for that?

Chairman GRAHAM. Maybe that's something we could do together. Sounds like a pretty good idea to me. Be glad to work with you.

I think those days were probably over with this administration. But the idea that the federal government had notice and the local community is willing to turn them over and they turned a blind eye and somebody got hurt, I would be willing definitely to create liability there.

Senator KAINE. And I'll just conclude Mr. Chair and say, you know, the mayor in me, the governor in me gets mad when things like this get blamed on mayors and governors, when often it's complete dysfunction at the federal level under multiple administrations. That's the real culprit. I yield back.

Chairman GRAHAM. Right. The lawyer in me wants to hold people accountable who create problems. Senator Scott.

STATEMENT OF SENATOR SCOTT

Senator SCOTT. Thank you, Chairman. Thanks for holding this important hearing. The United States is a nation of law and order. Those two things go hand in hand because when you have laws that are obeyed, you have order in your communities. But for too long, liberal states and cities have decided to simply ignore the federal laws they don't agree with.

When I was the governor of Florida, I didn't have to worry about our sheriffs not enforcing law but President Trump and our federal law enforcement have to worry about that every day. The American people voted very clearly to secure the border. That's what the election was about, and stop illegal immigration we're a country of legal immigration, not illegal immigration.

And without a single new law being enacted, President Trump has delivered and secured the border when President Biden said he couldn't do it. President Trump and the heroic men and women of our ICE and border patrol are enforcing our laws and they're securing our nation and the crime rate is down.

But some Americans say no, we won't follow federal law. We want to be a Sanctuary for people who broke the law to enter our country illegally. It is a violation. It's against the law to come into our country illegally, like most countries. It's dangerous. It's costly to the American taxpayer and has horrific consequences. That's why I and my colleague on this Committee, Senator Moreno, have introduced two bills to curb this kind of dangerous disregard for the law.

Our unifying American Security Interest Act and Sanctuary Jurisdiction Event Security Enhancement Act, which stopped DHS

funding grants and financial assistance from going to cities and jurisdictions that refuse to support our ICE and DHS agents in their efforts to keep our streets safe.

Americans clearly don't want their tax dollars used for handouts to illegal aliens for health care or food stamps while we have American citizens here at home who are going without. And that's exact kind of waste and financial mismanagement that these bills will stop.

But that's just the financial cost. Let's talk about the human costs. Laken Riley, a nursing student, 22 years old in Georgia was beaten with a rock and strangled by a Venezuelan illegal alien, who had been caught at the border and released under Biden's Open Borders rules.

Rachel Morin, 37 of Maryland, a mother of five who was raped and murdered while hiking by a Salvadorian criminal alien who crossed our border illegally, not once, not twice, but three times under Biden. Kate Steinle, 32, died in her father's arms on a San Francisco pier after being shot by a five-time Mexican deportee, who was allowed to roam freely because the city's Sanctuary policy led local officials to ignore an ICE detainer.

These are just a few of the many lives cut short in the potential unrealized lives because of Biden's Open Borders, that allowed dangerous people to come into this country, and the Sanctuary Cities that chose to support and encourage these criminals.

I'm from Florida. We are an immigration state, legal immigration. We don't support illegal immigration.

Sheriff Chapman, my question. How can open borders and lax immigration policies like a Sanctuary Cities be considered humane by Democrats if they fail to protect the very citizens, Democrats and other elected officials are meant to serve, and they're under oath of office to serve.

Hon. CHAPMAN. Well, I agree with that. I mean the most important thing is for us to keep safety, keep our people safe and we've got to do everything in our power, and that includes working with everybody. I make that point all the time to my constituents, saying "Look. We want to work with all of our federal counterparts. We all have different missions here, different cogs in the wheel. But we all have to work together to keep everybody safe."

Senator SCOTT. Is it better for you if you turn somebody, a criminal over to ICE, rather than let them back into the community?

Hon. CHAPMAN. Absolutely, absolutely.

Senator SCOTT. Mr. Wolf, in Sanctuary Cities, local authorities can decline ICE detainers, which means that criminals are released back in the community rather than transferred safely into ICE custody.

This creates opportunities for illegal fugitives to relocate or reoffend, like in the tragic murder of Kate Steinle. It also forces DHS officers to go into neighborhoods to root out violent criminals. Not only does this cost the taxpayer more; it also increases the risk for everyone involved, including bystanders, DHS officers and potential future victims.

How do these policies prioritize offenders over American victims by obstructing deportations?

Hon. WOLF. Well, when ICE is required to go into a community, out on the streets, right, instead of going into that jail setting and picking up that criminal illegal alien; it'll take one or two officers to go into a jail setting.

If they have to go into their neighborhood, it's going to take 15 to 20 officers to apprehend that same individual. You're putting not only the officers at risk, you're putting that alien at risk and you're putting others in that community at risk.

So the idea here, and it's an ironic kind of play here, is that individuals and politicians that want less ICE in their jurisdictions, they will give them Sanctuary policies to say "We want ICE out." But in fact, it just requires them to be more in their communities because they won't go into a jail setting.

They have to go into their neighborhoods. They have to continue to enforce immigration law. So if your idea is I want to see less ICE in my community, I don't like immigration enforcement, let them into your jail. Let them pick up those criminal illegal aliens. Otherwise, they're going to be out on your streets because they have to continue to enforce immigration law.

And of course, it's better with local law enforcement support and cooperation. But again, in many jurisdictions, Sanctuary jurisdictions, we don't see that either.

Senator SCOTT. Thank you, Chairman.

Chairman GRAHAM. Senator Moreno.

STATEMENT OF SENATOR MORENO

Senator MORENO. Thank you, Mr. Chairman. A quick question for all of you. You can just do a very simple if it's okay, yes or no. I'm going to try to ask it as a simple yes or no question. Are you familiar with U.S.C. 1325?

Hon. WOLF. Yes.

Hon. CHAPMAN. No.

Ms. VAUGHAN. Yes.

Mr. BIER. Yes.

Mr. DUKE. No.

Senator MORENO. You're not. Okay. So the law passed the 1952 is the Immigration and Naturalization Act (INA). It talked about how the country controls immigration.

Mr. Wolf, we'll start with you. Do you think that we made a mistake passing that law, that prohibits people from entering this country illegally?

Hon. WOLF. No, I don't think that's a mistake.

Senator MORENO. Sheriff, do you believe we made a mistake passing that law?

Hon. CHAPMAN. No, I don't think so.

Senator MORENO. Ms. Vaughan?

Ms. VAUGHAN. No.

Senator MORENO. Mr. Bier?

Mr. BIER. It was a mistake to not let them come legally. That's what we need.

Senator MORENO. But that's not what the law said.

The law said you can't enter the country illegally. Was that a——

Mr. BIER. There were definitely mistakes in the INA, many mistakes.

Senator MORENO. Was that part—don't be cute. Was that part of the law a mistake, yes or no?

Mr. BIER. The part of the law that says you can't come legally? Yes, that was a mistake.

Senator MORENO. That was a mistake, that you can't come illegally?

Mr. BIER. No, legally. Legally is what I said.

Senator MORENO. Okay. So you haven't answered to my question. But that's okay. You're a smug guy and that's part of your shtick. How about you?

Mr. DUKE. I don't know anything about this law.

Senator MORENO. Do you think it should be a crime to enter the country illegally?

Mr. DUKE. I don't know anything about this. I'm here to talk about budgets.

Senator MORENO. Well, no, no. You don't have to—look, I'm not a lawyer. You don't have to be a lawyer. It's a simple question. Should it be a crime? I'll say it slowly. Should it be a crime to enter the United States of America illegally without permission?

Mr. DUKE. I don't know anything about that.

Senator MORENO. Do you have a home?

Mr. DUKE. I rent.

Senator MORENO. Do you have a place that you sleep in?

Mr. DUKE. Yes, correct. Okay. Should it be a crime for people to enter your home without permission?

Mr. DUKE. Yes.

Senator MORENO. You're pretty certain on that one?

Mr. DUKE. Yes.

Senator MORENO. Okay. But you're not sure if it's okay to enter America without permission. Which is our home, by the way.

You don't know about America? Okay. Gotcha. All right. So this is the best the Democrats can come up with Mr. Chairman. I hope the Ranking Member can listen for just a second, if you don't mind. This is the best witnesses you've got?

A guy who can't distinguish whether it's okay to have people enter our country illegally. Of all the millions of people that you could have chosen to testify——

Senator MERKLEY. To my good colleague——

Senator MORENO. Oh, let me finish. The best you have is a guy who has no idea what our immigration law is and isn't sure if somebody should enter the country illegally.

Another guy is a smug guy who obviously has an agenda.

And in fact, let me just ask you a question, because I was——

Senator MERKLEY. Maybe you can maybe stick to the facts, instead of badgering the witnesses.

Senator MORENO. I'm not badgering the witnesses.

Senator MERKLEY. Well, you're attacking them.

Senator MORENO. I'm asking simple questions.

Senator MERKLEY. Yeah, why don't you ask some actual factual questions?

Senator MORENO. But why don't you do this? Why don't you speak when it's your turn? Oh, because we actually take turns.

Senator MERKLEY. You're addressing me, so I am addressing you back.

(Simultaneous discussion.)

Senator MERKLEY. You're looking right at me. You're addressing me.

Senator MORENO. Because here's what I find out ironic, is that you follow rules here about speaking order. People can't speak out of turn, but it's okay with you that people enter our country and skip their turn. See——

Senator MERKLEY. Oh, now you're attributing views to me I haven't expressed. Isn't that interesting?

Senator MORENO. What I would love to find interesting——

Senator MERKLEY. Not only are you attacking the witnesses, now you're attacking your colleagues here on the dais, which I think is very inappropriate.

Chairman GRAHAM. Gentlemen, gentlemen. Wait a minute. I'm in charge.

Senator MORENO. Yes, sir.

Chairman GRAHAM. You ask him a question.

Senator MORENO. Yes.

Chairman GRAHAM. He'll get a chance to answer that question. Finish your time.

Senator MORENO. Thank you. All right, moving on to the next question, Mr. Sheriff. How important is it for local law enforcement to cooperate with federal officials?

Hon. CHAPMAN. I can tell you it's worked for us, and it's kept our crime rate down extraordinarily low.

Senator MORENO. So if somebody is in your position in Hamilton County, which is the county where Cincinnati is with the Cincinnati Chief of Police, would you advise them that you think it's helpful to cooperate with law enforcement?

Hon. CHAPMAN. Absolutely.

Senator MORENO. Okay. Do you find it interesting that politicians who have no idea, no background in law enforcement, will pass laws that prohibit law enforcement to cooperate with federal officials, local law enforcement? Do you think that's a good idea?

Hon. CHAPMAN. No, it's not a good idea.

Senator MORENO. It's a terrible idea. Do you have death threats against your sheriff deputies on a regular basis?

Hon. CHAPMAN. We get threats. Not that often, but we do.

Senator MORENO. Constant, constant death threats or do you get constant?

Hon. CHAPMAN. Not constant death threats, but we do get threats, yes.

Senator MORENO. Do you have drug cartels that have bounties out for your deputies?

Hon. CHAPMAN. No. No, we don't.

Senator MORENO. Do you get your agent's doxed, where they go to your agent's—your deputies' homes and threaten their children and their wives and their kids?

Hon. CHAPMAN. No, we have not.

Senator MORENO. Do you get politicians that call your deputies Gestapo and Nazis?

Hon. CHAPMAN. We've had certainly politicians, not locally here, but certainly we've had a lot of people in the community that have said that.

Senator MORENO. Do you find any of that helpful when talking about law enforcement agencies? Because see, here's the irony. What we're disparaging is the people whose job it is to enforce the laws.

And I find it extraordinarily interesting that the people in charge of making these laws are disparaging the people who are enforcing those laws.

And I wonder and I'll ask for the record if the minority witnesses could just sign an affidavit that they have not received any money, and the organization that they work for or participate in from any organizations tied to the Mexican drug cartels, which would have an absolute incentive to have illegal migration at the levels that we saw under Biden?

Chairman GRAHAM. If you have any reason——

Mr. BIER. We haven't accepted any drug cartel money.

Senator MORENO. Again, if they could just put that in affidavit or an affiliated organization.

Chairman GRAHAM. You okay, Mr. Duke?

Mr. DUKE. I'd like to look at the affidavit.

Chairman GRAHAM. Okay, fair enough. But do you know if you've accepted any money?

Mr. DUKE. I do not know of anything like that.

Chairman GRAHAM. Right. Yeah, sure. Okay. Thank you. All right. So good hearing. Senator Merkley, I'll let you go first. I'll have a few words, then we'll go vote.

Senator MERKLEY. Well, let's start with the Gang of Eight that my colleague referred to from 2013. Democrats and Republicans came together, four and four. They crafted a bill on immigration.

It was all about the rule of law on the border. Democrats and Republicans together saying the rule of law matters, and it addressed many of the immigration challenges that we have in different sectors, be it employment, agriculture, a path to citizenship for people who'd been here a long time, a path to citizenship for Dreamers, Democrats and Republicans together.

So one of the myths we've heard today from some of my colleagues is that Democrats don't support the rule of law. Absolutely wrong. What happened to that 2013 bill that went over to the House of Representatives controlled by Republicans, they would not even hold a hearing on it.

They wanted to keep an open sore of immigration as a political issue. In 2024, Democrats and Republican Senate came together again, crafted a deal and it was President Trump as a candidate who said "Don't do it. I want to keep immigration as an election issue."

So I just think it would be more productive if we quit casting aspersions across the aisle, and go back to the philosophy of 2013 and work together to actually get a bill done, or the philosophy of 2024. And certainly all of us agree in going after violent criminals.

That's the second myth that was presented today that people don't support going after violent criminals. Absolutely a myth, and it's just irritating to continuously hear that.

Myth number three from one of my colleagues was that Democrats won't fund FEMA. In fact, last week in the Senate we asked unanimous consent to fund FEMA and the Coast Guard and Trans-

portation Security Administration (TSA), and it was the Republicans that objected. So let's clear up that piece.

The next myth, that is that undocumented individuals receive benefits. It is illegal under the law for undocumented individuals to receive Medicaid or Medicare or Children's Health Insurance Program (CHIP) or SNAP or Social Security benefits.

Another myth, that states that have decided that local police are more effective being local police, somehow attract immigrants because they're not held accountable to the law. They are held accountable to the law. In fact, you have lower crime rates in those states. So that's kind of interesting.

And in fact, we see that some states like Texas, they have 50 percent more undocumented immigrants as a ratio of population than Oregon does. So clearly it's not Oregon's policies that say we're more effective and safer and more prosperous when ICE does ICE and local police do local police work.

And yet another myth. One of my colleagues across the aisle said "States do not have powers." He says they do not have rights. But they do have powers, the 10th Amendment. The powers not delegated to the U.S. by the Constitution are reserved to the states and to the people.

And that is why the federal government cannot commandeer individuals in the states to do federal responsibilities. It's called the Constitution of the United States of America.

Another myth was that it's less safe in states where local police do local police work. There's many statistics that show the opposite of that. But let's just take for example, where is the highest murder rate in America? Is it in a state where local police are assigned to local police?

No. It's Mississippi. Who's the second highest murder rate? Louisiana. How is the murder rate in Oregon? It is one-fourth of what is in those states. So let's be clear again. This myth that you have higher crime rates in places where the local police gain the trust of the people, work carefully between state and local and with federal officials, but they don't do the work of ICE.

And if ICE knows that someone is in a jail, and they know because it's in the federal database and the FBI database and they want that person, it only takes a couple days to go get a judicial warrant and pick that person up. Every time there's a judicial warrant, that person is held.

So if it's just being lazy or sloppy or whatever else, there's a whole lot of misinformation coming forward here that needed to be corrected.

Chairman GRAHAM. Well, thank you very much. I have a different view of things. I really appreciate the hearing. Obama deported three million people. I don't think all of them were criminals. They didn't use judicial warrants. What you're trying to do is make it impossible to deport people.

If you have to go to a judge and get a judicial warrant to deport somebody, you effectively destroy the ability to deport. You're denying Trump the same system you are giving Obama. This hearing to me is stunning. I'm so glad we had it. You're all in on Sanctuary City policy.

If you're looking to America, if you're looking for anybody to change this system, our Democratic friends have no interest. They think it's good for America. The more illegal immigrants, the better. I don't. I'd like to have a rational immigration system.

Gang of Eight. I was involved in it. What we tried to do is stop future flow and the trade-off was to give people a pathway to citizenship, knowing you would stop future flow. You're right. The House blocked the bill during 20 to 2020—from 2020 to 2024, Joe Biden destroyed every concept of rational immigration.

He made it impossible for a Republican to sit down and talk with a Democrat, because during his four years, he took every rule and ignored it. There are no taxpayer dollars associated with legal immigrants. They were flown into the country on airplanes. They were bussed in, they were fed, they were given housing.

11 million people are not living in the streets. They're in some apartment or a house somewhere, and if you don't think that affects housing, that's crazy. 11 million people came at least in a four-year period, and to hear you say it, no impact on this country, that's just absurd.

So what are we going to do? We're going to try to start with the magnets. If you don't think Sanctuary City policy is a magnet for future illegal immigration, that's just insane. How can you fix it if you don't take the magnets off the table?

The bottom line is I want a vote on my bill to hold local and state officials accountable for violating federal law, when they should enforce it like they did during Obama, and you won't let me have a vote. You won't let me have the debate. The reason you don't want to vote is because 70 to 80 percent of the American public think the policies you're pushing are harmful.

There is a budget impact to being overrun in a country in a four-year period. To think this is not a problem is insulting to the American people. It is a problem, and you try to rationalize it and make it a good thing. It's not a good thing. It's a bad thing. At the end of the day, if you can't answer the question "It should be a crime to come into the country illegally," that says all you really need to know about the people here.

Of course it should be. It should be a crime as Senator Moreno said, to go in your house without being invited. We cannot have a sovereign nation when people just show up and come in and go anywhere they want to go. That's just not rational.

We've got more people on the terrorist watch list than we've ever had, 2,700 percent. The people who have come here are all not well-meaning. I understand immigration pretty well. I understand the need to fix it. The American people want to be fair.

But Senator Merkley, they're not going to buy into a system that constantly allows people to come in unchecked. Once you can prove to them you've stopped illegal immigration, then we could have a rational discussion about what to do with people who have been here a long time.

But you've doubled or tripled the number of people. So all I'm going—all I will say, this is a debate worthy of a great nation. Our Democratic friends are accepting of a Sanctuary policy. They don't think it's a problem. I do. Let's have a debate. Let's have a vote.

This will be good for the country going into 2026, as to who should be in charge of controlling our borders and enforcing law. This administration is enforcing the law.The prior administration obliterated the law. Thank you very much to all of you for coming. I appreciate your time and attention. Senator Merkley, thank you. The hearing's adjourned and oh, one second. I want to introduce a statement. Yeah, you didn't finish up.

Donald Rosenberg was unable to be here. I'd like to put his statement in the record. He represents Victims of Illegal Alien Crime. Without objection I'd like to introduce this statement if that's okay. Is that okay? [11]

Senator MERKLEY. Yes.

Chairman GRAHAM. Thank you. I want to thank our witnesses for appearing before the Committee. You want to finish your thought? Go ahead.

Senator MERKLEY. Mr. Bier, you have a pile of documents that you want to enter into the record. What are those documents?

Chairman GRAHAM. Without objection.

Mr. BIER. These are all the studies that show that immigrants do not increase crime, and places that place restrictions on ICE cooperation are not resulting in an increase in crime.[12]

Chairman GRAHAM. Without objection.

Senator MERKLEY. Thank you.

Chairman GRAHAM. Okay. The hearing record will remain open until noon tomorrow for the submission of questions to the record to the Committee Clerk. The hearing is adjourned. Thank you all very, very much.

(Whereupon, at 12:24 p.m., Tuesday, March 10, 2026, the hearing was adjourned.)

[11] Statement submitted by Chairman Graham appears in the appendix on page 107.
[12] Documents submitted by Mr. Bier appears in the appendix on page 181.

Opening Statement of Ranking Member Merkley
Senate Budget Committee Hearing: "Sanctuary Cities: The Cost of Undermining Law and Order"
March 10, 2026

Sen. Merkley's remarks, as prepared for delivery follow:

Thank you, Mr. Chairman.

A lot has happened over the last year.

The Big, Ugly Betrayal Law slashed nutrition assistance for hungry children, gutted health care for millions of families, and increased the enormous wealth of billionaires while massively increasing our national deficit and debt and massively increasing the budget for ICE.

Now, Trump has turned ICE into his secret police. I never thought I'd see federal agents wearing masks without personal ID, driving unmarked vehicles without agency ID, breaking down doors without a judicial warrant, smashing windows and dragging people out of their cars, or detaining people without access to attorneys or family.

Heartbreakingly, three American citizens have been killed by ICE. Renee Good and Alex Pretti, in Minnesota, and Ruben Ray Martinez, in Texas.

Sadly, we are just learning what happened to Mr. Martinez due to recent reporting by CBS News that obtained body cam footage showing him being killed by an ICE officer in March 2025.

Multiplying ICE's budget by a factor of seven and the agency's alarming change in behavior would be a very good topic for a hearing.

Another good topic for a hearing would be the budgetary costs of the new war with Iran. Wars always come with enormous costs. The costs of previous Middle East wars in Afghanistan and Iraq were staggering in terms of blood and treasure:

- More than 7,000 American servicemembers killed.
- More than 50,000 servicemembers were wounded.
- More than $6 trillion of treasure spent.

In Iran, seven U.S. servicemembers already have been killed and the Pentagon's preliminary cost estimate was $1 billion a day, and Republican lawmakers are now saying $2 billion a day.

This committee should be focused on putting money back in the pockets of American families, not waging another war in the Middle East.

I think it would be great for the Budget Committee to hold hearings on all of these significant items which involve vast changes in expenditure and vast changes in revenues, all very relevant to this committee.

But the topic of today's hearing has very little relevance to our budget expenditures.

The word "sanctuary" is a misnomer. Sanctuary policies do not make places a limitless safe haven where immigrants cannot be detained or deported.

These policies simply refer to states, cities, and local leaders deciding that local police should handle local police work.

These policies and state autonomy are rooted in the Tenth Amendment to the Constitution.

My Republican colleagues usually lean on states' rights. They know that, for decades, the courts have said the federal government can't commandeer local officials to be federal agents.

So, the idea of local police doing local police work is protected by our Constitution.

And there's a public safety rationale behind it.

We know that when local police serve as local police, they build relationships with the community and decrease crime.

Research shows that sanctuary jurisdictions have higher median household income, less poverty, less reliance on public assistance, and lower unemployment.

And they have lower crime rates. A 2022 study found sanctuary practices lead to a decline in crime rates in those communities.

But, when local police must double as federal immigration officers, local police get less information, trust in the community drops, and crime goes up.

And let's be clear: nothing about local police being local police hampers federal immigration enforcement – federal agents just need to get a judicial warrant.

And a reminder -- each of these issues – slashing nutrition assistance for hungry children, gutting health care for families, increasing the wealth of billionaires, exploding the deficit and debt, transforming ICE into Trump's secret police launching a new war in Iran are issues that the Budget Committee should hold hearings on.

Sanctuary cities are not new and do not have significant budget implications.

So, with all of the challenges we face, it's puzzling why this is today's topic.

But, as long as we're here, let's understand that states that choose to let local police be local police do pretty damn well.

With that in mind, I have invited two expert witnesses to join us today.

Mr. David Bier, from the Cato Institute, is an expert on sanctuary policies and the broader concerns about the Department of Homeland Security receiving $170 billion for immigration enforcement with little accountability or oversight.

Joining him is Mr. Brendan Duke, from the Center on Budget and Policy Priorities, who focuses on critical budgetary concerns, including how the Big, Ugly Betrayal Law has exploded our debt and raised costs for working families.

Thank you both for joining us today.

Thank you, Mr. Chairman.

###

The Honorable Chad F. Wolf

Written Testimony

U.S. Senate Committee on the Budget

Hearing: Sanctuary Cities: The Cost of Undermining Law and Order

March 10, 2026

Thank you, Chairman Graham and Ranking Member Merkley, for the opportunity to testify before the committee today.

In 1979, the Los Angeles City Council implemented "Special Order 40," which prevented local police officers from questioning individuals on their immigration status, and stopped officers from conducting immigration-related arrests.

What we now call "sanctuary cities" was born nearly 50 years ago in California – and since then, has exploded nationwide. According to the Department of Justice, 12 states and the District of Columbia, along with 22 other jurisdictions, for now, are considered "sanctuary jurisdictions" – defined by the DOJ as jurisdictions that "materially impede enforcement of federal immigration statues and regulations." According to 2024 research, approximately *eight million illegal immigrants live in sanctuary jurisdictions.*

That also means that approximately one in every three Americans today, at a minimum, live in sanctuary jurisdictions – comprising over 120 million people. This is not a localized phenomenon. Ultimately, sanctuary policies affect every American and puts them in increased danger.

While not all sanctuary jurisdictions have the same policies, they all share a core belief that it is acceptable, even justifiable, to ignore illegal immigration. Indeed, sanctuary jurisdictions believe that they, rather than the federal government, have the authority to decide which federal laws may be enforced, and ultimately who can stay in our country. These beliefs belie in our Constitution, legal precedent, and common sense.

Tragically, sanctuary policies are unleashing devastating consequences for the American people – especially those who live in sanctuary jurisdictions.

In California, the birthplace of the sanctuary movement, local politicians released 4,641 criminal aliens from local jails in 2025, rather than handing them over to ICE for removal. Those criminals included aliens who had committed homicide, assault, sexual predatory offenses, and burglary.

In New York, nearly 7,000 criminal aliens were released in 2025– again, including murderers, sexual predators, and drug dealers.

Let me state this as clearly as I can: sanctuary policies put the American people in jeopardy and danger. Unfortunately, Americans – including the men and women of law enforcement - have already borne the cost of deadly sanctuary city policies.

For example, American motorists are being killed because sanctuary jurisdictions are handing out commercial driver's licenses to illegal aliens. An off-duty CBP officer in New York City was ambushed and shot by two criminal illegal aliens with final orders of removal - both of whom were repeatedly released by New York's sanctuary policies. And in Fairfax County, Virginia, a criminal illegal alien committed a murder the day after he was released from jail due to the sanctuary policies of that jurisdiction.

All of these tragedies are entirely preventable.

Rather than transferring custody of dangerous criminal illegal aliens in a jail setting, sanctuary policies force ICE agents to find and detain those aliens within the community – putting everyone at risk. Ironically, sanctuary policies achieve the opposite of what their proponents want – more ICE agents in their communities, and more operations occurring on their streets. If sanctuary politicians want to limit the visible presence of ICE in their communities, they should partner with the federal government rather than actively impeding them.

Unfortunately, sanctuary and anti-ICE policies are metastasizing nationwide. Politicians at the local, state, and federal level are pushing for policies that would deepen the crisis. Some proposals would outright abolish ICE, or return it to its neutered status under the Biden Administration. Other proposals are increasingly banning the detention of illegal immigrants and even creating "ICE-free zones".

The recent demands to "reform" ICE, made by some in Congress as part of negotiations related to the ongoing DHS shutdown are unrealistic proposals that ignore the key role of sanctuary policies in sowing chaos and worsening public trust in law enforcement.

Make no mistake: some on the Left will not stop until we have become a Sanctuary Nation – largely free from all border security and immigration enforcement.

Despite much of the media narrative on the issue, the American people are remarkably aligned on three key issues according to recent polling.

1. First, 67% of Americans support local officials handing over illegal aliens in jails to ICE. (Harvard-Harris, taken January 28-29)
2. Second, 67% of Americans believe that state and local authorities should cooperate with federal immigration authorities on the deportations of criminals. (Harvard-Harris, taken January 28-29)
3. And finally, 73% of Americans believe all criminal illegal aliens should be deported.

When looking at the core mission of ICE - that sanctuary cities are working to subvert – a broad coalition of Americans agree that state and local governments should work with, not against federal law enforcement on immigration matters.

The evidence is clear: to restore the rule of law, common sense, and public safety, the era of sanctuary jurisdictions needs to end – permanently. Here is how we get there:

- First, federal dollars must be tied to immigration enforcement measures and law enforcement cooperation – in both directions. Localities that refuse to cooperate must be held accountable financially while jurisdictions that protect ICE officers, honor detainer requests, and provide detention capacity must be promoted.
- Second, immunity must be provided to any law enforcement official who chooses to enforce ICE detainers.
- Third, every illegal alien with a final order of removal from a judge should be removed.
- Fourth, there should be a prohibition on any form of driver's license for illegal aliens.
- Fifth, continued resources must be allocated to any locality willing to enter into a 287(g) agreement with ICE.
- Sixth, a statutory definition of a "sanctuary jurisdiction" should be established to give enforcement mechanisms teeth.
- And finally, we should strongly enforce Title 8 Section 1324 of the U.S. Code which makes it a federal crime to harbor, conceal, or shield an illegal alien from law enforcement and create new criminal penalties for officials who ignore federal immigration requests.

No longer should we ignore officials who clearly state they plan to impede immigration enforcement or harbor illegal aliens. Minneapolis Mayor Jacob Frey recently stated that "Minneapolis will remain a welcoming city for all." In fact, on its webpage on immigration enforcement, Minneapolis unequivocally states that "The City of Minneapolis is committed to serving all residents – no matter their immigration status." Here, Mayor Frey is telling us that not only does he not believe in immigration enforcement but that the city will openly provide services and protection to illegal aliens.

These statements and policies are a direct contravention of Title 8, Section 1324, which states that any person "who encourages or induces an alien to come to, enter, or reside in the United States, knowing or in reckless disregard of the fact that such coming to, entry, or residence is or will be in violation of law."

It is time for sanctuary cities to become an artifact of American history – rather than ongoing safe havens for drug dealers, murderers, and other criminal illegal aliens.

I would like to close by reflecting on our Constitution, its founding principles, and the rule of law. As our forefathers debated the Constitution, they understood that for federalism to succeed, federal law had to unite the country and serve as the "supreme law of the land." This wisdom

was reflected through the adoption of Supremacy Clause in 1787, which was subsequently ratified by the states.

Due to the Supremacy Clause, and long line of judicial opinions interpreting it, states do not have the authority to pick and choose which federal laws are recognized or enforced. And they do not have the authority to nullify federal laws by ignoring them or proactively creating obstacles to their effectiveness.

Ironically, sanctuary politicians think believe they are protecting American communities. However, we have seen repeatedly what happens when states and local jurisdictions attempt to circumvent federal law. The result is chaos. Aliens from all over the world rush to our borders because they know sanctuary jurisdictions will shield them from removal. And with them come criminals, gang members, and even terrorists.

In the end, Americans pay the price. Taxpayers are forced to subsidize newcomers who use welfare programs and other public services. American workers are forced to compete with illegal laborers who drive down wages. And the American public sacrifices its public safety and security with the arrival of millions unvetted migrants. Even when our immigration agencies attempt to vet them, our ability is severely limited by the lack of access to reliable criminal records from other countries.

In 2024, ICE acknowledged that, to the best of its knowledge, there were over 647,000 criminal aliens at large in the U.S.: 425,000 had criminal convictions and another 222,000 had criminal charges pending. Over 13,000 aliens had convictions for murder. Another 15,000 have convictions for sexual assault; 56,000 aliens had convictions related to dangerous drugs; 62,000 had convictions for assault; 18,000 have convictions for larceny; 14,000 had convictions for burglary; 13,000 had convictions for weapons offenses, and 2,500 had convictions for kidnapping.

Under sanctuary policies, these aliens are released into American communities rather than transferred to ICE for removal. Many will re-offend.

So, I would like to ask a few simple questions to those who continue to support sanctuary policies: How many more Americans need to die at the hands of illegal aliens released by sanctuary jurisdictions? How many more innocent victims need to be assaulted and murdered by those same criminal illegal aliens? How many more American motorists need to lose their lives on our roadways to illegal alien drivers? How many more law enforcement agents need to be shot at by hardened criminals released by sanctuary cities?

How many more Americans need to lose their lives from sanctuary city policies before our elected leaders choose to act?

Let us heed the lessons we have learned from the tragedies that have unfolded in sanctuary cities, whether New York City, Los Angeles, Chicago, Seattle, or San Francisco. The American people

are safer when state and local governments cooperate with federal immigration agencies. At the very least, state and local governments should give our immigration agents access to their jails and prisons so that federal agents can screen prisoners, determine immigration status, process removable aliens, and effectuate a transfer of custody to federal authorities once an alien is released. This requires no effort or affirmative action on the part of local governments, yet under these radical sanctuary policies, state and local officials slam the doors shut on our immigration agents. They would rather release criminal aliens into our communities in the name of sanctuary policies that have directly contributed to the violence and loss of life we have seen unfold in our communities nationwide.

This madness must stop.

The Constitution grants Congress the plenary power over immigration and must use it to end sanctuary policies once and for all.

Thank you.

United States Senate
Committee on the Budget
"Sanctuary Cities: The Cost of Undermining Law and Order"
Full Committee Hearing

Testimony of Sheriff Michael L. Chapman
March 10, 2026

Chairman Graham, Ranking member Senator Merkley, and other members of this committee. I am Loudoun County Sheriff Mike Chapman and lead Virginia's most complex and largest full-service Sheriff's Office employing over 900. Loudoun County sits just outside of Washington, DC, and is home to Leesburg, Middleburg, and the Dulles International Airport.

In Loudoun County, we celebrate a diverse community, have an extraordinarily diverse workforce, welcome everyone here legally, and within our organization, speak over 25 different languages.

I have served 47 years in this profession as a local police officer, a career DEA special agent, and for the past fourteen years, as an elected sheriff. I am honored to sit before you today to discuss our mutual concern of sanctuary cities and counties. I am fortunate that Loudoun County is part of a traditional form of government that enables the sheriff to conduct all law enforcement, jail and court operations under one roof - essentially making me both a sheriff and police chief. That differs significantly from our surrounding jurisdictions where appointed county, city and town police chiefs are the norm.

Loudoun's form of government has enabled me to make decisions in the best interests of our citizens, free of mandated political agendas. It has also allowed me to be the only Republican law enforcement leader in all Northern Virginia. That responsibility is a double-edged sword. While I don't answer to our 7-2 Democratic Board of Supervisors for policy decisions, I do receive my budget from them. This dichotomy causes significant friction, making this a very challenging area. Our system of government, however, provides our citizens with a check and balance. We endured a 2-year battle with our Board of Supervisors who pursued converting our sheriff's office into a police department several years ago to exercise complete power and control over our agency. We defeated this aggressive action because our citizens ultimately decided that they prefer having a direct voice in who leads their public safety through the election process.

This system and our commonsense approach to problems, putting citizens first, is why our Sheriff's Office has been awarded the "Favorite Government Service" for two years running by our local newspaper, and why I have been selected as "Favorite Public Servant" for the past five years.

Regarding immigration, it is important to distinguish criminal aliens who pose a threat to our community from immigrants here legally. Consequently, while our surrounding jurisdictions choose to be sanctuaries, not make this distinction, and not honor ICE detainers, we do just the opposite.

We have always cooperated with all of our state, local and federal counterparts, including ICE, and have routinely notified ICE of those in our custody for criminal offenses who also had active detainers issued by ICE, regardless of the Presidential Administration. We honor all detainers and warrants from other law enforcement jurisdictions just as I would expect them to honor ours. These practices likely contribute to why our agency has the lowest crime rate in the DC - Northern Virginia area, and is one of the safest counties in the nation.

That doesn't mean we're exempt from problems related to illegal immigration. In 2024, we experienced five homicides, three of which were committed by people here illegally. Two of the three were committed by an MS-13 gang member. In a neighboring county, one that refuses to contact ICE regarding criminals they arrest, two homicides were committed in just over the past two months. One occurred within 24 hours after the criminal, who had an outstanding ICE detainer on him, was released from jail without any notification to ICE. The other, police questioned why the subject had been released and warned the prosecutor of the danger, stating, "not if, but when" he assaulted again - resulting in a grandmother being stabbed to death at a bus stop. This exemplifies what happens with sanctuary counties - law enforcement's hands are tied by politicians and political agendas.

Last year, I signed on to the 287g Warrant Service Officer Program which enabled us to hold a person arrested for a criminal violation an additional 48 hours for ICE in the event ICE couldn't pick them up immediately. This enabled us to safely turn over criminal illegal aliens to ICE who, otherwise, would have been released into our community and could pose an additional threat to our residents. This simple process has kept ICE from having to conduct street operations. I have told my community that not turning over criminal aliens to ICE who could endanger our residents would be irresponsible, and dangerous to the very people I've sworn an oath to protect.

This process, which has worked well for us, is likely to become far more challenging considering the legislation currently in motion in Richmond. Our governor rescinded the previous administration's Executive Order with had enabled jurisdictions to cooperate ICE. This was followed by legislation proposed by both houses of our General Assembly which, if passed and signed by the Governor, will likely require judicial warrants from ICE for local law enforcement to act on anything. Considering illegal immigration is a civil offense, this action will no doubt force us to release detainees, incarcerated by us for criminal offenses unrelated to immigration, back into our communities where they will pose an additional threat to our residents.

If this committee can exercise any leverage to influence a different outcome, I'm confident that law enforcement officials throughout Virginia and other locations who are facing similar challenges, would be grateful. We take our jobs seriously and want to keep all of our citizens safe.

Thank you for your attention. I look forward to answering any questions you may have.

U.S. Senate Committee on the Budget

"Sanctuary Cities: The Cost of Undermining Law and Order"

March 10, 2026

Statement of Jessica M. Vaughan

Center for Immigration Studies

Thank you, Chairman Graham and Ranking Member Merkley, for the opportunity to testify. Today there are 13 states and about 100 localities that have adopted sanctuary policies. We estimate that more than half of the illegal immigrant population resides in sanctuary jurisdictions, with more than one-fifth in California.[1] These policies are a significant hindrance to immigration enforcement and a challenge to the supremacy clause of the Constitution. Equally important, they bring substantial costs to the community – the needless human cost of new victimizations at the hands of criminal aliens released instead of being turned over to Immigration and Customs Enforcement (ICE) for removal, and the considerable fiscal costs associated with providing services to illegal migrants who are attracted by the policies. Congress should act to preserve the prerogative of law enforcement agencies to cooperate with immigration enforcement, penalize and create liability for jurisdictions that enact laws or practices to obstruct enforcement.

What is a sanctuary policy? The term does not appear in immigration law, but is generally understood to mean a state or local policy that seeks to shield illegal immigrants from the reach of supposedly unfair or overzealous immigration enforcement. The first sanctuary policies appeared in the 1980s, when humanitarian groups in the southwest sought to protect Central Americans who had come into the United States illegally to escape conflict in their countries.

Over time, a few cities like San Francisco, Berkeley, and Santa Fe, led by politicians who opposed immigration enforcement and advocated for amnesty for illegal immigrants declared their jurisdictions to be sanctuaries. They enacted "don't ask, don't tell" ordinances to forbid local officials from denying public benefits and services to illegal aliens and prohibiting officials, including police, from alerting immigration authorities after encounters with them.

In response, in 1996 Congress amended the Immigration and Nationality Act to prohibit state and local governments from "in any way" restricting the ability of officials to communicate or share information with federal immigration authorities regarding citizenship or immigration status.[2]

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[1] Jason Richwine, "How Many Illegal Aliens Reside in Sanctuaries?," Center for Immigration Studies, December 17, 2024, https://cis.org/Richwine/How-Many-Illegal-Aliens-Reside-Sanctuary-Jurisdictions.

[2] See 8 USC 1373 and 1644: "Notwithstanding any other provision of Federal, State, or local law, a Federal, State, or local government entity or official may not prohibit, or in any way restrict, any government entity or official from sending to, or receiving from, the Immigration and Naturalization Service [now the Department of Homeland Security] information regarding the citizenship or immigration status, lawful or unlawful, of any individual."

While this provision would seem to render the local "don't ask, don't tell" policies to be illegal, but the law provides no direct enforcement mechanism for the federal government to disrupt or punish these practices.

Some have argued that these provisions are unconstitutional, violating the 10th Amendment protections against the "commandeering" of state resources by the federal government. Nevertheless there are strong arguments to the contrary.[3] Sections 1373/1644 have to do with preserving discretion for information sharing in the public interest, and do not require states or localities to pass laws or ordinances, enforce a federal mandate, or assist with immigration law enforcement (as pro-sanctuary officials frequently claim).

Then, in 2008 the federal government began implementing a nationwide fingerprint-sharing program known as Secure Communities, which allowed ICE to match the biometrics of all those arrested by local police with immigration databases, and upon detecting a match, potentially follow up with detainers, or requests to hold the alien for ICE to take custody for removal. This program helped boost interior criminal deportations under the first two years of the Obama administration, as ICE was no longer dependent on local police to identify and refer criminal aliens, and most local authorities were happy to help ICE remove some of the lawbreakers causing problems in the community.

Anti-enforcement groups soon moved to thwart this collaboration by convincing hundreds of state and local governments to adopt the most recent iteration of sanctuary policies -- prohibiting local law enforcement agencies from honoring ICE detainers, notifying ICE of the pending release of a criminal alien, or taking any action whatsoever to aid immigration enforcement, even against criminal aliens.

Typically, jurisdictions that have adopted such non-cooperation policies also have policies that allow illegal aliens to receive public benefits, such as driver's licenses, municipal identification cards, welfare benefits, health insurance, temporary shelter, housing assistance, in-state tuition and public universities, and more.

The Human Cost of Sanctuary Policies. Sanctuary policies that block state and local law enforcement agencies from cooperating with ICE end up shielding criminal aliens from immigration enforcement. According to ICE records that I obtained through the FOIA process], between October 2022 and February 2025, sanctuary policies resulted in the release of more than 26,000 criminal aliens ICE was seeking to take custody of.[4] More than half were released in California, and more than one thousand released by jails in each of the states of Illinois, Virginia, Massachusetts, and Connecticut.

In Virginia, for example, during this time period 1,698 aliens who had been arrested and who were in the custody of local authorities were released despite ICE detainers. More than two-thirds (1,157) were in Fairfax County. In 2024, the Fairfax County Adult Detention Center released two offenders that ICE was seeking who had prior homicide convictions. One had

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[3] See George Fishman, "Is Federal Anti-Sanctuary Law Constitutional?," Center for Immigration Studies, February 26, 2025, https://cis.org/Report/Federal-AntiSanctuary-Law-Constitutional.

[4] Jessica Vaughan, "Which Sanctuaries Have Released the Most Criminals?," Center for Immigration Studies, April 9, 2025, https://cis.org/Vaughan/Which-Sanctuary-Jurisdictions-Have-Released-Most-Criminals.

instant pending charges for aggravated assault on a police officer. Four others released by Fairfax County and one released by the jail in Manassas that year had pending homicide charges. Virginia jails released 75 aliens with convictions or pending charges for sexual assault, 46 with convictions or instant charges for weapons offenses, and 285 with convictions or instant charges related to dangerous drugs.

Releasing these individuals instead of allowing ICE to take custody enables them to remain in the community and continuing to commit crimes, sometimes with tragic consequences. In May 2025, ICE finally managed to arrest Mykol Santos-Santos, a gang member and citizen of Guatemala, who was caught crossing the border illegally in 2014, was released into the country, and settled in Virginia. He was arrested by ICE in a targeted operation in 2019, released on bond by an immigration judge, and ordered removed in 2023, but he remained at large, except when held in custody by Virginia law enforcement agencies. Between February of 2020 and May of 2025, he racked up 30 arrests for offenses including assault and battery, drug possession, drug possession with intent to distribute, burglary, theft, larceny, trespassing, property damage, obstruction of justice, attempting to flee police, failure to appear and violating court orders. During that time ICE filed detainers 16 times, all of which were ignored.[5]

Just four months after ICE re-arrested Santos-Santos, another criminal alien named Marvin Fernando Morales-Ortez, a citizen of El Salvador, was arrested for assault and brandishing a firearm. ICE filed a detainer immediately, which was ignored by Fairfax authorities, who released him. One day later, Morales-Ortez, who had prior violent offenses, shot and killed a man in his home in Reston.[6]

These are not isolated incidents. According to ICE officials, more than 10,000 criminal aliens were subsequently arrested for crimes after having been released under sanctuary policies.[7] Some of these cases are documented by ICE in a 2015 report published by my organization.[8]

Another recent case illustrates the positive side of how the process should work, and why local law enforcement agencies should routinely practice cooperation with ICE and other DHS agencies. On February 19, 2026, in Lee County, NC, at the scene of a highway crash, sheriff's deputies and highway patrol officers encountered a man involved who gave them five different names, none of which could be authenticated or matched to a birth date. Upon fingerprinting, they learned his true name and that he was a citizen of El Salvador. The local officers then

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[5] "ICE Washington, D.C. arrests illegal Guatemalan gang member with extensive criminal history," May 19, 2025, https://www.ice.gov/news/releases/ice-washington-dc-arrests-illegal-guatemalan-gang-member.
[6] DHS Calls on Fairfax County Politicians to Honor ICE Detainer for Criminal Illegal Alien Previously Released Because of Their Sanctuary Policies Who Went on to Commit Murder the Next Day," December 19, 2025, https://www.dhs.gov/news/2025/12/19/dhs-calls-fairfax-county-politicians-honor-ice-detainer-criminal-illegal-alien.

[7] Tom Homan, quoted in Douglas Hanks, "In Miami, Jeff Sessions praises an end to sanctuary for 'criminal aliens,'" Miami.Herald, August 17, 2017, https://www.miamiherald.com/news/local/community/miami-dade/article167606497.html#storylink=cpy.
[8] Jessica Vaughan, "Rejecting Detainers, Endangering Communities," Center for Immigration Studies, July 13, 2015, https://cis.org/Report/Rejecting-Detainers-Endangering-Communities.

reached out to ICE, and subsequently discovered that the man was a MS-13 gang member wanted in El Salvador for kidnapping, drug trafficking and homicide, and he was taken into federal custody.[9]

The Fiscal Cost of Sanctuary Policies. Besides the public safety problems, sanctuary policies bring substantial costs to taxpayers. Recent settlement patterns and the accounts of migrants themselves confirm that sanctuary policies do attract illegal immigration, as illegal migrants seek out communities that put out a welcome mat. The unavoidable costs to state and local government include emergency health care and schools. In addition, many sanctuary jurisdictions also choose to allow illegal immigrants to access other public benefits, including Medicaid or other subsidized health insurance, food assistance, shelters or other publicly-funded housing, driver's licenses, educational vouchers, and in-state tuition.

My organization has found that approximately 61 percent of households headed by illegal immigrants are using at least one major welfare program, and often more than one.[10] This rate compares to 51 percent of legal immigrant-headed households using welfare, and 37 percent of U.S.-born citizen-headed households using welfare. Illegal immigrants can receive welfare on behalf of their U.S.-born children, and illegal-immigrant children can receive free/reduced price school lunch/breakfast and WIC directly. A number of states provide Medicaid to some illegal-alien adults and children, and a few provide SNAP. We have no evidence that the higher use rates of illegal aliens reflects widespread cheating or fraud; we believe that the high rates of welfare use primarily reflect the fact that illegal immigrants generally lower education levels and their resulting low incomes than legal immigrants or native-born, in addition to the presence of U.S.-born children who are eligible for all welfare programs from birth.

Such a high rate of welfare use by illegal immigrant-headed households may seem implausible. However, more than half of all illegal-immigrant households have one or more U.S.-born children. These children have the same welfare eligibility as any other U.S. citizen, including even public housing.[11] Second, many states offer Medicaid directly to illegal immigrants. For example, 14 states (California, Colorado, Connecticut, Illinois, Maine, Massachusetts, Minnesota, New Jersey, New York, Oregon, Rhode Island, Utah, Vermont and Washington) plus the District of Columbia currently offer Medicaid to all low-income children, regardless of immigration status. Even more states (29) provide it to all low-income pregnant

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[9] "Crash investigation in Lee County leads to arrest of MS-13 member wanted for serious crimes," WTVD Raleigh Durham, February 21, 2026, https://abc11.com/post/crash-investigation-lee-county-leads-arrest-jonathon-josue-ayala-melendez-ms-13-member-wanted-serious-crimes/18629715/.

[10] Steven Camarota and Karen Zeigler, "Welfare Use by Immigrants and the U.S.-Born, 2024," Center for Immigration Studies, February 4, 2026, https://cis.org/Report/Welfare-Use-Immigrants-and-USBorn-2024.

[11] The eligibility of the U.S.-born children of illegal immigrants extends even to housing. HUD regulations (the Housing.Handbook).state that if at least one member of a family is eligible (e.g., a U.S.-born child), then the family can live in federally subsidized housing, though they may receive prorated assistance. New York City has a similar rule for its own housing programs.

women, again, without regard to legal status. A few states go beyond this and offer Medicaid to other adult illegal immigrants.[12]

Third, illegal-immigrant children and pregnant women have the same eligibility for the federal WIC program as U.S. citizens do under federal law;[13] and all children are eligible for free subsidized school meal programs as well. In addition, there are six states (California, Connecticut, Illinois, Maine, Minnesota, and Washington) that offer SNAP-like benefits to illegal immigrants under limited circumstances.

Finally, it's important to note that because the overwhelming majority of illegal immigrants have no education beyond high school, as a result, a very large share of illegal immigrants have incomes low enough to qualify for welfare.

The cost of these programs has exploded in the wake of the unprecedented surge in illegal immigration under the Biden administration from 2021 to 2024. For example, Massachusetts experienced an influx of 50,000 illegal immigrants during that time period, sending the cost of its emergency shelter program, including food and transportation services, skyrocketing to over $1 billion per year.[14]

In 2023, the Massachusetts legislature quietly passed a measure in a supplemental spending bill allocated $6 million to provide SNAP benefits for seven months to certain classes of inadmissible aliens, including lawfully and unlawfully present aliens (but not recent legal immigrants) who met the income requirements. It was intended to cover most new arrivals who were not already eligible for benefits, and to be a bridge to more permanent funding that would be appropriated in the next fiscal year's budget. However, the funds were drained in just over two months and the program was shut down. According to the Massachusetts Law Reform Institute, 4,000 families received SNAP benefits during this time.[15]

Other costs – shelter NY: And in NY a similar story: https://nypost.com/2024/08/13/us-news/nyc-migrant-crisis-costs-will-crack-eye-popping-5-billion-on-shelters-security-and-food/

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[12] Akash Pillai, Drishi Pillai, and Samantha Artiga, "State Health Coverage for Immigrants and Implications for Health Coverage and Care," KFF, May 29, 2025, https://www.kff.org/racial-equity-and-health-policy/state-health-coverage-for-immigrants-and-implications-for-health-coverage-and-care/.

[13] See the WIC eligibility guidelines here: https://www.fns.usda.gov/wic/eligibility#:~:text=To%20be%20eligible%20for%20WIC%2C%20applicants%20must,whether%20the%20individual%20is%20at%20nutrition%20risk.

[14] Michael Dorgan, "Migrant influx pushing Mass. shelter costs past $1B in FY25: report," Fox News, June 18, 2025, https://www.foxnews.com/politics/migrant-influx-pushing-mass-shelter-costs-past-1b-fy25-report?msockid=15e06b74fddd66652fb57eacfc4367f0.

[15] Jessica Vaughan, "Massachusetts: A Case Study in Mass Immigration and the Welfare State," Center for Immigration Studies, July 25, 2024, https://cis.org/Report/Massachusetts-Case-Study-Mass-Immigration-and-Welfare-State.

Costs for Education. One of the most significant burdens that illegal immigration imposes on state and local governments is the responsibility of educating children brought by their parents who crossed illegally, children who arrived unaccompanied (to work or to join parents already living here illegally), or children born here to parents who are illegal aliens. The Center estimates that there currently are approximately four million children of illegal aliens or unaccompanied minors in U.S. schools.

As an example, Massachusetts has an estimated 25,000 children in the state's schools who are illegal aliens, and at least that number who are the U.S.-born children of illegal aliens living there. Of these, at least 10,000 arrived during the Biden administration, including 8,500 unaccompanied minors.

Taxpayers spend an average of nearly $23,000 per student per year, setting the base cost of illegal alien children at $575 million per year — with 40 percent of the current cost attributable to the arrival of children under Biden policies. Moreover, according to the studies reviewed by the CBO, the cost of educating students who do not speak English fluently is 20 to 40 percent higher than the cost of educating a U.S.-born student.

In addition to schooling, the state provides other services to illegal alien children who arrived as unaccompanied minors. According to the most recent annual report of the Massachusetts Office for Refugees and Immigrants (ORI), services to unaccompanied minors represent a whopping 61 percent of the office's $19.4 million total budget. This dwarfs the amount of funding that is devoted to legal immigrants and refugees for programs such as naturalization, assimilation, and employment assistance. The vast majority (85 percent) of ORI's budget comes from federal funding.

Healthcare Costs. Many sanctuary states allow all illegal immigrants to have access to government-subsidized or government-paid healthcare – in addition to the "emergency" health care that under federal law must be provided to anyone who needs it, regardless of immigration status. Many of the new arrivals, including Haitians and Ukrainians, have been granted entry under programs that gave them immediate access to federal means-tested benefits, including Medicaid.

Some sanctuary states, provide healthcare coverage to all children who lack private health insurance. Again, using Massachusetts as an example, all children under the age of 19 who lack private healthcare coverage, regardless of immigration status, are covered for primary, preventative, and dental care under the state's Children's Medical Security Plan. The remainder of the illegal population that is without private insurance and not covered by Medicaid has access to limited publicly funded health insurance. These services include pre-natal and maternity services, dialysis, and certain other conditions. In addition, under the Massachusetts Health Safety Net program, low income, uninsured inadmissible immigrants may have certain hospital facility services, such as hospital rooms, nursing, and technicians, covered by the state-funded program. One state study academic study estimated in 2023 that the direct cost of health insurance for each additional nonelderly adult immigrant (legal or illegal) to be about $3,800 per person per year.

Other states that declared themselves to be sanctuaries experienced similar upsurges in the cost of welfare programs. Several states, including California[16] and Illinois[17], have since had to scale back these programs because they became unaffordable.

Justifications Given for Sanctuary Policies Have Been Disproven. Proponents of sanctuary policies commonly invoke two main reasons for imposing restrictions on local law enforcement agencies. First, they maintain that cooperation with federal immigration authorities will erode immigrants' trust in local law enforcement and cause immigrants to refrain from reporting crimes. Second, they frequently claim that state and local law enforcement agencies lack the authority to honor immigration detainers without a "judicial" warrant. Both of these defenses are false.

1. "The Chilling Effect" Myth. Sanctuary proponents often assert that non-cooperation policies are needed to enable immigrants to feel comfortable reporting crimes. This frequently heard claim has never been substantiated and, in fact, has been refuted by a number of reputable studies and testimonies.[18] No evidence of a "chilling effect" resulting from local police cooperation with ICE has emerged from federal or local government data or independent academic research.

It is important to remember that crime reporting can be a problem in any place, and is not confined to any one segment of the population. In fact, most crimes are not reported, regardless of the victim's immigration status or ethnicity. According to the Bureau of Justice Statistics (BJS), in from 2020-23, only about 40 to 50 percent of violent victimizations and 50 to 60 percent of robbery victimizations were reported to police from 2020-23, with significantly lower numbers of violent victimizations being reported in urban areas.[19]

Further, data from the Bureau of Justice Statistics have shown no meaningful differences among ethnic groups in crime reporting. The combined results of three years of data (2017-19) from the National Crime Victimization Survey (NCVS), which is the most authoritative study of crime reporting in the United States, show that immigrant victimizations are just as likely, and in some cases more likely, to be reported to police than crimes against the native-born. This was true even though the data was collected in the first three years of the Trump administration, during which time the media and advocacy groups routinely asserted that immigration

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[16] Paul Kiernan,»California Cuts Back on Costly Immigrant Healthcare," Wall.St¡Journal?December 20, 2025, https://www.wsj.com/politics/policy/california-cuts-back-on-costly-immigrant-healthcare-70f2794d?mod=hp_lead_pos11.

[17] Peter Hancock, "State on track to end health coverage program for immigrant adults," Capitol News Illinois, May 14, 2025, https://www.nprillinois.org/government-politics/2025-05-14/state-on-track-to-end-health-coverage-program-for-immigrant-adults.

[18] See, as just one example, the June, 2017 testimony of Kenneth Blanco, Acting Assistant Attorney General of the DOJ Criminal Division before the Senate Judiciary Committee, reported here: https://cis.org/Vaughan/Top-Justice-Official-Scoffs-Immigrant-Crime-Reporting-Chilling-Effect.

[19] Bureau of Justice Statistics, "Reporting to Police by Type of Crime and Location of Residence, 2020-2023," July 2025, https://bjs.ojp.gov/library/publications/reporting-police-type-crime-and-location-residence-2020-2023/web-report.

enforcement had been dramatically expanded. The data also provides insight on why some immigrants and non-citizens might not report crimes, showing that only a tiny number — about 1 percent — said that the reason for not reporting was because of fear of the authorities. There is no evidence in the NCVS data that crimes against immigrants are reported to police at lower rates than crimes against the native-born, indicating that the routine, even active, cooperation between local law enforcement and federal immigration authorities that takes place in most jurisdictions does not suppress crime reporting by immigrants.[20]

We found that this was consistently true for various types of crimes, and it is true for female victims, for Hispanic non-citizens (a group that includes many illegal aliens), for younger and presumably more recently arrived immigrants, across most geographic regions, and in both small and large communities. This is the case even though law enforcement in most jurisdictions routinely cooperates with federal immigration enforcement authorities. We found no indication that parts of the country with sanctuary jurisdictions had higher crime reporting rates by immigrants. On the contrary, the South had the highest crime reporting rates by immigrants, and it the region of the country with the fewest sanctuary policies.

A multitude of other studies refute the notion that local-federal cooperation in immigration enforcement causes immigrants to refrain from reporting crimes:

- A major study completed in 2009 by researchers from the University of Virginia and the Police Executive Research Forum (PERF) found no decline in crime reporting by Hispanics after the implementation of a local police program to screen offenders for immigration status and to refer illegals to ICE for removal. This examination of Prince William County, Virginia's, 287(g) program is the most comprehensive study to refute the "chilling effect" theory. The study also found that the county's tough immigration policies likely resulted in a decline in certain violent crimes.[19]
- The most reputable academic survey of immigrants on crime reporting found that by far the most commonly mentioned reason for not reporting a crime was a language barrier (47 percent), followed by cultural differences (22 percent), and a lack of understanding of the U.S. criminal justice system (15 percent) — not fear of being turned over to immigration authorities. (Davis, Erez, and Avitable, 2001).
- The academic literature reveals varying attitudes and degrees of trust toward police within and among immigrant communities. Some studies have found that Central Americans may be less trusting than other groups, while others maintain that the most important factor is socio-economic status and feelings of empowerment within a community, rather than the presence or level of immigration enforcement. (See Davis and Henderson's 2003 study of New York and Menjivar and Bejarano's 2004 study of Phoenix).

Recent studies based on polling of immigrants about whether they might or might not report crimes in the future based on hypothetical local policies for police interaction with ICE, such as one recent study entitled "Insecure Communities", by Nik Theodore of the University of

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[20] Jessica M. Vaughan, Steven A. Camarota, and Karen Zeigler, "Are Immigrants Less Willing to Report Crime? Data from the National Crime Victimization Survey says "No." October 14, 2021, Center for Immigration Studies, https://cis.org/Report/Are-Immigrants-Less-Willing-Report-Crime.

Illinois, Chicago, should be considered with great caution, since they measure emotions and predict possible behavior, rather than record and analyze actual behavior of immigrants. Moreover, the Theodore study is particularly flawed because it did not compare crime reporting rates of Latinos with other ethnic groups.

Ironically, sanctuary policies lead to exactly the kind of enforcement that advocates say is frightening to immigrants. When ICE is not able to arrest criminal alien targets in jails, they have to apprehend them in public – at their dwelling, at work, in a courthouse, or on the street. Besides the fact that such at-large arrests require more officers and more time to surveil and plan the arrest, these operations also put officers and the public at risk. The targets have attacked ICE officers, or try to flee, sometimes into schools and medical centers, causing havoc. They will even leave their children behind, as occurred recently in Minneapolis

The reason that immigration enforcement has not become a spectacle in Texas, Florida, and other states that also have experienced a significant uptick in enforcement is because ICE is working with, not against, local authorities, and is able to take care of business in the secure environment of jails rather than in neighborhoods, where anything can happen.

For these reasons, law enforcement agencies across the country have found that the most effective ways to encourage crime reporting by immigrants and all residents are to engage in community outreach, hire personnel who speak the languages of the community, establish anonymous tip lines, and set up community sub-stations with non-uniform personnel to take inquiries and reports — not by suspending cooperation with federal immigration enforcement efforts.

2. Legal Issues on Detainers. Some jurisdictions have adopted sanctuary policies that prohibit agencies from honoring ICE detainers. Sometimes these policies are imposed by elected officials and sometimes they are recommended by county attorneys or adopted by sheriffs who are concerned about liability for lawsuits. While there remains some outstanding legal controversy over certain issues, including liability, there should be no controversy whatsoever regarding a local agency's ability to honor a detainer in cases of aliens arrested for controlled substance offenses. Federal law explicitly permits local officers to notify federal immigration authorities to request a detainer, and directs federal immigration authorities to decide promptly if a detainer can be issued, and to act to take the alien in custody if appropriate (8 USC 1357(d). There is no valid reason or excuse for local law enforcement agencies not to be contacting ICE and requesting detainers for alien drug offenders — especially for those who traffic in heroin, fentanyl, or other opioids.

Ironically, this provision was enacted by Congress because of widespread frustration among local law enforcement agencies and their congressional representatives that the federal immigration agency (INS) was not responding to their requests to remove drug offenders from their community. This was a time when there were significantly fewer than 1,000 investigative agents available to conduct their work for the entire United States. While detainers routinely were filed by INS agents for serious offenders, among those who fell through the cracks were second-tier drug offenders. This so angered police chiefs and sheriffs that they lobbied Congress to require INS agents to respond by filing a detainer if the arresting police agency requested it.

Fast forward a few decades, and we now have substantially more agents and officers in ICE, the successor agency to the INS, but now we have police, sheriff's departments, and even some correctional agencies, that decline to honor immigration detainers, meaning many non-citizen drug offenders are able to remain here with impunity, thanks to the sanctuary policies.[21]

For many years, numerous sanctuary jurisdictions have conditioned their cooperation on a requirement that ICE provide a "judicial" warrant to enable them to hold a criminal alien inmate for ICE. This is an unreasonable requirement that is designed as a pretext to refuse cooperation. Under our immigration laws, passed by Congress, which has plenary authority to decide how immigration laws can be enforced, there is no requirement for immigration officers to obtain an arrest warrant issued by a judge. These warrants do not exist Congress recognized that such a requirement would be impractical and onerous in the context of routine immigration enforcement (which is primarily civil in nature), and would be an undue burden on the judiciary as well. The implication that the lack of a "judicial" warrant necessarily amounts to a civil rights violation or an abuse of authority is misleading. Further, in practice and by policy, ICE does go above and beyond the requirements of the law by issuing a warrant of arrest or warrant of removal with every immigration detainer, which also includes a statement of probable cause that the alien is removable. Moreover, immigration detainers allow for law enforcement agencies to notify ICE, rather than hold the individual in custody, to give the agency a reasonable amount of time to take custody of an inmate before release – yet many sanctuary jurisdictions prohibit this communication, even in the case of violent and/or serious offenders.

Recommendations for Congress.

1. Stipulate that certain types of federal funding (such as grants to law enforcement agencies and reimbursement for the cost of incarcerating illegal aliens) shall be withheld from jurisdictions that adopt certain sanctuary policies and/or violate federal law;
2. Clarify that state and local law enforcement agencies have legal authority and discretion to cooperate with ICE, along with reasonable immunity while acting under this authority;
3. Allow ICE to decline to transfer custody of aliens under a detainer to sanctuary jurisdictions;
4. Provide a private right of action for victims who suffer serious harm by deportable aliens released by egregious sanctuaries;
5. Withhold tax-exempt status for municipal bonds issued by sanctuary jurisdictions.

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[21] Dan Cadman, "An Anti-Sanctuary Bill Doesn't Quite Get to the Goal Line", Center for Immigration Studies, February 11, 2018.

Testimony of
David J. Bier
The Selz Foundation Chair in Immigration Policy
at the Cato Institute
Before
the Committee on
the Budget
United States Senate

March 10, 2026
Re: "Sanctuary Cities: The Cost of Undermining Law and Order"

Cato Institute • 1000 Massachusetts Ave., N.W. • Washington, D.C. 20001 • (202) 842-0200
Fax: (202) 842-3490 • www.cato.org

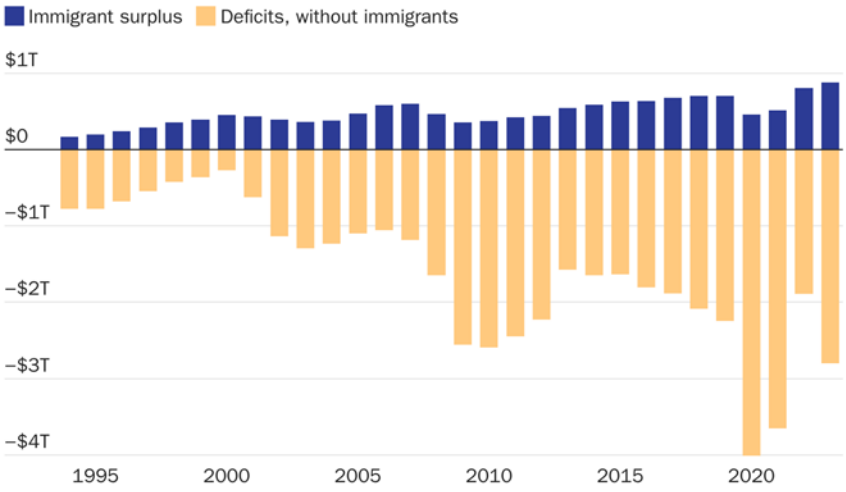
Chairman Graham, Ranking Member Merkley, and distinguished members of the Committee, thank you for the opportunity to testify.[1]

My name is David Bier. I occupy The Selz Foundation Chair in Immigration Policy at the Cato Institute, a nonpartisan public policy research organization in Washington, D.C. For nearly half a century, the Cato Institute has produced original research showing that people—whatever their ancestry, background, or birthplace—can benefit the United States when the Constitution's principles of individual liberty and limited government are followed.

Immigrants come for freedom—freedom to work for us and with us, to improve our lives. As I have testified before this committee in the past, immigrants will play critical roles in America's economic vitality in the future.[2] They work at higher rates and are nearly twice as likely to start businesses.[3] Cato's research has shown that immigrants have reduced federal, state, and local government budget deficits by a combined $14.5 trillion, including $1.7 trillion from illegal immigrants, over the last 30 years.[4]

Immigrants' fiscal surplus has grown even as deficits have exploded

Net fiscal impact, immigrants and US population without immigrants, 2024 dollars, 1994–2023



Source: Calculations are based primarily on the Annual Social and Economic Supplements of the US Census Bureau's Current Population Survey for March 1994–2023. See Appendix for full details.
Note: Includes interest savings from reduced debt in prior years.

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Cities are forced to deal with the fallout of Congress's flawed immigration system.

Unfortunately, Congress has failed to update the legal immigration system in any meaningful way since 1990. As a result, only about 3 percent of those seeking lawful permanent resident status in the United States received green cards in 2024.[5] For people who are not close relatives of US citizens, the percentage is less than one percent.

Unfortunately, rather than fix this system, the current administration has undermined it, banning a majority of the few legal immigrants from abroad who were previously permitted to immigrate. That includes nearly all legally admitted refugees—except for South African Afrikaners—and about half of all immigrant visa applicants, including half of all spouses and minor children of US citizens.[6]

Congress's continual refusal to correct its past mistakes and legalize immigration has forced states and local governments to deal with the fallout of illegal immigration. America's cities benefit from the economic contributions of these immigrants, who are integrated into their communities as family members, friends, parishioners, workers, and employers. In some areas of the United States, as many as one in five people lives in a household with an illegal immigrant.[7] As a result, Congress and the president have forced these communities to choose between aiding federal law enforcement and tearing apart their cities.

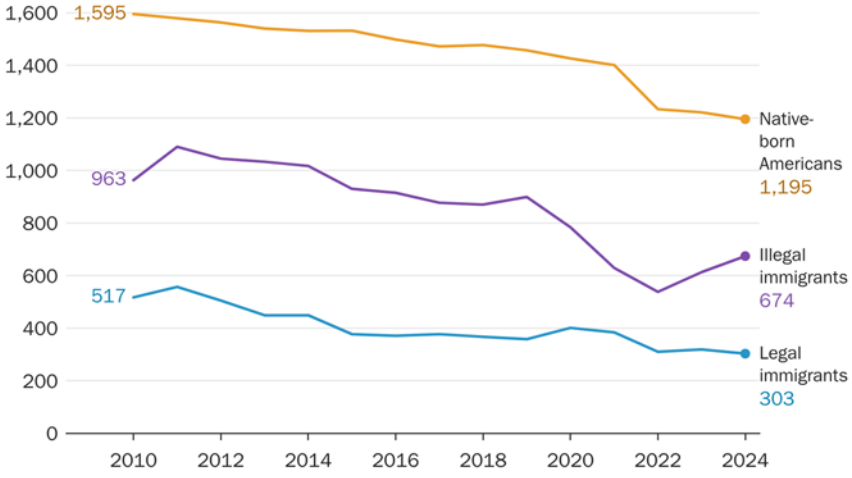
Noncitizens who threaten the lives and liberty of Americans should be prosecuted, convicted, imprisoned, and deported—whether they came in legally or not—and states may have a role to play in that effort. However, the federal government's focus on indiscriminate enforcement undermines trust and cooperation with municipalities. A better approach would require Immigration and Customs Enforcement (ICE) to concentrate its efforts on serious offenders so that localities could be confident about ICE's intentions when working with local officers.

Cities have good reasons to limit participation in immigration enforcement.

- *Illegal immigrants provide goods and services that improve the lives of Americans.* Mass deportation would cut US production by about 7 percent of Gross Domestic Product.[8] Illegal immigrants look after Americans' children, provide their health care, and care for them when they become elderly and disabled. Immigrants build and clean Americans' homes, harvest their fruits and vegetables, and process their meat and seafood. Mass deportation would crash local economies, creating a death spiral of lost consumers, closed businesses, and declining tax revenue.
- *Many Americans' jobs directly depend on illegal immigrant workers.* For instance, there are 33 million US-born Americans who are managers or supervisors for over 8 million unauthorized immigrant workers.[9]
- *Illegal immigrants are providers for US citizen family members.* There are over one million undocumented spouses of US citizens, and nearly 17 million US citizens share a household with an unauthorized immigrant.[10] Mass deportation would impoverish many of these families, cutting household income at least in half.[11]
- *Illegal immigrants supported state and local governments* with more than $37 billion in state and local tax revenue in 2022.[12]
- *Illegal immigrants reduce crime rates by committing fewer crimes.* More than a dozen studies have found that immigration, including illegal immigration, is associated with less crime.[13] Illegal immigrants were half as likely to have committed an offense serious enough for them to be incarcerated in the United States in 2023.[14] In recent years, there has been a negative correlation between murders and immigration court filings in major US cities.[15]

Immigrants, regardless of legal status, are less likely to be incarcerated

Subgroup incarceration rate per 100,000 residents, ages 18–54



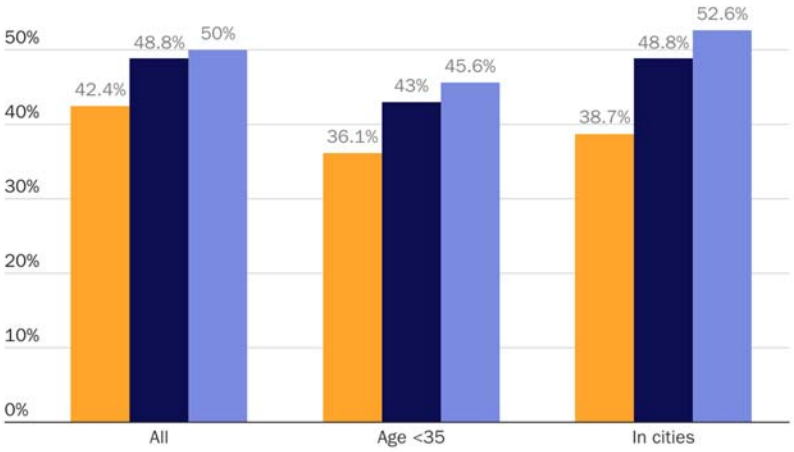
Source: American Community Survey, 2024;Michelangelo Landgrave and Alex Nowrasteh, "Illegal Immigrant Incarceration Rates, 2010–2024," Cato Institute, 2026 (forthcoming).

Fewer than half a million immigrants with criminal convictions of any kind were on ICE's deportation docket in 2024—which is less than 3 percent of the roughly 14 million illegal immigrants.[16] Like parking violations, illegal presence in the United States is a civil offense, not a crime. Although ICE's docket does not include every illegal immigrant convicted of anything, it consists of the vast majority. Regardless, 3 percent is a low share compared to the broader population. Roughly 30 percent of Americans have been arrested,[17] and about 8 percent have felony convictions.[18]

- *Illegal immigrants can directly stop crimes.* An unauthorized immigrant working as a convenience store nightwatchman stopped a burglary in Texas.[19] Another in New Mexico chased down a child abductor, safely returning a 6-year-old girl to her parents.[20] Two Venezuelan asylum seekers in New York City (Oswaldo Robles Lino and Josnan Alberto Palacios) stopped a stabbing in 2024.[21]
- *Illegal immigrants help solve crimes.* From 2017 to 2023, noncitizens—including illegal immigrants—reported 2.5 million crimes to police, reporting violent crimes at higher rates than US-born Americans.[22] Over the last decade, about 100,000 unauthorized immigrants have obtained legal status through their collaboration with law enforcement,[23] and local agencies have about 415,881 requests pending for unauthorized immigrants to receive legal status based on their cooperation with them right now.[24] Hispanics were somewhat more likely to report crimes in sanctuary cities from 1980 to 2004.[25]

Crimes against immigrants are more likely to be reported to the police

Share of violent crimes reported by nativity, 2017–2023



Source: National Crime Victimization Survey (NCVS), Bureau of Justice Statistics.
Note: Differences with the US-born are statistically significant at the 95 percent confidence level.

- *Cities have more important law enforcement priorities*. In the United States, more than 40 percent of murders, about 60 percent of violent crimes, and 85 percent of property crimes go unsolved.[26] Mandating that police divert resources away from these offenses to enforce immigration status violations would make cities less safe. It prioritizes jail space for non-threatening individuals over addressing actual threats to the community. In Los Angeles, for instance, it costs over $400 per day to hold someone in jail.[27]
- *Reasonable restrictions on aiding immigration enforcement do not increase crime*. A dozen studies have found that limits on participation in immigration enforcement do not lead to higher crime rates.[28] Sanctuary policies were associated with fewer assaults and burglaries after adoption in 2014.[29] There was no change in Florida counties that adopted sanctuary policies.[30] From 1999 to 2010, sanctuary policies had no effect on crime in those cities.[31] Cities that first participated in ICE's "Secure Communities" jail surveillance program did not lead to decreases in crime relative to other cities.[32] "Sanctuary cities," broadly defined, saw more significant decreases in both violent and property crimes from 2014 to 2016,[33] and they did not experience higher violent or property crime rates than other cities.[34] Additionally, California's SB 54 did not increase crime rates.[35]

  A limitation of this research is that it mostly predates many of the most restrictive policies on immigration enforcement participation, and it includes a diverse range of

policies. From 2010 to 2015, these policies did reduce deportations, but not of people with violent convictions.[36] This is an indication that those policies did not severely hamper ICE's efforts to target violent criminals during that period. Whether these conclusions hold true for more recently enacted policies remains to be studied.

Illegal immigrants could contribute more to their cities and the country if they had a permanent legal status and employment authorization. States and cities should never participate in immigration enforcement efforts targeting peaceful individuals.

The Constitution protects state autonomy.

States have the independent authority to decide how their law enforcement agencies interact with federal immigration enforcement. Although regulating immigration falls solely under federal jurisdiction, and federal law is supreme, states may still determine how to allocate their resources and whether to assist in enforcing federal law.

Under the Constitution's federalist system, state governments are not creations of the federal government. Under the 10th Amendment, states retain all powers not delegated to the federal government. This system of "dual sovereignty" keeps the states directly accountable to their citizens for any actions that they take. State officials cannot claim that the federal government forced them to take any particular action.

As the Supreme Court stated in *Printz v. United States* (1997), "The power of the Federal Government would be augmented immeasurably if it were able to impress into its service—and at no cost to itself—the police officers of the 50 States."[37] The Supreme Court has extended this principle to include the use of federal monetary grants to coerce cooperation. *NFIB v. Sebelius* (2012) stated: "Congress may use its spending power to create incentives for States to act in accordance with federal policies. But when 'pressure turns into compulsion,' ibid., the legislation runs contrary to our system of federalism."[38] In *Murphy v. National Collegiate Athletic Association* (2018), the Supreme Court clearly stated that: "Congress cannot issue direct orders to state legislatures."[39]

Moreover, several courts have found that it is unconstitutional for states to detain individuals solely at the request of ICE.[40] More than a dozen settlements have involved subjects of immigration detainers who were detained by localities.[41] This means that states and localities *cannot* lawfully arrest and detain someone merely on the basis of an immigration detainer request from the federal government without a court order supported by probable cause. Many states and cities are labeled "sanctuaries" simply for following these court decisions.

During its first term, the Trump administration attempted to impose new immigration reporting requirements for certain law enforcement grants. However, four of the five appeals courts that considered the issue found that those requirements were adopted unlawfully and unconstitutionally.[42] As the Seventh Circuit Court of Appeals wrote in 2020, "states do not forfeit all autonomy over their own police power merely by accepting federal grants."[43]

The administration sows mistrust between federal and local law enforcement.

Notwithstanding these facts, the administration is attempting to bully state and local governments into compliance with its agenda.

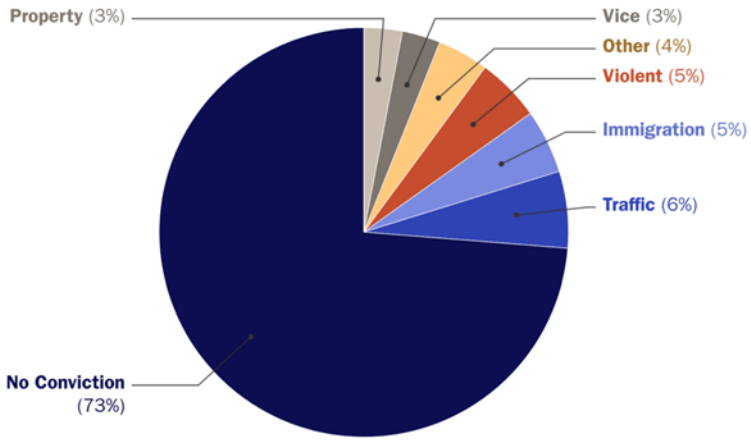
- *The administration is attempting to coerce state governments unconstitutionally.* The president has issued an executive order that attempts to block all federal grants to

municipalities that do not allocate their resources to help ICE.[44] His administration has sued Illinois and Chicago for refusing to help ICE, asserting that the president can mandate that they do what he wants,[45] but the case was dismissed.[46] The Department of Justice (DOJ) has also issued a memorandum requiring criminal investigations into state and local officials who fail to participate in federal enforcement.[47]

- *The administration is attempting mass deportation of peaceful immigrants.* President Trump signed an executive order removing requirements that ICE target public safety threats, and instead mandating that ICE and DOJ focus on immigration status violations.[48] In the most recent data showing the type of crimes, only 5 percent of people booked into detention had a violent criminal conviction, and 73 percent had no conviction.[49] Just as significantly, the Department of Homeland Security (DHS) itself classified just 10 percent of detainees as the highest threat level, and 72 percent of detainees were not deemed a threat at all.[50]

Only 5 percent of individuals booked into ICE detention ever had a violent criminal conviction

ICE book-ins into custody, by most serious conviction, FY 2026, Oct 1 - Nov 15, 2025



Source: Immigration and Customs Enforcement, "ICE Initial Book-Ins by Criminality and MSC: FY2025 YTD," November 15, 2025. See Appendix Table A for classifications.
Convictions include convictions at any time, not just during the period listed.

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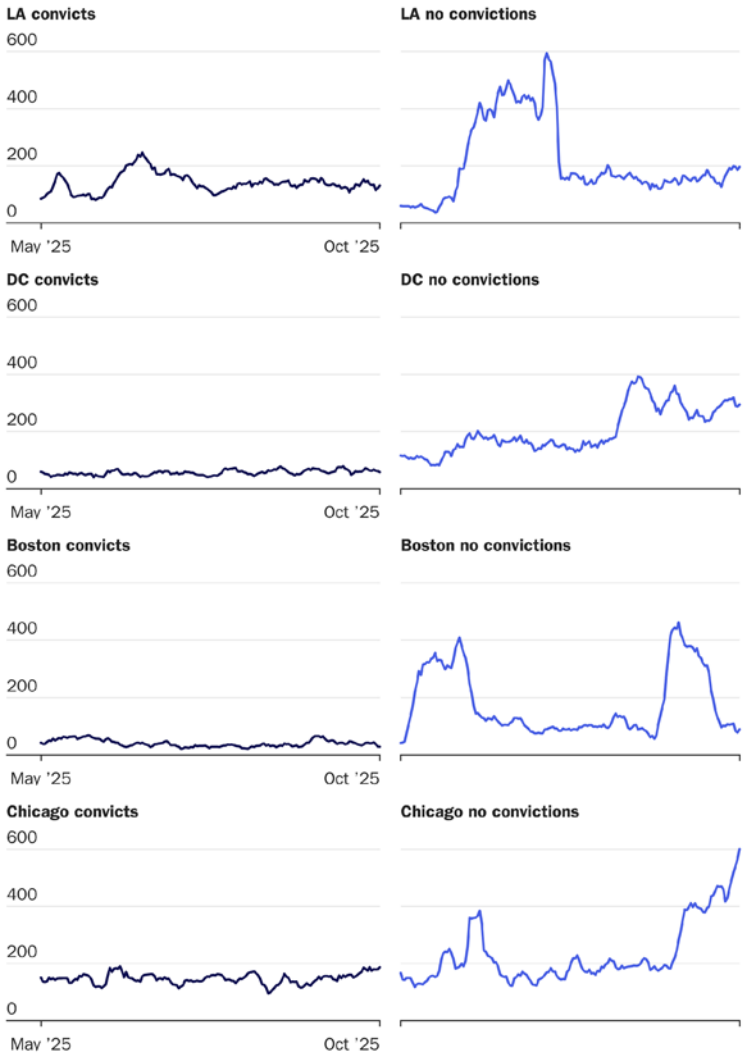
- *The administration's crackdown on sanctuary cities is not catching many criminals.* In each case where DHS has conducted an enforcement surge operation in a sanctuary city, nearly all the increase in arrests came from individuals without criminal convictions. From the week prior to the peak week, 91 percent of the increase in ICE arrests came from non-convicts in Los Angeles; 95 percent in D.C.; 92 percent in Boston; and 88 percent in Chicago, though the data series ends before the peak in Chicago.

Nearly all the increase in arrests during DHS surge operations are not criminals

ICE arrests, rolling weekly totals, May 1 - October 15, 2025



Source: Immigration and Customs Enforcement via Deportation Data Project FOIA request.

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- *The administration is engaging in racial and demographic profiling of US citizens.* DHS agents detain, interrogate, and demand proof of residence from people on the street based on demographic profiling without evidence of a violation of the law. When this unconstitutional harassment was temporarily enjoined in Los Angeles, arrests fell by two thirds immediately after a court found it was illegal.[51] DHS appears not to track its use of profiling. But to give a sense of the prevalence of indiscriminate targeting of people on the streets, consider that by October 2025, more than one in four arrests were of immigrants on the streets—that is, outside of any facility, prison, jail, court, DHS office, etc.—who had no criminal conviction or charges and no final removal order. In other words, there was no reason for them to be approached by government agents on patrol. This policy ensnares many US citizens and legal residents. Fox News reported that ICE agents interrogated about ten people for every one person they arrested in Florida.[52]

ICE relies heavily on indiscriminate arrests of immigrants on the streets

ICE arrests, rolling weekly totals, Jan 1 - October 15, 2025

Source: Immigration and Customs Enforcement via Deportation Data Project FOIA request.
Notes: Street arrests are defined as any arrest not in state or local custody, a jail, prison, police department, office, court, detention center, or other facility

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- *The administration is targeting lawful immigrants for deportation.* DHS is ending parole and Temporary Protected Status for over two million immigrants who had legal status at the start of this administration.[53] ICE is arresting people with valid parole status and subjecting them to detention and expedited removal.[54]
- *The administration is attempting to arrest and deport US citizens.* The president signed an executive order that purports to strip US citizenship from, and render deportable, all children born to temporary legal residents and people without permanent legal status, despite the constitutional guarantee of birthright citizenship.[55]
- *The administration blames cities for its own failures*: DHS has said that it must make arrests on the streets because jails in Minneapolis and other sanctuary cities do not cooperate. DHS has failed to issue detainers for nearly 30 percent of the noncitizen inmates in Minnesota prisons, despite the prisons informing DHS of their presence and their willingness to coordinate transfers.[56] In one revealing instance, DHS erroneously arrested a US citizen on January 19 and claimed it was looking for another man who was

already in prison.[57] DHS has repeatedly claimed that the few criminal immigrants it has arrested in Minnesota were taken "off the streets" when, in fact, they were transferred directly from Minnesota prisons.[58] When ICE did arrest immigrant criminals outside of a jail or prison in Minnesota, it usually had nothing to do with a sanctuary policy.[59] In one case, for example, DHS claimed that a target was released by Minnesota officials when, in fact, he had been released by DHS officials under the first Trump administration and had no connection to cooperation with DHS's current operation.[60]

- *The administration is entering homes of US citizens without judicial warrants.* DHS implemented a non-public policy of entering residences to make civil immigration arrests of people with administrative orders of removal. According to DHS Secretary Noem, DHS has already used this policy at least 28 times to enter homes.[61] One such raid resulted in the illegal arrest of a US citizen in Minnesota, who was dragged from his home in nothing but shorts in freezing weather.[62] This policy violates the Constitution, threatens Americans' freedom, and creates unsafe, dangerous situations for DHS agents.
- *The administration does not follow its own use of force protocols*: The DHS agents who shot Renee Good three times violated standard police practices and DHS protocol by attempting to conduct an arrest without verifying that the vehicle was turned off, trying to stop a moving vehicle with their hands, walking in front of a moving vehicle, shooting at a moving vehicle, shooting at the driver of a moving vehicle, shooting despite no threat of death or serious bodily injury, and shooting despite the availability of a "safe alternative."[63] The DHS agents who killed Alex Pretti threw a bystander to the ground who was not impeding their operations and then used deadly force despite the absence of an imminent threat of death or serious bodily injury and the availability of other options. Despite these facts, DHS has asserted that both shootings were justified and according to officer training.[64] DHS also shot a man who, according to an FBI affidavit, was 10 feet away and running away from the agent.[65] Similar shootings, like that of Marimar Martinez in Chicago, occurred in other sanctuary cities.[66]
- *The administration is arresting victims and witnesses of crimes.* In one of its first actions, this administration rescinded policies that instructed officers to look for indications that an immigrant was a victim before arresting them, and it announced, "Current beneficiaries of victim-based immigration benefits may be subject to civil immigration enforcement."[67] In the months since, we have seen repeated cases of DHS arresting victims and witnesses of crimes.
    - In Iowa, ICE arrested Felipe Hernandez after he reported being shot during an attempted robbery before he had even healed.[68] Local police arrested him for a bench warrant based on an overdue traffic ticket and turned him over to ICE pursuant to state law.
    - In Michigan, ICE arrested Veronica Ramirez Verduzco after she reported an assault.[69]
    - In Kansas, ICE arrested U visa applicant, Jose Madrid-Leiva, despite having worked with police on a robbery case.[70]
    - In California, the Immigration Center for Women and Children filed a lawsuit on behalf of eight crime-victim U visa applicants who were arrested in 2025.[71] In a

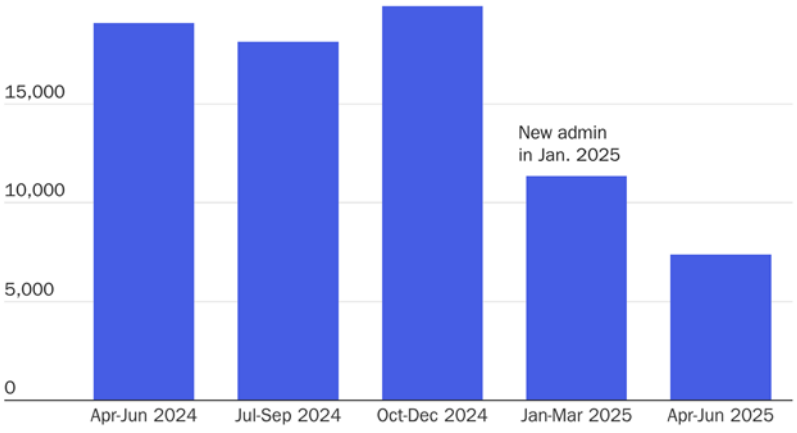
typical case, ICE had arrested a domestic violence survivor named Lupe who had lived in the United States for three decades and who had helped police successfully prosecute her attacker in 2017.

- In California, a judge ordered the release of Victor Amada Rodriguez-Flores, an immigrant crime victim and U visa applicant whom ICE had arrested.[72]
- In Texas, ICE arrested Oscar Bonilla after he agreed to help investigators crack a criminal case.[73] The Justice Department then criminally charged him for illegally entering the country.
- In Maine, local police notified Border Patrol about a man who was part of a group trying to stop a suicide attempt and who had called the police for help, and Border Patrol arrested him.[74]

These types of actions have led to an enormous decline in U visa applications for immigrant crime victims seeking status based on their cooperation with police. Filings have fallen by 63 percent from the last quarter of calendar year 2024 to the second quarter of 2025.[75] The administration has not published more recent statistics.

Immigrant crime victims who worked with the police filed 63% fewer applications in 2025

U visa applications filed, April 2024 to June 2025



Source: USCIS, "All USCIS Application and Petition Form Types," 2024-2025

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- *The administration is harassing, detaining, arresting, and killing people for protesting their activities.* DHS has detained or arrested numerous individuals for following, observing, and recording them, even though the Constitution protects every American's right to do so.[76] DHS Secretary Kristi Noem has said that it is "violence" to be "doxing" and "videotaping them where they're at when they're out on operations."[77] Not only do

DHS agents want to operate anonymously, with masks, no names, and often without even uniforms, they insist on threatening, detaining, and arresting people who observe, record, or protest their activities. Observers have recorded DHS agents threatening to "put hands on you and arrest you" if they "continue to follow us."[78] One judge found an "ongoing, sustained pattern of conduct" where drivers were stopped without probable cause of a crime for "following federal law enforcement agents at a safe distance to observe the agents' activity."[79]

- *The administration is violating habeas corpus and ignoring court orders*: The Constitution guarantees all individuals in the United States the writ of habeas corpus, or the right to challenge their confinement in court. In thousands of cases, DHS has asserted that individuals have no right to seek release, and judges have overwhelmingly rejected these assertions.[80] Nonetheless, DHS has violated nearly 100 court orders to release people detained without bond in Minnesota alone.[81] DHS has violated court orders in dozens of other cases regarding different immigration matters as well, undermining the rule of law and the Constitution.[82]

The administration's reckless assault on the rule of law in pursuit of indiscriminate mass deportation creates mistrust between immigrants and law enforcement and between state and local governments and the federal government. Congress should restore cooperation and trust by requiring that ICE target only serious criminal threats and work collaboratively with state and local governments on that mission.

Congress should not attempt to coerce states and cities into mass deportation.

Unfortunately, rather than attempting to reestablish trust, Congress is considering legislation like S. 3805, the "End Sanctuary Cities Act of 2026," that explicitly seeks to force states and localities into participation with mass deportation.[83]

- *S. 3805 is dangerously unconstitutional.* It would set a precedent that the federal government could force state and local officials to implement any policy in any area—from crime and guns to vaccines and the environment. As the Supreme Court has warned, this action would erode federalism and local accountability, turning the states into agencies of the federal government rather than sovereign states responsible to their own voters.
- *S. 3805 criminalizes officials for routine law enforcement prioritization.* Every municipality and state must decide which individuals to arrest, hold, or release. They make these decisions every day based purely on public safety concerns. S. 3805 would impose criminal penalties for failing to participate in civil immigration enforcement, even at the expense of local officials' broader public safety concerns.
- *S. 3805 imposes unfunded mandates and liabilities on states and localities.* In addition to the risk of lawsuits, S. 3805 requires state and local governments to allocate their criminal law enforcement resources to engage in deportation efforts, without providing any additional funds to states and localities to carry out the mandate. Local police already struggle to keep pace with America's high crime rates. They do not need additional burdens imposed—especially when Congress refuses to pay for those burdens.
- *S. 3805 mandates local law enforcement violate the Constitution and court orders.* S. 3805 mandates that cities give ICE at least 48 hours' notice prior to releasing anyone.

This would force law enforcement to violate court orders and settlements stating that local police cannot detain people solely based on immigration detainers, as this is an arrest without probable cause. It will also force them to ignore state and local judges who grant bail to defendants pending trial. This would cause them to violate their own laws and state constitutions. Moreover, since many US citizens have been targeted by ICE detainers, the bill would effectively mandate that US citizens be detained even if they have evidence of US citizenship.[84]

- *S. 3805 targets city officials who follow state policies over which they have no control.* Officials in every jurisdiction in at least 14 states will become targets for criminal prosecutions under S. 3805, even though they had no choice but to adopt the policies that the bill targets because state laws preempt local laws.[85]
- *S. 3805 raises the threat of criminal liability for local police for every criminal arrest that they make.* If someone they arrest turns out to be someone sought by ICE, local officials would face a choice between violating state or federal law. In this situation, agencies may choose to make fewer arrests than they otherwise would make solely based on public safety concerns.

Congress should be investigating and stopping the current constitutional violations in immigration policy, not enacting new ones. Instead, Congress should follow the advice of the Major City Chiefs Association: "The decisions related to how local law enforcement agencies allocate their resources, direct their workforce and define the duties of their employees to best serve and protect their communities must be left in the control of local governments."[86] While Congress may not always agree with state and local decisions about how to allocate their resources, their officials are better equipped to prioritize those resources than the federal government.

Immigration enforcement should focus on high-priority targets.

To restore the rule of law, maintain accountability, and restore trust with states and cities across the country, Congress should:

1. Order the administration to prioritize deportable criminal threats for removal;
2. Prohibit wearing masks and require federal agents to identify themselves;
3. Ban racial profiling and require federal agents to obtain warrants for specific individuals based on probable cause of a violation of the law prior to making arrests;
4. Reinforce current law that prohibits interrogating U.S. citizens about their status;
5. Require Border Patrol to patrol within a 10-mile radius of land borders;
6. Create a private right of action for violations of constitutional rights by federal agents;
7. Explicitly prohibit qualified immunity defenses for constitutional violations;
8. Prohibit threats or arrests in retaliation for First Amendment-protected activity;
9. Explicitly permit states and localities to investigate crimes committed by federal agents;
10. Allow states to sue the federal government for violations of their residents' rights; and
11. Permit nationwide injunctive relief by courts for policies violating federal law.

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Center on Budget and Policy Priorities
1275 First Street NE, Suite 1200
Washington, DC 20002
Tel: 202-408-1080
info@cbpp.org
www.cbpp.org

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Costly Tax Cuts Increase Our Nation's Fiscal Challenges

Testimony of Brendan Duke, Senior Director for Federal Budget Policy, Center on Budget and Policy Priorities, Before the Senate Budget Committee

Thank you, Chair Graham, Ranking Member Merkley, and members of the Budget Committee. My name is Brendan Duke, and I am Senior Director for Federal Budget Policy at the Center on Budget and Policy Priorities (CBPP). CBPP is a nonpartisan research and policy institute that advances federal and state policies to help build a nation where everyone has the resources they need to thrive and share in the nation's prosperity.

Our nation faces serious long-term fiscal challenges. Addressing them will require policymakers to recognize several realities:

- An aging population means that we will need to spend more to meet our obligations to seniors. Measured as a share of the economy (gross domestic product or GDP), revenues currently are around their average level of recent decades, but spending on Social Security and Medicare has risen as the baby-boom generation has moved into retirement. Spending outside of those two programs has fallen as a share of GDP, refuting claims that our fiscal challenge simply reflects overspending.
- The American people favor a broad range of critical public services, investments in our economy such as education, research, and infrastructure, and supports for seniors, people with disabilities, and individuals and families who need help affording food, health care, child care, and housing.
- Tax cuts enacted over the last 25 years – the Bush tax cuts, their extensions under the Obama Administration, and the 2017 Trump tax cuts – have weakened revenues, driving up deficits. In 2012 – before most of the Bush tax cuts were made permanent and the Trump tax cuts were even conceived – the Congressional Budget Office (CBO) projected that the deficit in 2025 would be 1.8 percent of GDP. The actual 2025 deficit was 5.8 percent. The difference is due to the tax cuts, as programmatic spending in 2025 was lower as a percent of GDP than CBO had projected.

- H.R. 1, the deeply flawed megabill enacted last year, doubled down on those tax-cut mistakes. It has a net cost of $3.4 trillion through 2034 despite imposing massive program cuts that will take away health care and food assistance from millions of people (CBO more recently estimated the ten-year cost *through 2035* at $4.7 trillion including its effects on the economy and additional interest payments). Independent analyses agree that it will not generate enough economic growth to meaningfully offset the cost of these tax cuts.
- The push for large-scale deportation of immigrants and closing off the U.S. to new immigrants not only harms our communities and families, but it also makes our fiscal challenges worse. Immigration can reduce the need for the politically difficult measures required to improve the fiscal outlook by expanding the workforce and increasing revenue. But CBO projects that the Administration's actions to *reduce* immigration will increase deficits by $500 billion over ten years, principally by reducing the number of taxpayers.

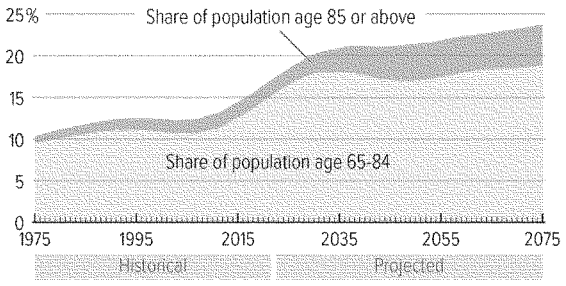
An Aging Population Will Require More Revenue

Analyses of the federal budget typically focus on government revenue and spending as percentages of GDP. In 2025, revenue was 17 percent of GDP, very similar to the average over the past 50 years and the same as in 1985. Programmatic spending, on the other hand, grew from 19.2 percent to 19.9 percent of GDP between 1985 and 2025. And net interest — the cost of servicing the net federal debt — grew from 3.0 percent to 3.2 percent of GDP over that period.

While many in Congress have concluded, "we have a spending problem, not a revenue problem," this claim ignores the reality that spending on Social Security and Medicare has gone up because the population has aged; programmatic spending outside of those two programs is actually *lower* as a percent of the economy today than in 1985.[1] Non-defense discretionary spending, for example, fell from 3.8 percent to 3.2 percent of GDP between 1985 and 2025.

FIGURE 1

An Aging Population Results in Higher Costs for Social Security and Medicare



Source: Social Security Administration

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Forty years ago, the members of the baby-boom generation (those born between 1946 and 1964) were in or still approaching their "prime working years"; today they are in their "prime retirement years," with all but the very youngest now eligible for Social Security and Medicare. And over the next 50 years, this trend of an aging population will continue. (See Figure 1.)

The combination of population aging and basic arithmetic means we face a choice: either we raise revenue above the 17 percent level that was typical when the country was younger, or we make drastic cuts to programs and renege on commitments we've made, including to seniors who receive benefits through Medicare, Social Security, or Medicaid (the country's largest payer of long-term care services). Two-thirds of federal program spending each year goes to the combination of Social Security, Medicare, long-term care in Medicaid, defense, and veterans. Without additional revenue, therefore, there is simply no way to significantly slow the rise in the federal debt while shielding these programs from cuts.

Actions by the current Congress and Administration show how damaging these program cuts would have to be – and how deeply we would have to cut to address our rising debt trajectory. The Trump Administration has attempted — often illegally — to close whole agencies, engaged in mass layoffs, and at various times tried to freeze or impound various funds, but with little savings to show for it. Similarly, congressional Republicans failed to offset the full cost of the trillions of dollars in tax cuts they enacted last year despite imposing a 22 percent cut to the Supplemental Nutrition Assistance Program (SNAP) and 18 percent cut to Medicaid by 2034, and those program cuts have sparked substantial and warranted public pushback. And even the Trump Administration says it opposes cuts to Social Security and Medicare benefits. Indeed, large bipartisan majorities in Congress *increased* Social Security benefits one year ago by repealing Social Security's windfall elimination provision and government pension offset without even attempting to offset the cost.[2]

Meeting long-standing retirement and health care commitments to seniors, managing the future risks associated with higher debt, and making critical national investments will, therefore, require substantially more revenue. Trump Administration actions once again help prove this. The deficit fell from 6.3 percent to 5.8 percent of GDP between fiscal years 2024 and 2025, in large part due to about $120 billion in additional revenue from tariffs. (Last month the Supreme Court ruled a large fraction of these tariffs are illegal.) These tariffs — while regressive and economically destructive — nonetheless reflect the reality that revenue needs to be part of the fiscal discussion.

Without 25 Years of Unpaid-for Tax Cuts, Our Debt Today Would Be Lower and Stable

The retirement of the baby-boom generation was both predictable and predicted. And at the beginning of the 21st century, our fiscal system was projected to deliver adequate revenue to meet our needs because revenue was projected to rise above 17 percent of GDP. But that was before policymakers enacted a series of enormously costly tax cuts that dramatically changed the fiscal trajectory.

One way to see this is to compare current budget data with CBO projections from 2012 – the year before more than four-fifths of the Bush tax cuts were made permanent and before the 2017 Trump tax cuts were proposed and then enacted. CBO's 2012 projections for 2025 assumed the Bush tax cuts would expire as scheduled and the 2017 tax cuts would never happen.

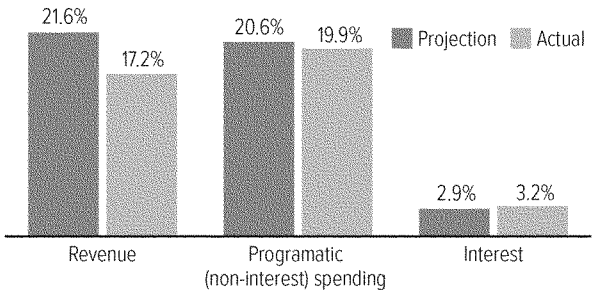
In 2012 CBO projected that the fiscal year 2025 deficit would be 1.8 percent of GDP – low enough that economic growth could reasonably be expected to keep the ratio of debt to GDP from growing endlessly from that point forward. But the 2025 deficit was 5.8 percent of GDP. The reason is simple: despite rising costs due to the aging of the baby-boom generation, policymakers have enacted unpaid-for tax cuts in the past two decades that have eroded the revenue base.

The deficit increase was *not* because of higher programmatic spending. In 2012, CBO projected that programmatic spending would make up 20.6 percent of GDP in 2025; the actual 2025 level was 19.9 percent, in part because health care cost growth slowed as compared to the 2012 CBO estimates. The higher non-interest deficit is entirely attributable to the drop in revenue, from CBO's projection of 21.6 percent of GDP to the actual 2025 level of 17.2 percent, mostly as a result of the permanent extension of the Bush tax cuts and enactment of the 2017 Trump tax cuts.[3] (See Figure 2.)

FIGURE 2

Permanent Tax Cuts Lifted 2025 Deficits Well Above 2012 Projections

Actual numbers vs. 2012 projections – before Trump tax cuts, permanent Bush cuts – as a share of GDP



Source: 2012 Congressional Budget Office (CBO) projection of 2025 and CBO Economic and Budget Update, February 11, 2026. "Programatic spending" refers to spending excluding interest payments.

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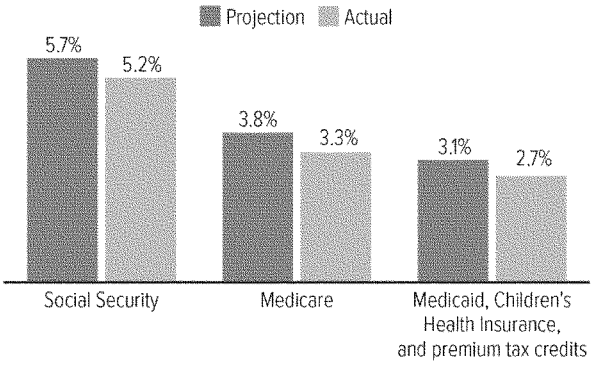
Importantly, 2025 spending for key individual programs – such as Social Security, Medicare, Medicaid, the Children's Health Insurance Program (CHIP), and the Affordable Care Act's premium tax credits – also came in below CBO's 2012 projections. This shows that it's possible to meet our commitments to our seniors while ensuring millions of low- and moderate-income Americans have health care if we are willing to raise the revenue to do so. Notably, the 2025 costs of Medicaid, CHIP, and the premium tax credits were below the 2012 projections despite the subsequent enactment of premium tax credit enhancements, which unfortunately expired at the end of last year. (See Figure 3.)

FIGURE 3

Spending on Social Security, Medicare, and Other Health Care Programs in 2025 Was Below Projections a Decade Earlier

Actual numbers vs. 2012 projections - before Trump tax cuts, permanent Bush cuts - as a share of GDP



Source: 2012 Congressional Budget Office (CBO) projection of 2025 and CBO Economic and Budget Update, February 11, 2026.

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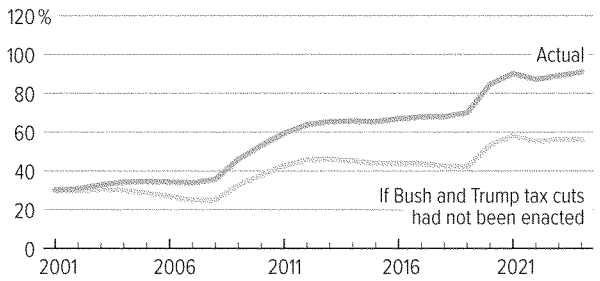
Switching from deficits to debt, we can directly see the burden that decades of unpaid-for tax cuts have placed on our fiscal system. My CBPP colleagues estimate that if the Bush tax cuts, their extensions under President Obama, and the 2017 Trump tax cuts had not been enacted, the debt ratio ("debt net of financial assets") would be considerably lower: 56 percent of GDP in 2024, compared to the actual 92 percent.[4] (See Figure 4.)

FIGURE 4

The Debt Is Higher Due to the Bush and Trump Tax Cuts

Debt as a percentage of GDP



Note: Debt refers to "net debt," which is the government's total debt net of its financial assets.
Source: CBPP analysis of data from CBO and JCT

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Finally, our fiscal deficit is not our only deficit – we also have a public investment deficit. We are underinvesting in the building blocks of economic growth, including science and research, clean energy transition, and the investments in children and families that reduce hardship in the near term while also increasing future productivity and prosperity.

Stabilizing our fiscal trajectory, let alone making critical investments, will require reversing the revenue losses from the Bush and Trump tax cuts. There are myriad policies that can do this. It does not mean reversing every individual tax cut enacted over the last 25 years; for example, the tax increases could be more progressive than the tax cuts. The solution, while mathematically simple, is politically difficult. But it's substantially less politically difficult than walking away from our commitments to the millions of people who count on Medicare and Social Security, slashing spending for defense and veterans, and taking food assistance and health coverage away from people who need help affording the basics.

Deeply Flawed Republican Megabill Worsens Fiscal Outlook While Increasing Hardship

Congress missed an opportunity to learn this lesson in the major tax and spending legislation it passed last year. Instead, it doubled down on the mistakes of the Bush tax cuts, their deficit-financed Obama-era extensions, and the first round of Trump tax cuts; Congress also added more than $170 billion for immigration detention and border enforcement, bolstering a large detention and removal apparatus that is separating families and violating the legal and constitutional rights of many immigrants and U.S. citizens who get swept up by the deportation dragnet.

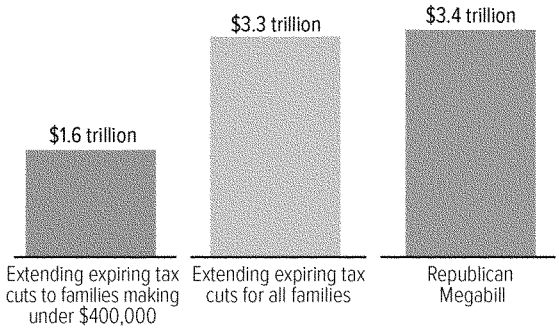
As a result, H.R. 1 will carry a net cost of $3.4 trillion through 2034 despite its cuts to health care and food assistance, according to CBO (CBO more recently estimated the ten-year cost through 2035 as $4.7 trillion including its effects on the economy and additional interest payments). Even worse, this figure could rise to $4.2 trillion if the temporary new tax cuts in the law, currently set to expire in 2028 and 2029, are extended.

If Congress instead had simply extended the 2017 tax law's expiring provisions for families, without any cuts to Medicaid and SNAP or other offsets, the resulting bill would have cost slightly less than H.R. 1. And extending the 2017 tax cuts for the 98 percent of households making under $400,000 would have cost *far* less. (See Figure 5.)

FIGURE 5

Extending Expiring Tax Cuts for Most Households Would Have Been Less Expensive Than Megabill Despite Deep Cuts to Health and Food Assistance

FY 2025-2034 cost



Note: $1.6 trillion is a Treasury estimate, the others are from the Joint Committee on Taxation and the Congressional Budget Office. Treasury estimates are generally higher than JCT and CBO estimates.

Source: Department of Treasury Office of Tax Analysis, "The Cost and Distribution of Extending Expiring Provisions of the Tax Cuts and Jobs Act of 2017"; JCT memo on revenue effects of extending the temporary 2017 tax law provisions (April 3, 2025); CBO Pub. 61570.

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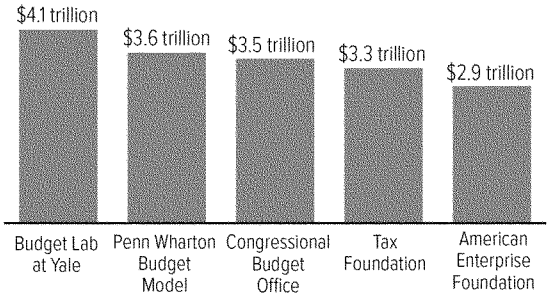
No independent organization — including right-leaning ones such as the Tax Foundation and the American Enterprise Institute[5] — has found that the megabill will generate enough economic growth to meaningfully offset the cost of these tax cuts. Indeed, some analyses, such as those from CBO, the Budget Lab at Yale and the Penn Wharton Budget Model, find that accounting for economic growth effects would *increase* its price tag.[6] (See Figure 6.)

FIGURE 6

Independent Analyses Show Megabill Adds Trillions to Deficits, Even Accounting for Growth

Dynamic estimates of the 2025-2034 cost of the megabill



Note: Budget Lab figure is CBPP calculation of Budget Lab percent-of-GDP estimate using January 2025 CBO GDP projections. Tax Foundation number is adjusted to exclude interest costs from new borrowing.

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Tariffs, another key piece of the Administration's agenda, have brought in revenues, and while the Administration may have to rebate a large fraction of them in light of last month's Supreme Court ruling, the President said he plans to seek to recreate those tariffs using different authorities. But they amount to a regressive tax increase on consumers, raising prices on everything from food to baby strollers to manufacturing inputs such as industrial chemicals. Many of those most affected by the tariffs also face the loss of health care and food assistance and were left out of the Child Tax Credit expansion in the megabill.

The net effect of the megabill's tax cuts and the tariffs is a big windfall for the wealthy. The Budget Lab – using calculations based on the tariff regime as it stood before last month – estimates that households with incomes in the top 10 percent will enjoy an income increase averaging $9,700 (1.9 percent), while households with incomes in the bottom 70 percent will end up worse off and those with incomes in the bottom 10 percent will lose an average of $2,200 (5.6 percent).

Immigrants Strengthen Economy, Improve Fiscal Outlook

The steps required to significantly improve the fiscal outlook, whether through revenue increases or program cuts, can be politically challenging. Immigration can reduce the need for difficult measures (though not eliminate them entirely), but the Administration is moving in the opposite direction.

People who are immigrants contribute to our communities and the U.S. economy in many ways. They work at high rates and make up more than a third of the workforce in some industries.[7] Their geographic mobility helps local economies respond to worker shortages, smoothing out bumps that could otherwise weaken the economy. Immigrants are entrepreneurial, comprising a disproportionate share of startups.[8] And children born to immigrant families are upwardly mobile, promising future benefits not only to their families and communities but to the U.S. economy overall. People who are immigrants do important jobs – from care workers who tend to children and seniors to construction workers who build homes to store owners to Nobel-winning scientists who forge a path of discovery and innovation – and enrich our neighborhoods and communities.

Immigrants are younger than the U.S. population overall and they boost our national birth rate; in other contexts many conservatives have embraced a higher birth rate as a policy goal.

A low birth rate can lead to a decline in the labor force and a slowing and less dynamic economy. Immigrants can counteract these effects. Moreover, the decline in the native-born birth rate, combined with the aging of the baby-boom generation, means that immigrants can boost our workforce, growing our economy and helping to support the baby-boom generation in their retirement years. A recent Cato Institute report found that immigrants have been essential contributors to federal, state, and local budgets, paying more in taxes for each year from 1994 to 2023 than they received in benefits. Over that period, in fact, immigrants created a cumulative fiscal surplus of *$14.5 trillion* in 2024 dollars.[9]

CBO's recent "Budget and Economic Outlook" demonstrates the economic and fiscal costs of the Administration's anti-immigrant policies.[10] CBO cut in *half* its projection of the annual growth rate of the U.S. population over the next ten years, relative to last year's projection. CBO now estimates that the 2035 working-age population will be 2.4 million people smaller than in its previous projection. CBO also projects that slower population growth will reduce capital investment. This has important fiscal costs, too — CBO projects that the Administration's actions to reduce immigration will increase deficits by $500 billion over ten years, principally by reducing the number of taxpayers.

Those higher deficits will make deficit reduction that much harder, adding to the damage from H.R. 1.

Financing income tax cuts tilted to the wealthy by cutting programs to help families afford food and medicine while raising taxes on imported food and medicine, even as we discourage immigration and disparage immigrants — the approach this Administration has relied on – is no way to address our fiscal imbalances. We need a better approach, and revenues need to be a large part of the solution.

---

[1] Program spending is outlays excluding net interest. Spending on net interest is ultimately the result of both Congress's program spending and revenue decisions since the beginning of the Republic, i.e., the resulting level of net debt, in addition to the market forces that determine interest rates.

[2] Kathleen Romig, "Repealing Social Security's WEP and GPO Rules Would Be Misguided," CBPP, September 19, 2022, https://www.cbpp.org/research/social-security/repealing-social-securitys-wep-and-gpo-rules-would-be-misguided.

[3] CBO's 2012 projection assumed the Alternative Minimum Tax (AMT) would continue not to be indexed for inflation and would thus cause the revenue-to-GDP ratio to rise. (Later that year it was indexed to inflation as part of the legislative package that made most of the Bush tax cuts permanent.) But the legislation making most of the Bush tax cuts permanent still significantly reduced revenue, even setting aside its changes to the AMT. The analysis of the debt trajectory below — which separates out the effect of adjusting the AMT for inflation and the Bush tax cut extension — still shows that the debt-to-GDP ratio would be approximately stable without those tax cuts but with the AMT nonetheless adjusted for inflation. And it is still the case that program costs in 2025 are lower than CBO had projected in 2012; the increased deficits are still entirely the result of tax cuts.

[4] As noted above, this analysis assumes that Congress already "patched" the AMT by permanently adjusting its parameters for inflation, so the cost of such a patch does not count as part of the Bush tax cuts.

[5] Kyle Pomerleau, "A Preliminary Macroeconomic Analysis of the 'One Big Beautiful Bill Act,'" American Enterprise Institute, June 12, 2025, https://www.aei.org/articles/a-preliminary-macroeconomic-analysis-of-the-one-big-beautiful-bill-act/; Garrett Watson *et al.*, "One Big Beautiful Bill Act Tax Policies: Details and Analysis," Tax Foundation, February 10, 2026, https://taxfoundation.org/research/all/federal/big-beautiful-bill-senate-gop-tax-plan/.

[6] Budget Lab, "Long-term Impacts of the One Big Beautiful Bill Act, as Enacted on July 4, 2025," July 30, 2025, https://budgetlab.yale.edu/research/long-term-impacts-one-big-beautiful-bill-act-enacted-july-4-2025; Penn Wharton Budget Model, "President Trump-Signed Reconciliation Bill: Budget, Economic, and Distributional Effects," July 8, 2025, https://budgetmodel.wharton.upenn.edu/issues/2025/7/8/president-trump-signed-reconciliation-bill-budget-economic-and-distributional-effects.

[7] Arloc Sherman, "Immigrants Contribute Greatly to U.S. Economy, Despite Administration's 'Public Charge' Rule Rationale," CBPP, August 15, 2019, https://www.cbpp.org/research/immigrants-contribute-greatly-to-us-economy-despite-administrations-public-charge-rule.

[8] Pierre Azoulay *et al.*, "Immigration and Entrepreneurship in the United States," American Economic Review: Insights, March 2022; Saheel A. Chodavadia *et al.*, "Immigrant Entrepreneurship: New Estimates and a Research Agenda," National Bureau of Economic Research Working Paper No. 32400, May 2024, https://www.nber.org/papers/w32400.

[9] David J. Bier, Michael Howard, and Julian Salazar, "Immigrants' Recent Effects on Government Budgets: 1994-2023," Cato Institute, February 3, 2026, https://www.cato.org/white-paper/immigrants-recent-effects-government-budgets-1994-2023.

[10] CBO, "The Budget and Economic Outlook: 2026 to 2036," February 11, 2026, https://www.cbo.gov/publication/61882.

Questions for the Record
From Senator Moreno
For Mr. David J. Bier
Sanctuary Cities: The Cost of Undermining Law and Order
March 10, 2026
Senate Budget Committee

Mr. Bier, please be advised that you are under a legal obligation to provide truthful statements to Congress under 18 U.S. Code § 1001 and were sworn in under oath at our recent hearing. Has the Cato Institute, your employer, ever received, directly or indirectly, any form of financial support (donations, grants, etc.) from transnational, Latin American, or Central American narcotrafficking or gang-affiliated organizations?

No. And to be clear, the policies that prevent people from coming legally are most responsible for the way in which illegal immigration has benefited these criminal organizations. Legal immigration would remove immigration from these organizations' control and reassert appropriate governmental vetting over the immigrant flow to the United States.

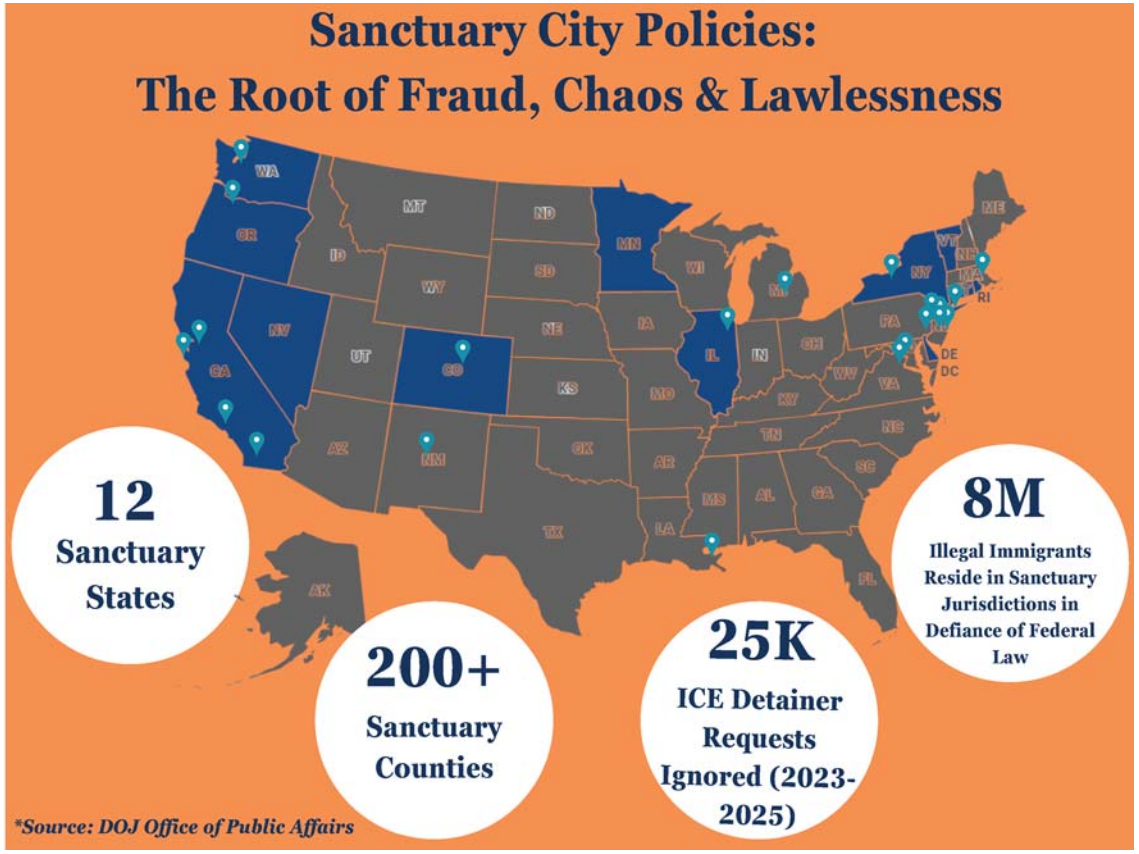
Questions for the Record
From Senator Moreno
For Mr. Brendan Duke
Sanctuary Cities: The Cost of Undermining Law and Order
March 10, 2026
Senate Budget Committee

Questions for the record submitted to Brendan Duke, Senior Director, Federal Budget, Center on Budget and Policy Priorities

Submission of The Honorable Bernie Moreno, U.S. Senator from Ohio

QUESTION: Mr. Duke, please be advised that you are under a legal obligation to provide truthful statements to Congress under 18 U.S. Code § 1001 and were sworn in under oath at our recent hearing. Has the Center on Budget and Policy Priorities, your employer, ever received, directly or indirectly, any form of financial support (donations, grants, etc.) from transnational, Latin American, or Central American narcotrafficking or gang-affiliated organizations?

ANSWER: No, based on information provided by Center on Budget and Policy Priorities President Sharon Parrott.


Sanctuary City Policies:
The Root of Fraud, Chaos & Lawlessness
12
Sanctuary States
200+
Sanctuary Counties
25K
ICE Detainer Requests Ignored (2023-2025)
8M
Illegal Immigrants Reside in Sanctuary Jurisdictions in Defiance of Federal Law
*Source: DOJ Office of Public Affairs

FOX5
WASHINGTON DC
Fairfax police warned prosecutors about repeat offender months before deadly bus stop stabbing
Abdul Jalloh
Illegal Immigrant from Sierra Leone
Stephanie Nicole Minter
41-year-old Mother from Virginia

Abdul Jalloh

2012: Jalloh enters the U.S. illegally

Racks up over 30 arrests including: rape, malicious wounding and assault

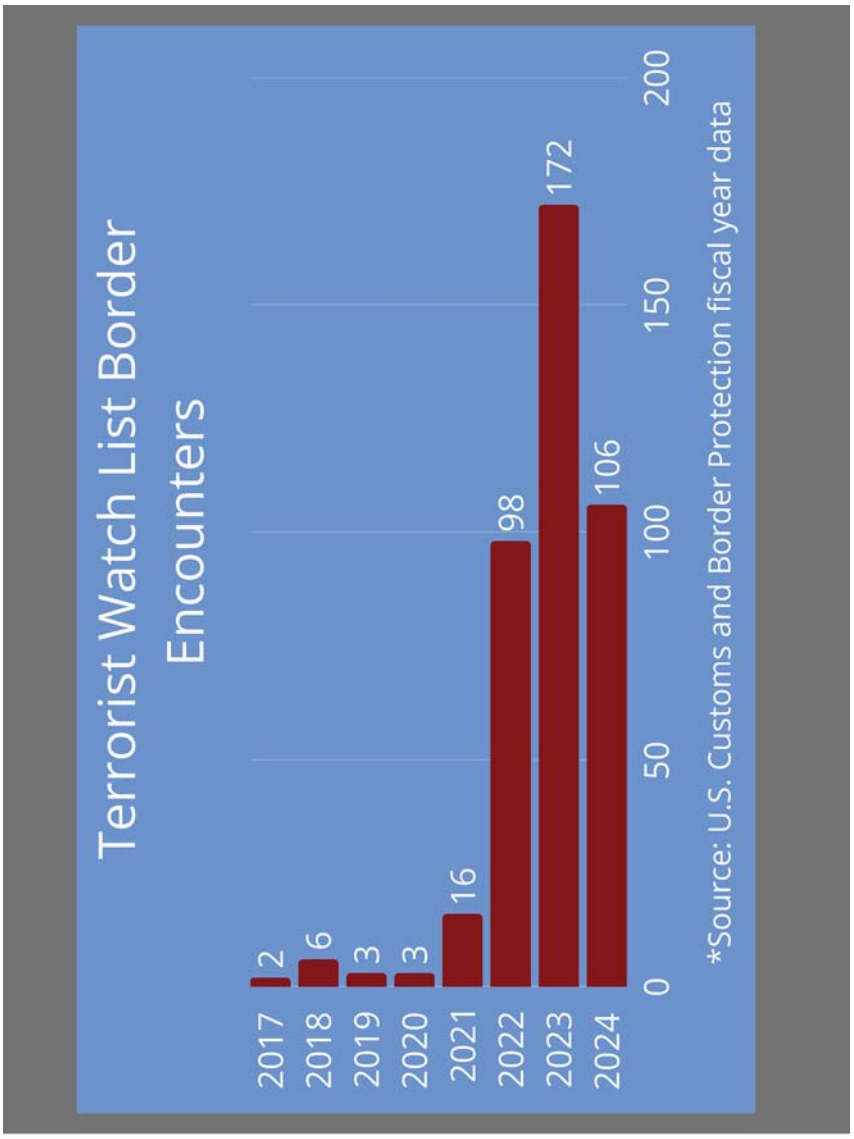
2020: ICE submits a formal detainer to deport Jalloh. The request is blocked by Virginia's Attorney for Fairfax County Steve Descano, a Soros-backed prosecutor and defender of sanctuary cities, who ignores it.

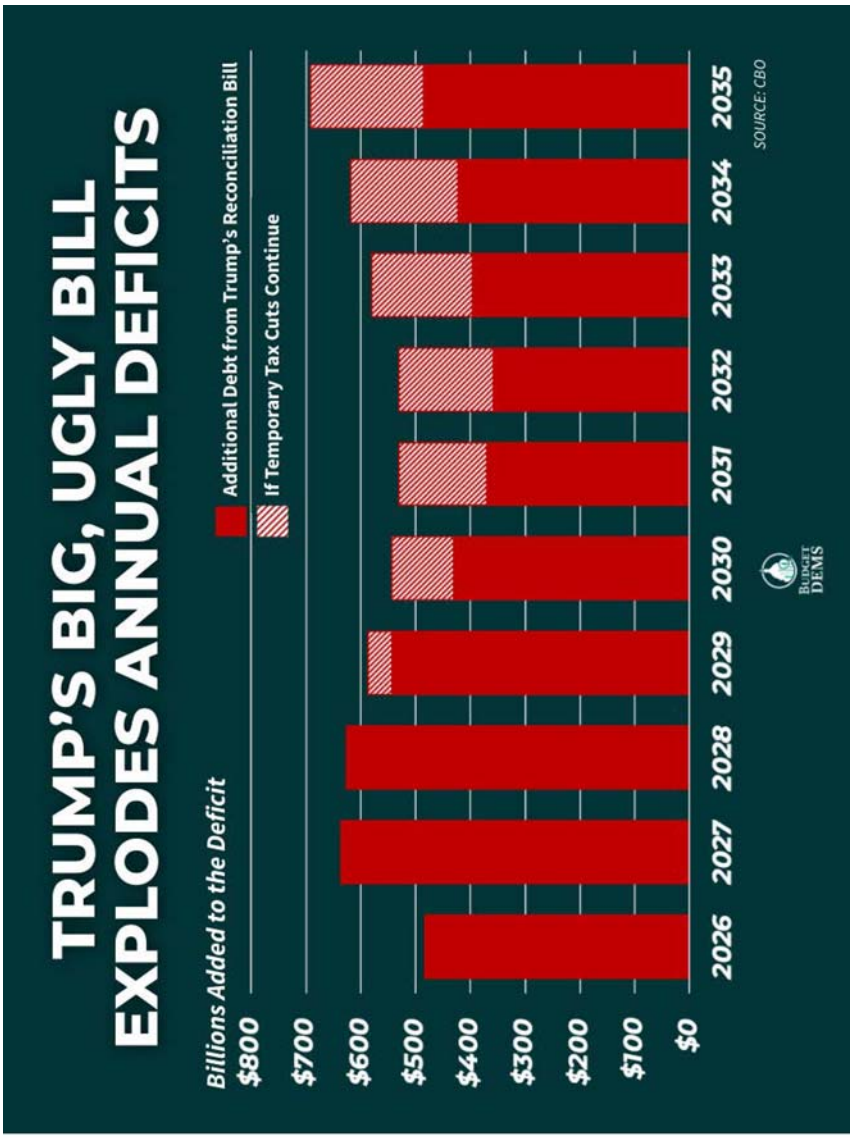
May 2025: Jalloh arrested once more for stabbing an individual in the leg, but Descano releases him again.

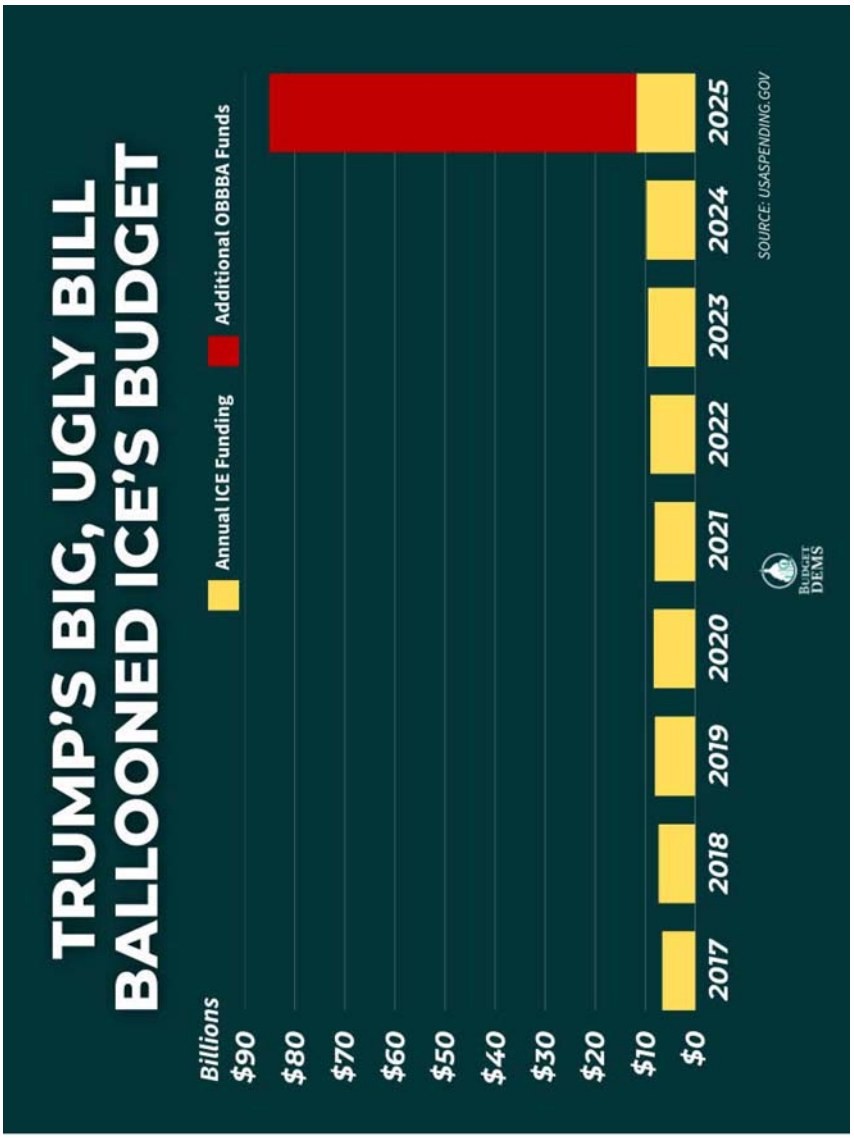
February 2026: Jalloh brutally murders Stephanie Nicole Minter.

THE TRUE COST OF
SANCTUARY CITIES
ENDANGER PUBLIC SAFETY
10,000+ criminals released by sanctuary policies who went on to be arrested for additional crimes
CREATE MAGNETS FOR FRAUD
Over $9 billion in fraud stolen by Somali immigrants in Minnesota
ENTICE ILLEGALS WITH HANDOUTS
59% of illegal-immigrant households on government welfare programs costing taxpayers $42 billion a year

ENDING SANCTUARY CITIES ACT OF 2026
Restores Law & Order By Stopping the Obstruction of Federal Immigration Enforcement
Imposes Criminal Penalties on State & Local Officials That Ignore or Prohibit Compliance with DHS Detainers
Ends the Cycle of Releasing Illegal Immigrants Charged with Crimes Back into Communities
Subjects Violators to 25 Years in Prison for Releasing Illegal Aliens Charged with or Convicted of Murder, Rape, or Sex Offenses


Terrorist Watch List Border Encounters
2017
2018
2019
2020
2021
2022
2023
2024
2
6
3
3
16
98
172
106
0
50
100
150
200
*Source: U.S. Customs and Border Protection fiscal year data

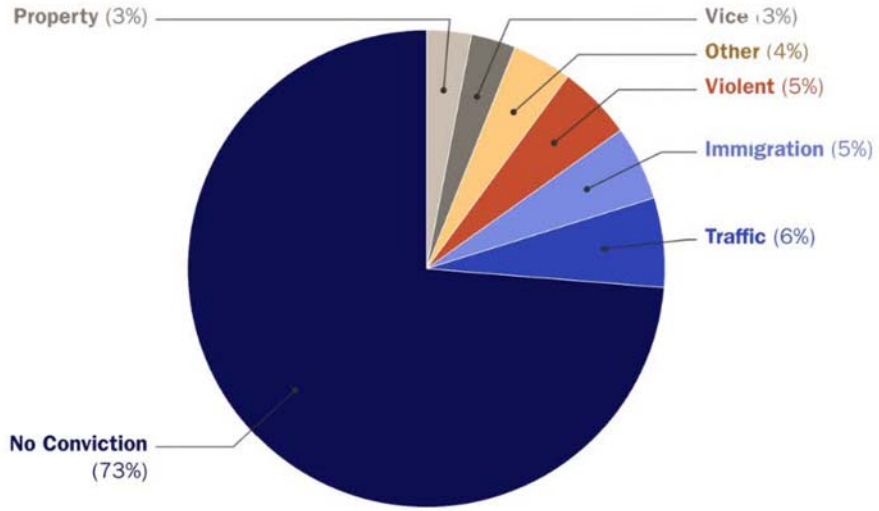

TRUMP'S BIG, UGLY BILL EXPLODES ANNUAL DEFICITS
Billions Added to the Deficit
Additional Debt from Trump's Reconciliation Bill
If Temporary Tax Cuts Continue
$800
$700
$600
$500
$400
$300
$200
$100
$0
2026
2027
2028
2029
2030
2031
2032
2033
2034
2035
SOURCE: CBO
BUDGET DEMS


TRUMP'S BIG, UGLY BILL
BALLOONED ICE'S BUDGET
Billions
Annual ICE Funding
Additional OBBBA Funds
$90
$80
$70
$60
$50
$40
$30
$20
$10
$0
2017
2018
2019
2020
2021
2022
2023
2024
2025
SOURCE: USASPENDING.GOV
BUDGET DEMS

CREDIT: CNN
RUBEN RAY MARTINEZ
CREDIT: FACEBOOK
RENEE NICOLE GOOD
CREDIT: VA OFFICIAL PORTRAIT
ALEX PRETTI

ONLY 5% OF INDIVIDUALS BOOKED INTO ICE DETENTION EVER HAD A VIOLENT CRIMINAL CONVICTION

ICE book-ins into custody, by most serious conviction, FY 2026, Oct 1 - Nov 15, 2025



Immigration and Customs Enforcement, "ICE Initial Book-Ins by Criminality and MSC: FY2025 November 15, 2025. See Appendix Table A for classifications.

not just during the period listed.

CATO INSTITUTE

SENATE BUDGET COMMITTEE

SANCTUARY CITIES: THE COST OF UNDERMINING LAW AND ORDER

March 10, 2026

WRITTEN TESTIMONY OF DONALD ROSENBERG
PRESIDENT, ADVOCATES FOR VICTIMS OF ILLEGAL ALIEN CRIME
FATHER, DREW ROSENBERG JANUARY 9, 1985 – NOVEMBER 16, 2010

Thank you for the opportunity to testify before the Senate Budget Committee hearing, Sanctuary Cities: The Cost of Undermining Law and Order."

For over three decades, the United States has operated under two immigration systems. One, the Immigration Reform and Control Act (IRCA), enacted by the United States Congress in 1986; the other, the nullification of federal law and the establishment of sanctuary policies and laws by cities, counties, and states.

Despite the bipartisan 1986 IRCA, which resulted in amnesty for 4 million aliens who had entered the country illegally, labeled as a one-time fix, the enforcement part was largely ignored.

Leaders of the Democratic Party, Presidents Clinton, Obama, and Biden (back when he was a Senator), and Senator Schumer spoke out forcefully against illegal immigration as recently as 2010. During the Clinton and Obama terms, millions were deported (although mostly returned from the border), millions also illegally crossed the border, and millions ignored the end of their visas and just remained. The result by 2016 was estimates of 11-15 million (many thought more) illegal aliens residing in the United States.

Donald Trump's promise to seal the border and remove all illegal aliens during the 2016 presidential campaign resulted in an upset win over Hillary Clinton. It further caused an increase in sanctuary laws and policies being enacted in almost exclusively Democratic enclaves. Additionally, an already compromised large segment of the media completely abrogated its journalistic responsibilities when it came to reporting on immigration. Along with the Democratic Party and illegal immigration advocacy groups, they convinced many in the public that illegal aliens were the victims and that the real victims of illegal alien crime were racists, bigots, and white supremacists.

I recommend Ann Coulter's recent article, "How Did They Think Open Borders Would End?"

In July 2023, I testified at the House Judiciary Subcommittee on Immigration Integrity, Security, and Enforcement. The hearing was titled "The Consequences of Criminal Aliens on U.S. Communities." While the efforts of the new administration, ICE, and CBP, over the past year, have shown progress, the efforts

of sanctuary cities, the Democratic party, illegal immigration advocates, and a mostly complicit media have done everything possible to thwart efforts to remove those who have violated our immigration laws. What happened to no one is above the law?

My son, Drew, was killed fifteen years ago on his way home from law school by a criminal illegal alien, Roberto Galo. The illegal alien who ran him over drove back and forth over his body three times, attempting to flee. The third time, he stopped when a man stood in front of his car. His rear tire was resting on my son's abdomen. Five men had to lift the car off his lifeless body. Surely that is a consequence of illegal immigration and San Francisco's sanctuary city policy.

Almost a year to the day before Drew was killed, San Francisco Mayor Gavin Newsom announced an illegal policy that allowed illegal aliens to drive without a license. In June 2010, Galo was caught driving without a license, no insurance, and driving the wrong way on a one-way street.

Because he couldn't find a licensed driver to drive his car, he was arrested, and the vehicle was impounded. By law, a car driven by an unlicensed driver shall remain impounded for a minimum of 30 days. Police Chief George Gascon released the car, 18 hours after impoundment, and Galo was back on the road. A few weeks later, District Attorney Kamala Harris dropped the charges. A few months later, Drew was dead.

For those who diminish the crime of driving without a license, at the time, over 7,000 people a year were killed by unlicensed drivers.

For the past fifteen years, I have been fighting the scourge of illegal immigration. In 2017, along with other victims of illegal alien crime, we established Advocates for Victims of Illegal Alien Crime (AVIAC).

The obvious question is, why would there be a need for such an organization when illegal aliens have no right to be in the country?

The answer is that for decades, the Democratic Party, and yes, some Republicans, and your predecessors have failed the American people. No, that's wrong. You betrayed the American people, resulting in the deaths of tens of thousands of American citizens, hundreds of thousands of rapes and sexual molestations

against women and children, and literally millions of crimes committed by people who have no right to be here. (Source: 2018 GAO Criminal Alien Statistics)

The chaos that ensued in Minneapolis and other sanctuary cities since President Trump began enforcing our immigration laws -what a novel concept – was a direct result of those cities attempting to nullify federal law.

The tragic deaths of Renee Good and Alex Pretti would never have happened had Minneapolis at the very least recognized that no one is above the law, and those who entered our country illegally or overstayed their visa by law must leave or be removed.

Mayor Jacob Frey and Governor Tim Walz, with help from most of the legacy media, convinced Good, Peretti, and many others that the illegal aliens were the victims. They need to be protected.

For years, advocates for illegal aliens, the entire Democratic Party (which I was a member of for almost fifty years), the legacy media, and the establishment of sanctuary cities/states legitimized illegal aliens.

To sanitize their crime, 8 USC 1325, illegal aliens became illegal immigrants, then undocumented immigrants, then unauthorized immigrants (sometimes unauthorized Americans), immigrants, migrants, and now “your neighbors”.

To be fair, not all illegal aliens are killers, rapists, and violent criminals. Many work hard and are very nice people. However, that doesn’t excuse their having committed a serious crime. That isn’t a reason to allow them to remain in the United States.

Law-abiding American citizens should not be the guinea pigs to feed your political ambitions.

Illegal aliens are “accessories before the act”. Politicians, advocates, and the media cite them as the reason for sanctuary laws, driver's license privileges, health care, and other benefits, which have resulted in massive crime, a tremendous increase in traffic fatalities, and financial burdens on taxpaying citizens and legal permanent residents.

Good's death, as it should have been, was covered by the legacy media, enflaming the public about how cruel the administration's immigration policies were. Of course, other than Fox, The New York Post, and a few other media outlets, the legacy media never cover the carnage committed against US citizens and legal permanent residents (LPRs). If you want to see cruel go to www.dhs.gov.

After seeing online postings claiming that "undocumented immigrants" were only responsible for killing four people over the past ten years, I created a list of a few Americans killed by illegal aliens in the days surrounding Ms. Good's death.

- **Arya Cruz Acencio – 8** Killed by a twice DUI convicted illegal alien protected by CA sanctuary law, who killed her while driving drunk. Her father also had his leg amputated and has serious head injuries.
- **Aiden Antonio Torres De Paz – 11** Killed by an illegal alien on Thanksgiving morning. The killer had been deported 4 times. California denied the ICE detainer

Arya and Aiden were killed days apart in San Diego

- **Bravilio Galeano Ayala – 16 & Samir Canales Molina – 18** Both shot to death by an illegal alien in Charlotte.
- **Amber Paris – 15** Killed in a hit and run by an illegal alien in Charlotte. The killer was released on bond, and the driving without a license charge was dismissed.
- **William Carter (25) and Jennefer Lower (24)** - They were newlyweds married for 16 days in Oregon by an illegal alien who had been granted a CDL license from California.
- **Fletcher Harris (20) and Skylar Provenza (19) K**illed by an illegal alien driving drunk in Rowan County, NC

These cases are just the tip, of the tip of the iceberg. None of these stories was covered in the legacy media.

Meanwhile, here's a short sample of a few of the thousands of illegal aliens, with criminal records, arrested in Minneapolis/St Paul: again with no national or local news coverage.

- **Sahal Osman Shidane –** Criminal sexual conduct of a victim 13-15
- **Oudone Lothirath –** Assault, Aggravated Assault – Gun
- **Mong Cheng** – Homicide and other crimes
- **Phuc Trong Nguyen** – Rape and other crimes
- **Francisco Jorge-Salgado** – Assault, Drug Sales, Possession of a Weapon, Probation Violation
- **Pedro Ornelas** – Rape, Sexual Assault
- **Mariama Kanu** – Homicide, Burglary

- **Abdulkadir Sharif Abdi** – Fraud, Probation Violation, Vehicle Theft
- **Abdi Gelle Mohamed** – Sexual Abuse of a Minor
- **Heydi Aguilera Bustillo** – Child Neglect, Aiding Prisoner Escape

Maybe if Good, Pretti, and the other protestors had been aware that these were the "neighbors" Mayor Frye was referencing, they wouldn't have been out on the street "peacefully protesting."

Look at other sanctuary cities, Boston, Chicago, and Los Angeles, and you'll see similar numbers (adjusted for population). Mayors Wu, Johnson, and Bass, are we to believe that removing these illegal aliens made your cities less safe? Will more crimes be committed because these people have been deported?

When confronted, we often hear support for deporting these people, followed by, "but most of the people you're deporting have no criminal record." Although every illegal alien has either committed a criminal act by coming here or violated the provisions of their visa, the people ICE has detained (listed above) at one time fit that description as well. Do we have to wait until they kill or rape someone to do what the law requires? Regardless, it's too late at that point.

We constantly hear that families are torn apart and babies are ripped from their mothers. But what about my family? What about the families that have been destroyed by illegal alien crime?

Nothing is stopping these families from all departing the United States. Never discussed, but all the children born in the United States to illegal alien parents and incorrectly granted U.S. citizenship are also citizens of their parents' country of origin.

If they choose not to leave, they can visit their parents, FaceTime them, speak to them by phone, email, or text. Those of us who have suffered the ultimate loss? We can stand over our loved one's grave or longingly stare at the urn of ashes sitting on the mantle. Excuse me if I have little sympathy for those who, by breaking our laws, are inconvenienced.

In my over fifteen years of advocacy for the victims of illegal alien crime, I have never heard one Democratic president, senator, house member, governor, or mayor voluntarily mention the death of a US citizen at the hands of an illegal alien. Yet, they are constantly shedding tears for illegal aliens.

We are reminded constantly that Renee Good had three children. Rachel Moran, who was out jogging, not antagonizing and impeding ICE agents from doing their job, had five children and was raped and murdered by an illegal alien.

Alex Pretti was a nurse who, the week before his death, was cursing ICE agents and then kicked their car twice, smashing the taillight. He also impeded ICE from doing its job. Laken Riley was a nursing student out for a jog when she was raped and murdered by an illegal alien.

Liam Conejo Ramos, 5, was abandoned by his father and was taken to McDonald's by ICE agents before reuniting with his dad and being detained by ICE for a few days. Twelve-year-old Jocelyn Nungary was strangled to death and her body thrown into a creek by two illegal aliens.

At President Trump's March 5, 2025, address to a joint session of Congress, he recognized the moms in attendance, and no Democrat had the common decency to stand.

But it's not just crime that's at issue.

Despite the canard that illegal aliens contribute more than they take, that's just not remotely true. Yes, some pay taxes, as do tourists, but what they pay is small compared to the cost they incur. They aren't paid well, and many, because they are vulnerable, don't even receive minimum wage.

The costs to educate their children K-12 range from $15,000 a year to $25,000 annually in California and over $30,000 in New York.

Fourteen states and Washington DC offer some form of health coverage to illegal aliens. Some, regardless of age. New York and Illinois, just for those 65 and over (the most expensive).

It is estimated that the benefits to their children who are citizens amount to between $20 and $30 billion annually.

What is the cost to the victims of the crimes they commit? What was my son's life worth? What is a rape or child molestation worth?

How do we even calculate the increase in housing costs when multiple families crowd into a house or apartment they can afford, because there are multiple wage

earners, but US citizens can't? Additionally, most of the larger cities (all sanctuaries) that have high populations of illegal aliens also have severe housing shortages, which drive up the cost of low-income housing.

Many schools are severely overcrowded, and the classes are filled with children who speak many different languages. That doesn't just add a cost for interpreters but makes for a very difficult educational experience for all the children.

Our inner-city emergency rooms are perpetually overcrowded, and although not everyone there is here illegally, you'll rarely hear English being spoken.

So, what's the solution?

Many Democrats want to abolish ICE or severely limit its ability to enforce the law. No surprise, as their idea of immigration reform is amnesty for all. It's clear that sanctuary states and cities are meant to protect criminals. They make things worse for illegal aliens who haven't committed additional crimes.

Knowing that they had little power to accomplish anything legislatively, and the fact that most Americans wanted all illegal aliens removed, their solution was to create chaos.

ICE agents never wore masks until protestors forced them to by making threats against their families. An ICE agent who's a good friend told me he often can't drive directly home because he's afraid he's being followed.

The "mostly peaceful protestors" impede ICE agents from doing their job. The interference they create does reduce the number of illegal aliens they can detain.

Sanctuary cities increase the number of ICE agents needed to process an illegal alien for deportation.

Anything to delay.

What happens if we abolish ICE or severely limit its ability to enforce the law?

Murders, killings, rapes, child molestation, and all other crimes will increase. The taxpayer expense to maintain an illegal alien population of 20 million or more, and additional births of over 300,000, will increase the burden on U.S. taxpayers.

What happens if we remove all illegal aliens?

All crimes they commit, stop. All the money we spend supporting them could be used to help American citizens or to start paying down our debt. People from all over the world who have waited patiently for years to legally immigrate to America will now actually have a chance. And lastly, many, if not most, ICE agents will lose their jobs as they are no longer needed.

Instead of abolishing ICE, let's abolish sanctuary cities.

SENATE BUDGET COMMITTEE

SANCTUARY CITIES: THE COST OF UNDERMINING LAW AND ORDER

March 10, 2026

SUPPLEMENTAL STATEMENT OF DONALD ROSENBERG
PRESIDENT, ADVOCATES FOR VICTIMS OF ILLEGAL ALIEN CRIME
FATHER, DREW ROSENBERG JANUARY 9, 1985 – NOVEMBER 16, 2010

Dear Chairman Graham, Ranking Member Merkley, and members of the Senate Budget Committee,

Unfortunately, an illness precluded me from attending and testifying at the hearing, Sanctuary Cities: The Cost of Undermining Law and Order. Having participated twice before in Congressional hearings and having watched all the Senate and House hearings on illegal immigration for the past decade, I can't say I was surprised by the comments and questions from the Democratic Senators.

From ranking member Markley's ridiculous opening comments to witness Brendan Duke, who didn't know whether entering the US illegally was good or bad, it was more of the same: obfuscation, irrelevance, and sometimes utter nonsense.

The bottom line is that the Constitution doesn't forbid state and local cooperation with the federal government. Considering all the crimes caused by illegal aliens and the cost to US taxpayers, sanctuary law/polices is abhorrent. The excuses are blatantly false, and the representations are repugnant

Sanctuary laws/policies require those jurisdictions to expend more resources in arresting, prosecuting, and incarcerating criminal illegal aliens repeatedly, as recidivism runs over 40 percent in the first year after release (or never incarcerated at all) and 68 percent within three years. The cost to the victims of these crimes is incalculable. A simple notification to ICE costs almost nothing and protects future victims. The claim of limited resources is outrageous.

Whatever happened to "no one is above the law?"

It was often mentioned during the hearing that crime rates in sanctuary jurisdictions were lower than in non-sanctuary jurisdictions. That may be true (although the studies that make the claim don't prove it), but it is irrelevant. My son and the tens of thousands of others were killed by illegal aliens, not a "rate" of crime. Ignored are the literally millions of other victims of illegal alien crime. Even if removing all illegal aliens increased the "rate of crime," the number of crimes and victims would go down precipitously.

Isn't it better to have fewer victims than a lower "rate of crime"?

The notion that sanctuary allows those here illegally to report crimes without fear of deportation is both preposterous and overblown. I am unaware of any law enforcement agency in the United States that asks people about their immigration status when they call to report a crime. Furthermore, there is a direct correlation between sanctuary jurisdictions and those that are very soft on crime, releasing the accused within hours of arrest. I certainly wouldn't report a crime if I knew the criminal would be released immediately and might know I was the one who turned them in.

Advocates claim that trust must be built between the community and law enforcement. I'm all for trust, but at what cost and what effectiveness? When you look at the crud being arrested and deported out of Boston, Chicago, Los Angeles, Minneapolis, and other sanctuary cities, it certainly doesn't look like the trust thing is working very well. Deportations will make us all safer.

Senator Merkley made the point that Mississippi, not a sanctuary state, had the highest crime/murder rate in the country. Is the solution to bring in more illegal aliens? I would suggest the Senator ask Congressman Bennie Thompson about crime in Jackson.

Senator Merkley, in his opening statement, exemplified the lack of a cogent argument as to why sanctuary states/cities are good.

He spoke about the Big, Beautiful Bill and how it benefited the wealthy, denied health care, and increased the national debt. Maybe, but what does that have to do with sanctuary law and policies? He had the audacity to say that the bill was a "betrayal of American citizens". Really! He supported 10-13 million illegal aliens entering the country with little or no vetting. He protects illegal aliens even when they've committed more crimes once in the country. That's not just a betrayal but treason.

Merkley continued, "Sanctuary states and cities are not places that are safe havens where immigrants cannot be detained or deported. It refers to the decision that local police will serve as local police and not be commandeered to be assistant ICE agents. These policies are rooted in the 10th Amendment to the Constitution."

So, what are sanctuary cities or states? We can define them by what they do. Sanctuary cities and states exist to protect illegal aliens who have committed additional crimes once in the United States from being deported. They severely impede ICE's ability to carry out its constitutional responsibility to enforce federal law. Further, Senator Markley, we aren't talking about immigrants. We are talking about illegal aliens. They are not one and the same.

You are so concerned about the ICE budget, yet sanctuary states and cities tremendously increase the costs to find and remove these people.

No one is asking the local police to be assistant ICE agents. Yes, they are asking for some minimal cooperation, but as local police aren't they supposed to protect local citizens from criminals, whether those criminals are citizens or illegal aliens? There are over 35,000 police officers in New York City, which has a population of 8 million. There are 13,000 ICE agents for a country of 340 million. Please don't try to convince me that releasing criminals back into our communities makes us safer.

And yes, the 10th Amendment may give them the right to refuse to cooperate on "states' rights" grounds, but don't states and cities have a responsibility to protect law-abiding citizens?

Senator Van Hollen asked Chad Wolf if people have the right to peacefully protest, which, of course, they do. He then questioned whether Renee Good and Alex Pretti were domestic terrorists, implying that they were peacefully protesting.

When Mr. Wolf refused to comment until the investigation into their deaths was concluded, Van Holin angrily claimed that the video was all that was needed to come to a decision. The one thing the videos clearly showed was that neither Good nor Pretti was peacefully protesting. He then claimed that their parents deserved an apology from the U.S government. If that's the case I'm still waiting for an apology from Gavin Newsom (Mayor of San Francisco), Kamala Harris, (District Attorney of San Francisco) Barrack Obama (President of the United States) and every Democratic Senator and Congressman from 1986-2010 who sat on their collective asses and allowed the abomination of illegal aliens entering the U.S. to happen. Mr. Van Hollen was a congressman from 2003-2017. I'm awaiting his apology.

Senator Van Hollen, along with David Bier, claims that those in detention pose no threat level at all. Bier claimed they could be released right now without any threat to the public. Roberto Galo, the man who

killed my son, posed no threat from 1999 when he entered the country illegally until November 16, 2010, at 5:30 P.M. PT. Thirty seconds later, he killed my son.

Van Hollen, smirking, about the "Big Lie," then continues talking about how the White House and others claim that immigrants are the cause of the growing national debt and that immigrants take more than they contribute. Bier then discusses the contributions of immigrants. Yes, Senator Van Hollen, what you are saying is a big lie. I repeat, we are talking about illegal aliens, not immigrants.

Senator Padilla complains that enforcement actions across the country have become damaging to our communities and local economies. He claims the data shows that sanctuary policies make communities safer, healthier, and more prosperous. They have lower crime rates and higher median incomes. Less poverty, less reliance on public assistance, higher labor force participation, and lower unemployment.

It's hard to believe that a Senator from California would make that statement.

I live there and have for over 36 years. Other than cleaner air, the state has deteriorated in every imaginable way. We have a massive housing shortage and the second-highest rent in the nation. We have half of the homeless population of the entire country. We have a massive budget deficit partly because of all the funds we spend supporting illegal aliens, with the highest expenses for schooling and health care for millions of people who shouldn't be here. We have the highest poverty level in the U.S. Over 17 percent of residents (7 million) are unable to cover basic needs, and 34.8 percent of residents are considered poor or near poor. It is the massive public assistance that drives our budget deficits. Lastly, California had one of the highest unemployment rates in the country, at 5.6 percent.

It's time to "Make California Great Again."

Senator Kaine, for the third time in this hearing, conflated immigrants with illegal aliens. "I wish we had a hearing about the positive impacts immigrants have on the United States." Fine, but this hearing is about Sanctuary Cities: The Cost of Undermining Law and Order. He then stated, "When we have hearings, it's about immigrants doing bad things." No, it's about illegal aliens doing bad things and why, if we enforced our laws, those bad things wouldn't happen.

You are all United States Senators. Your priority is to protect United States citizens. You aren't failing the American people; you are betraying them. It is you, with the help of a compliant media, that is undermining law and order, and we, the victims of illegal alien crime, are paying the price.

Not all illegal aliens will kill, rape, molest a child, or commit other crimes. But because of how they entered the country (or overstayed their visa) they forfeited their right to remain here.

As we have seen, allowing illegal aliens into the country is very easy; removing them is much more difficult. However, if we are a "country of laws," we must remove them, however long it takes.

Barbara Jordan, a Texas Congresswoman and the Chair of the U.S. Commission on Immigration Reform under President Clinton, said it best.

"Credibility in immigration policy can be summed up in one sentence: *Those who should get in, get in; those who should be kept out, are kept out; and those who should not be here will be required to leave.*"

Many in the democratic Party want to abolish ICE. Fine, I'm good with that, but first end all sanctuary jurisdictions and then deport all illegal aliens. We won't need ICE. Think of all the money and lives we'll save.

It is obvious that the Democratic Party does not want to enforce our immigration laws. I hope that none of your loved ones meet the fate that so many law-abiding American citizens have met. If that should happen, I want you to know that I will be there for you despite your refusal to be there for the American people.

While there isn't a legal definition for sanctuary cities or states, we can define them by what they do. Sanctuary cities and states exist to protect illegal aliens who have committed additional crimes once in the United States from being deported.

Why would United States Senators want to protect these people from deportation? While you deny it, the numbers tell a different story as tens of thousands of detainers are ignored every year. You claim that all that is needed is a judicial warrant, knowing that requesting such would all but bring deportations to a slow crawl. But more importantly, knowing that a judicial warrant is not required by law to turn over an illegal alien to ICE.

Statement for the Record of

Dan Rayfield
Attorney General of Oregon

Senate Budget Committee
March 10, 2026

Chairman Graham, Ranking Member Merkley, and Members of the Committee,

Thank you for the opportunity to submit testimony on several important, related topics—including efforts by local law enforcement to keep our communities safe; the ongoing collaboration between state, local, and federal law enforcement officials; and the rights of States in our federal system to prioritize resources on local public safety needs. I welcome the opportunity to share Oregon's experiences, as well as my concerns about the End Sanctuary Cities Act of 2026.

Our Shared Priority—Keeping Communities Safe

Let me first emphasize a common value that we all share: Keeping communities safe. This is the top priority for our front-line law enforcement officers. Day in and day out, state and local law enforcement officers valiantly and tirelessly respond to the most urgent public safety needs of our communities. They do so under pressure and in extremely challenging circumstances—and almost always with limited resources, given the practical constraints on the budgets of our state and local governments.

As a function of limited resources, the reality is that state and local governments must make choices and must carefully target taxpayer dollars to the greatest public safety needs within each of our communities. Allowing local law enforcement to prioritize local public safety needs is good policing, good policy, and good government—in each of our states and localities, it ensures that officers can focus on the greatest public safety threats; prosecutors can bring cases in a timely manner; and victims can receive justice and restitution. Indeed, this longstanding ability of state and local governments to set their own law enforcement priorities reflects the genius of our federal system, in which our Constitution reserves the police power to the States, which in turn remain directly accountable to the people.

In these politically polarized times, it's easy to lose sight of an important fact: Cooperation remains the norm for federal, state, and local law enforcement. Effective public safety efforts require strong, ongoing partnerships between local, state, and federal officials—partnerships that remain laser-focused on actions that keep our communities safe. In Oregon, our state and local officers regularly partner with federal law enforcement on a variety of important efforts and initiatives that keep violent criminals off of our streets. We remain committed to that coordination and those relationships. And from our experience, true and effective partnership requires conversations and dialogue between governments about the local needs of our communities. It is simply not effective when the federal government dictates priorities or makes threats in a way that is divorced from on-the-ground public safety needs.

Background on Oregon's Sanctuary Laws

For nearly 40 years, Oregon's sanctuary laws have prohibited state and local officials from using state resources to aid in federal immigration enforcement efforts. Under those laws—which were enacted in 1987 and have peacefully coexisted with seven presidential administrations—state and local public bodies must decline certain requests for information or assistance from Immigration and Customs Enforcement (ICE) unless the federal government first obtains a judicial warrant or court order. That process ensures that ICE seeks assistance from our state only after a neutral judge has an opportunity to review the request and determine whether it satisfies applicable legal requirements.

To be clear, federal immigration officers can and do detain unlawfully present immigrants in Oregon. And when federal immigration officers obtain a judicial order or warrant, Oregon's sanctuary laws present no barrier to state-federal cooperation—indeed, in those circumstances, our laws *require* state and local officials to comply with ICE's underlying request for information. In other words, while the people of Oregon have made a policy choice to prioritize the state's limited resources on local law enforcement needs (rather than enforcement of federal immigration laws), our laws provide a mechanism for federal immigration officials to seek information from state and local partners, in appropriate circumstances.

This longstanding process is both flexible and practical, and fully consistent with federal law. *See United States v. California*, 921 F.3d 865, 889 (9th Cir. 2019) ("Federal law provides states and localities the *option*, not the *requirement*, of assisting federal immigration authorities."). Indeed, federal courts have already made clear that federal laws do not displace Oregon's sanctuary laws—and that a contrary interpretation would present serious constitutional concerns. *See City & Cnty. of San Francisco v. Garland*, 42 F.4th 1078, 1085 (9th Cir. 2022) (holding that its narrow interpretation of 8 U.S.C. § 1373, the key federal law in this area, "erased any alleged conflict" between state and federal law); *Oregon v. Trump*, 406 F. Supp. 3d 940, 972 (D. Or. 2019), *aff'd in part, vacated in part, remanded sub nom., City & Cnty. of San Francisco*, 42 F.4th 1078 (9th Cir. 2022) (concluding that, to the extent a conflict exists, § 1373 is unconstitutional under a "straightforward applicable of anti-commandeering principles").

Oregon's sanctuary laws ensure that state and federal officials can carry out their respective public safety duties in a manner that respects both the rule of law and the dual sovereignty within our federal system. Perhaps most importantly, Oregon's approach retains the confidence of the public across our state—when a ballot measure proposed repealing Oregon's sanctuary laws in 2018, nearly two-thirds of Oregon voters rejected that proposal.

Prioritizing Public Safety in our Communities

Oregon enacted its sanctuary laws in 1987 and retained those laws in 2018 with overwhelming, bipartisan votes because those laws delineate clear lines between state and federal authorities, allow a mechanism for state-federal cooperation, and help keep our communities safe. Community leaders—including police chiefs and sheriffs from across the country—have repeatedly told us that when immigrants don't feel comfortable calling local law enforcement,

crime goes unreported and communities become less safe for everyone. Our local correctional facilities tell us that they want to prioritize the detention of violent criminals rather than non-violent immigration detainees. And there are many other circumstances in which clear lines between state and federal authorities furthers key public safety objectives and other important community needs—for example, in Oregon, immigrant witnesses feel safe testifying in our courts, and immigrant children feel safe attending our neighborhood schools.

Let me provide a specific example that I heard from one of our district attorneys here in Oregon that was particularly compelling to me. Recently, the district attorney's office in one of Oregon's largest counties conducted a murder trial that relied on the testimony of a key witness. That witness happened to be an undocumented immigrant who was scared to show up at the county courthouse because she was worried that ICE might come and pick her up. Fortunately, the witness showed up and the district attorney's office was able to get a conviction. But without Oregon's sanctuary laws drawing a clear line between the roles of state and federal authorities, that might not have been the case.

Ultimately, our public safety objectives are best achieved when each level of law enforcement focuses on what they do best. The primary role of state and local law enforcement is—and has always been—to focus on and be responsive to the needs of local communities, enforcing state and local laws. It is the role of the federal government to enforce federal immigration laws.

The Right of States to Decide

Oregon has the oldest sanctuary law in the nation. I have provided my thoughts on Oregon's experience. I also recognize not every State or local jurisdiction will agree with our approach. But that's the beauty of our federal system—each of our States has the right to make its own policy choices when it comes to setting local law enforcement priorities and allocating state and local tax dollars. Importantly, it is not the federal government's role to compel or coerce States into a particular approach. That is especially true in public safety and policing matters, where every jurisdiction has different needs.

And that is the root of my concerns regarding the End Sanctuary Cities Act of 2026. Threatening state and local officials with federal criminal penalties merely because they obey longstanding state sanctuary laws will not make our communities safer. It will not reduce tension. It is not consistent with our constitutional design.

We can do better. Oregon welcomes discussion and debate on these important issues. Our laws are based in our experiences and speak to the priorities and values of our people—and they also allow coordination with federal authorities in appropriate circumstances. I would suggest that's the best approach here: States must prioritize their local public safety needs, but should be open to partnership with federal authorities when that collaboration helps make our communities safer.

Thank you for the opportunity to offer testimony on these important issues today, and to share some lessons Oregon has learned along the way. I urge you to oppose the End Sanctuary Cities Act of 2026.

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Date: March 6, 2026

Re: End Sanctuary Cities Act 2026

Position: OPPOSE

To: Senator Lindsey Graham, Senate Budget Committee Chairman, Senator Jeff Merkley, Senate Budget Committee Ranking Member

Distinguished Members of the Senate Budget Committee,

My name is Lieutenant Diane Goldstein (Ret.) and I am submitting this testimony on behalf of myself, having served a 21-year career at the Redondo Beach Police Department in California, and on behalf of the Law Enforcement Action Partnership (LEAP), where I serve as executive director.

LEAP is an international nonprofit group of police, prosecutors, judges, and other criminal justice professionals who speak from firsthand experience serving in the justice system. Our mission is to make communities safer by focusing law enforcement resources on the greatest threats to public safety, addressing the root causes of crime, and working toward healing police-community relations.

As law enforcement professionals, we oppose the End Sanctuary Cities Act of 2026 because it would undermine the very public safety it claims to protect. Trust is the lifeblood of effective policing. We cannot prevent, detect, or solve crimes without the public's partnership and cooperation. When immigrant communities fear that any interaction with local police could trigger immigration consequences for themselves or others, that trust collapses. People stop calling 911, stop reporting crimes, and stop cooperating as witnesses. Criminals benefit, victims suffer, and police lose the community relationships that make effective policing possible.

Policies that draw a clear line between local policing and federal immigration enforcement help prevent this breakdown in trust. They allow officers to focus on the public safety priorities identified by their communities rather than being drawn into federal immigration enforcement. When local departments are pressured to

prioritize federal immigration objectives, officer time and resources are diverted away from serious threats such as violent crime, domestic violence, and community safety concerns. The result is a policing environment that is both less effective and less trusted.

This legislation goes even further by imposing criminal liability on local officials who follow those policies, creating a new federal crime—"obstruction of immigration laws by official interference"—that would apply to any "responsible executive official" who restricts compliance with a federal request for advance notice of an individual's release. These definitions are extremely broad. In practice, this bill could sweep in police chiefs, sheriffs, and other senior law enforcement leaders who are simply implementing policies enacted by state and local leaders. The measure contains no safe harbor for officials who are following the laws of their own jurisdictions, meaning a police chief or sheriff could face federal prosecution simply for complying with the policies they are legally required to enforce.

The penalties attached to this offense are extraordinarily severe. Depending on the underlying charge against the individual released, a local official could face prison sentences ranging from mandatory jail time to as much as 25 years. These penalties attach not to harm caused, but merely to the category of offense the individual had been charged with—including cases in which there has been no conviction. In other words, a local police leader could face penalties comparable to those imposed on serious violent offenders simply for adhering to local policy decisions.

Beyond these practical concerns, the bill raises serious constitutional questions. The Supreme Court has repeatedly held—in cases such as *Printz v. United States* (1997) and *New York v. United States* (1992)—that the federal government cannot compel state or local officials to administer or enforce federal regulatory programs. This principle, known as the anti-commandeering doctrine, exists to preserve the balance of federalism and protect local democratic decision-making. But rather than encouraging cooperation through incentives, the End Sanctuary Cities Act attempts to force cooperation by threatening state and local officials with federal criminal prosecution.

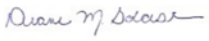
Even setting aside the constitutional concerns, the bill presents significant enforcement problems. Its vague definition of who qualifies as the "responsible executive official" leaves unclear who would ultimately be liable when policies originate from elected bodies but are implemented by law enforcement leaders. At the same time, the decision of whom to prosecute would rest entirely with federal prosecutors, creating the risk that enforcement could become selective or politically motivated. The legislation also provides no notice, compliance period, or administrative process before criminal liability attaches—meaning a local official could effectively become a federal criminal immediately upon adhering to a policy enacted by their jurisdiction.

Policing works best when communities and local agencies have the flexibility to set their own public safety priorities. Policies that blur the line between local policing and federal immigration enforcement erode trust and legitimacy, and legislation that threatens police chiefs and sheriffs with prison for following their own local laws takes us even further away from effective policing.

The path to safer communities lies in strengthening trust, respecting local decision-making, and allowing law enforcement to focus on serving the needs of their communities. The End Sanctuary Cities Act moves in the opposite direction, and I urge lawmakers to reject it.

Thank you for the opportunity to share our law enforcement perspective in opposition to this bill.

Respectfully,



Lt. Diane Goldstein (Ret.)
Executive Director, Law Enforcement Action Partnership

American Immigration Council

Statement of the American Immigration Council

Submitted to the Senate Committee on The Budget
Hearing Entitled "Sanctuary Cities: The Cost of Undermining Law and Order"
March 10, 2026

The American Immigration Council ("the Council") is a non-profit organization that envisions a nation where immigrants are embraced, communities are enriched, and justice prevails for all. We strive to create a society that values immigrants as vital contributors and where everyone is afforded an equal opportunity to thrive socially, economically, and culturally. We do this by shaping immigration policies and practices at the federal, state, and local levels through educating decisionmakers and the public and advancing sensible policy solutions through research and advocacy.

Recently, the Trump administration has escalated its attacks on so-called "sanctuary jurisdictions" as it considers demands for DHS funding cuts and overdue accountability. Despite the committee's focus on so-called "sanctuary" policies, there is no legal definition of the term, which refers to a range of laws and policies passed by state and local governments that aim to prioritize building trust with communities and promoting safety through limits on local cooperation with federal immigration enforcement efforts.

These policies may take many forms, but typically they place some form of restriction on the use of state and local government resources for federal immigration enforcement. These policies do not prevent federal officials from carrying out their immigration enforcement duties; "sanctuary" policies do not create a forcefield that prevent ICE from taking action.

In general, sanctuary policies may take the following forms:

- policies restricting the ability of state and local police to make arrests for federal civil immigration violations, or to detain individuals on civil immigration warrants;
- policies prohibiting "287(g)" agreements through which Immigration and Customs Enforcement (ICE) deputizes local law enforcement officers to enforce federal immigration law;
- policies that prevent local governments from entering a contract with the federal government to hold immigrants in detention;
- policies preventing immigration detention centers;

- policies restricting the police or other city workers from asking about immigration status;
- policies restricting the sharing of certain information on immigrants with the federal government;
- policies restricting local police responses to federal immigration detainers; and
- policies refusing to allow ICE into local jails without a judicial warrant.

Most modern sanctuary policies emerged during the Obama administration, as the "Secure Communities" program led to an increasing number of immigrants with minor non-violent interactions with local law enforcement being subject to immigration detainers and eventual removal. These policies have emerged again as a focus of attention during the Trump administration, given the increasingly deadly and chaotic mass enforcement agenda.

As a result, over the last year, especially in the wake of violent immigration enforcement in Minnesota, we have seen more states around the country passing laws that their legislators say aim to protect the safety of their communities from the worst harms of immigration enforcement. Two states, Maryland and New Mexico, for example, recently enacted legislation that codifies restrictions on the use of local police to enforce federal immigration laws.

Unfortunately, the Trump administration has targeted these policies for rebuke, falsely asserting that they endanger local communities or are otherwise illegal. However, studies have shown the former to be empirically false, and courts have found the latter to be untrue.

Here's what the last decade of sanctuary policies has taught us:

- **Research shows that sanctuary policies do not make communities unsafe.** Over the last decade, there has been extensive research into the empirical impacts of sanctuary policies. Research has shown that localities with sanctuary policies generally have lower crime rates than those without. When looking at specific jurisdictions where sanctuary policies are adopted, research consistently shows that such policies either have no measurable impact on crime or may even result in some crime going down. No study has ever shown that sanctuary jurisdictions are less safe than others or that sanctuary policies have resulted in an increase in crime.
- **Most sanctuary policies are constitutional and have been upheld in court repeatedly**. During both his first and second term, President Trump's Department of Justice sought to overturn local sanctuary policies through litigation. These lawsuits have failed nearly unanimously. For example, in 2025, federal judges dismissed lawsuits brought by the Trump administration against New York, Chicago, and Rochester. Similar lawsuits were defeated in 2019. While other lawsuits remain pending, the Trump administration has repeatedly failed in its argument that federal law and the Supremacy Clause preempt state noncooperation policies. Indeed, these policies reflect the constitutional framework that reserves general police powers to states and local

governments. Preserving local decision-making authority allows communities to establish public safety priorities that reflect local conditions, while respecting the division of responsibilities between state, local, and federal governments.

- **Building community trust and promoting public safety are key components of sanctuary policies.** Effective law enforcement depends on public trust. When residents feel safe reporting crimes and cooperating with police — without fear of unrelated immigration consequences — policing becomes more effective. These policies may also make communities safer by allowing local law enforcement to focus on preventing and solving crime rather than enforcing federal immigration mandates.
- **Sanctuary policies do not prevent the federal government from prioritizing violent criminals for immigration consequences.** Sanctuary policies do not prevent cooperation in serious criminal cases; instead, they help ensure enforcement efforts are focused where they matter most for safety. Indeed, many local governments with sanctuary policies explicitly exclude people with violent criminal records from their protections. However, under the Trump administration, DHS is focused on arresting as many noncitizens as possible to meet artificial quotas and has consistently misled the public about who is being prioritized for immigration enforcement. During Operation Metro Surge in Minnesota, DHS officials even declined to follow up on some detainers because they were focusing on community arrests instead.
- **Sanctuary policies allow local leaders to focus on their communities instead of responding to changes in federal immigration enforcement.** Federal immigration enforcement practices have varied dramatically across administrations, leaving states and localities to adjust to rapidly shifting priorities and enforcement approaches, as well as changes in immigration law. Last year, more than 350 state and local government officials opposed federal efforts to impose penalties against localities with these policies, writing that "When residents fear that accessing schools, hospitals, libraries, food banks, and other essential services could expose them or their loved ones to immigration enforcement, they are less likely to seek assistance—undermining public safety, health, education, and economic stability for everyone." This emphasizes that local leaders and communities should be able to determine how their resources are used. Because local governments are not reimbursed for most cooperation with ICE, these policies can prevent the diversion of local dollars away from essential services such as policing, emergency response, and community programs.
- **Limiting state and local involvement in federal immigration enforcement can reduce legal and financial liability**. When state and local governments are forced to participate in federal immigration enforcement, they may face increased legal exposure, financial liability, and risk to constitutional compliance when they detain immigrants longer than permitted solely on a non-binding "request" from ICE. Therefore,

sanctuary policies can help reduce these risks by keeping local agencies within clearly defined lawful roles—local governments and police departments are responsible for local public safety, while immigration enforcement is a federal responsibility.

As Congress considers legislation relating to sanctuary policy, this Committee should ensure that any lawmaking is based on sound policy and research, rather than deceptive talking points advanced to further a political agenda.

The Wayback Machine - https://web.archive.org/web/20250529215954/https://www.dhs.gov/sanctuary-jurisdictions
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Homeland Security

MENU

Sanctuary Jurisdictions Defying Federal Immigration Law

Sanctuary jurisdictions undermine the rule of law and endanger the lives of Americans and Law Enforcement

Executive Order 14287: *Protecting American Communities from Criminal Aliens* requires that a list of states and local jurisdictions that obstruct the enforcement of Federal immigration laws (sanctuary jurisdictions) be published. Sanctuary jurisdictions including cities, counties, and states that are deliberately and shamefully obstructing the enforcement of federal immigration laws endangering American communities. Sanctuary cities protect dangerous criminal aliens from facing consequences and put law enforcement in peril.

The list below was created to identify sanctuary jurisdictions, which are determined by factors like compliance with federal law enforcement, information restrictions, and legal protections for illegal aliens.

Each jurisdiction listed will receive formal notification of its non-compliance with Federal statutes. DHS demands that these jurisdictions immediately review and revise their policies to align with Federal immigration laws and renew their obligation to protect American citizens, not dangerous illegal aliens.

Note that the list can be reviewed and changed at any time and will be updated regularly. No one should act on this information without conducting their own evaluation of the information.

Close all Open all

Sanctuary Jurisdiction List

Alaska

California

Colorado

Connecticut

Delware

District of Columbia

Georgia

Hawaii

Idaho

Illinois

Indiana

Kansas

Kentucky

Louisiana

Maine

Maryland

Massachusetts

Michigan

Minnesota

Nebraska

Nevada

New Hampshire

New Jersey

New Mexico

New York

North Carolina

North Dakota

Ohio

Oregon

Pennsylvania

Rhode Island

Tennessee

Vermont

Virginia

Counties

- Albemarle County
- Amherst County
- Arlington County
- Augusta County
- Brunswick County
- Charlotte County
- Chesterfield County
- Dinwiddie County
- Fairfax County
- Gloucester County
- Halifax County
- Hanover County
- Henrico County
- Martinsville County
- Middlesex County
- Prince William County
- Rappahannock County
- Richmond County
- Tazewell County
- Warren County

Cities

- Abingdon
- Alexandria
- Charlottesville
- Duffield
- Hampton
- Lynchburg
- Manassas
- Martinsville
- Newport News
- Portsmouth City
- Richmond
- Tazewell
- Virginia Beach

Washington

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Keywords

IMMIGRATION ALIEN BORDER SECURITY LAW ENFORCEMENT

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REPORT JAN 26, 2017

The Effects of Sanctuary Policies on Crime and the Economy

Statistical analysis illustrates that across a range of social and economic indicators, sanctuary counties perform better than comparable nonsanctuary counties.

AUTHORS

Tom K. Wong

Advancing Racial Equity and Justice, Building an Economy for All, Restoring Social Trust in Democracy, +4 More

The historic Treme section of New Orleans, October 2012. (AP/Gerald Herbert)

Introduction and summary

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As the Trump administration begins to implement its immigration policy agenda, the issue of local assistance with federal immigration enforcement officials is back in the spotlight. So-called sanctuary jurisdictions are one focus of that debate. Sanctuary counties—as defined by this report—are counties that do not assist federal immigration enforcement officials by holding people in custody beyond their release date.[1] Using an Immigration and Customs

Enforcement, or ICE, dataset obtained via a Freedom of Information Act request filed by the Immigrant Legal Resource Center,[2] the analyses in this report provide new insights about how sanctuary counties perform across a range of social and economic indicators when compared to nonsanctuary counties.

To understand the effects of having a sanctuary policy, we statistically match counties based on a broad range of demographic characteristics and then compare sanctuary counties to nonsanctuary counties to better understand the effects that sanctuary policies have on a local jurisdiction.

The data are clear: Crime is statistically significantly lower in sanctuary counties compared to nonsanctuary counties. Moreover, economies are stronger in sanctuary counties—from higher median household income, less poverty, and less reliance on public assistance to higher labor force participation, higher employment-to-population ratios, and lower unemployment.

Among the main findings:

- There are, on average, 35.5 fewer crimes committed per 10,000 people in sanctuary counties compared to nonsanctuary counties.
- Median household annual income is, on average, $4,353 higher in sanctuary counties compared to nonsanctuary counties.
- The poverty rate is 2.3 percent lower, on average, in sanctuary counties compared to nonsanctuary counties.
- Unemployment is, on average, 1.1 percent lower in sanctuary counties compared to nonsanctuary counties.
- While the results hold true across sanctuary jurisdictions, the sanctuary counties with the smallest populations see the most pronounced effects.

Altogether, the data suggest that when local law enforcement focuses on keeping communities safe, rather than becoming entangled in federal immigration enforcement efforts, communities are safer and community members stay more engaged in the local economy. This in turn brings benefits to individual households, communities, counties, and the economy as a whole.

Sanctuary jurisdictions, detainers, and notifications

To what extent should local law enforcement agencies, or LEAs, be required to assist federal immigration enforcement officials? Localities have no legal obligation to engage in federal immigration enforcement actions[3] and often find themselves legally liable when they do.[4] For more than a decade, local law enforcement officials have argued against assisting federal immigration enforcement agencies such as ICE. Assisting in federal immigration enforcement efforts can drive a wedge between local law enforcement officials and the communities they serve, which undermines public safety. According to a report issued by the International Association of Chiefs of Police:

[S]tate and local law enforcement should not be involved in the enforcement of civil immigration laws since such involvement would likely have a chilling effect on both legal and illegal aliens reporting criminal activity or assisting police in criminal investigations.[5]

The Major Cities Chiefs Association, which represents the 68 largest LEAs in the United States, similarly concluded that commingling the work of local police with federal immigration enforcement efforts "would result in increased crime against immigrants and in the broader community, create a class of silent victims and eliminate the potential for assistance from immigrants in solving crimes or preventing future terroristic acts."[6]

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One of the ways that localities become entangled in federal immigration enforcement is through what is known as an immigration detainer, or ICE Form I-247D.[7] A detainer is a request that a LEA hold a person for up to 48 additional hours after his or her release date, so that ICE can decide whether to take the person into custody for immigration detention and removal proceedings. Detainers were widely used in the now defunct Secure Communities program,[8] which ended in 2014, and they continue to be used in the successor Priority Enforcement Program.[9]

Despite their continued use, when former Department of Homeland Security, or DHS, Secretary Jeh Johnson ended Secure Communities he stated, "A number of federal courts have rejected the authority of state and local law enforcement agencies to detain immigrants pursuant to federal detainers issued under the current Secure Communities program."[10] Indeed, a series of court decisions have ruled that the use of detainers violates Fourth Amendment and due process rights.[11] For example, in *Galarza v. Szalczyk*,[12] a man was held under a detainer for three days after he posted bail despite having a driver's license, a social security card, and informing police that he was born in New Jersey. The man was released only when ICE confirmed that he was an American citizen. He subsequently filed a civil rights suit against the United States, the City of Allentown, and Lehigh County challenging his unlawful detention. After positive rulings by the federal district court and the court of appeals, he settled for nearly $150,000 in damages. Other similar lawsuits have proven costly for the jurisdictions that have held people on detainers.[13]

Whereas localities have a range of policy options available to them to ensure that all individuals are treated equally regardless of their immigration status,[14] a common thread that runs through sanctuary jurisdictions is their acknowledgement that detainers infringe on Fourth Amendment and due process rights. To be clear, such localities are not refusing to comply with the law. In fact, every single jurisdiction still shares fingerprint data upon arrest with the FBI, which in turn shares these data with the DHS for immigration status checks. Rather, in declining a detainer request, localities are choosing not to hold an individual beyond the point at which the person would otherwise be released from custody, which is generally the point at which the legal authority to continue detaining the individual is over.[15]

Definitions, data, and method

The sanctuary jurisdictions analyzed here are defined as counties that ICE has identified as not willing to accept detainers. In the dataset, ICE codes 2,492 counties by their "Current Detainer/Notification Acceptance Status."[16] These counties account for 92.2 percent of the total U.S. population and 95.3 percent of the total foreign-born population in the United States. Of the 2,492 counties coded by ICE, 608 are defined by ICE as sanctuary jurisdictions.

The analyses begin by comparing all sanctuary counties to all nonsanctuary counties in the ICE dataset across a range of social and economic indicators with an eye toward identifying statistically significant differences. The results are divided into six groups, following the National Center for Health Statistics' urban-rural classification, running from large central metro counties to noncore, rural counties.[17] Data on crime come from the most recent Federal Bureau of Investigation, or FBI, Uniform Crime Reporting, or UCR, Program.[18] Data on economic indicators come from the recently released 2015 American Community Survey, or ACS, 5-year Estimates.[19]

Next, the report pushes further by using advanced statistical techniques to add rigor to the analyses. More specifically, coarsened exact matching, or CEM, is used to statistically match sanctuary counties to nonsanctuary counties.[20] CEM is a method for improving causal inferences that estimates the sample average treatment effect on the treated, or SATT. In other words, CEM statistically matches sanctuary counties to comparable nonsanctuary counties; compares

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differences in outcomes between sanctuary counties and the matched nonsanctuary counties; allows us to evaluate these differences while controlling for differences in population, the foreign-born percentage of the population, and the percentage of the population that is Latino;[21] and then uses the results of the analysis to estimate the effect that being a sanctuary county has on crime and the economy.[22]

Crime is lower in sanctuary counties compared to nonsanctuary counties

Crime is statistically significantly lower in sanctuary counties compared to nonsanctuary counties. Crime is defined here as the total number of violent crimes—murders, rapes, robberies, and assaults—and property crimes—burglaries, larceny, motor vehicle thefts, and arsons—per 10,000 people. The data indicate that crime is statistically significantly lower in sanctuary counties in large central metro counties, small metro counties, micropolitan counties, and noncore, rural counties. Large central metro counties show the most pronounced difference. Large central metro sanctuary counties have 65.4 crimes fewer per 10,000 people than large central metro nonsanctuary counties.

Perhaps more importantly, the results of the CEM analysis show that crime is statistically significantly lower in sanctuary counties compared to nonsanctuary counties when statistically matching and then controlling for population characteristics, including total population and the foreign-born percentage of the population. The results of the CEM analysis show that there are, on average, 35.5 fewer crimes per 10,000 people in sanctuary counties—a result that is highly statistically significant.

Economies are stronger in sanctuary counties compared to nonsanctuary counties

Median household income

Median household income is statistically significantly higher in sanctuary counties compared to nonsanctuary counties. This holds true across the entire range of urban-rural classifications. The results of the CEM analysis show that median household income is, on average, $4,352.70 higher in sanctuary counties when statistically matching and then controlling for population characteristics. This result is highly statistically significant.

Is this result driven by income gains among Latinos? Surprisingly, no. Unpacking the data shows that white median household income is statistically significantly higher in sanctuary counties compared to nonsanctuary counties. This also holds true across the entire range of urban-rural classifications. The results of the CEM analysis show that white median household income is on average $2,836.10 higher in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant. On the other hand, while Latino median household income is generally higher in sanctuary counties compared to nonsanctuary counties, these differences are not statistically significant. There is thus no evidence to suggest that income gains in sanctuary counties accrue to Latinos at the expense of whites. A closer look at the data also shows no evidence that income gains in sanctuary counties accrue to Latinos at the expense of African Americans, as median household income for African Americans is also generally higher in sanctuary counties compared to nonsanctuary counties.

Poverty

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Consistent with higher median household income, the data also show that poverty is statistically significantly lower in sanctuary counties compared to

nonsanctuary counties, and this generally holds true across the entire range of urban-rural classifications.

Beginning with total poverty, the results of the CEM analysis show that the percentage of people who live at or below the federal poverty line is, on average, 2.3 percent lower in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant. Moreover, white poverty is, on average, 1.4 percent lower in sanctuary counties, and Latino poverty is, on average, 2.9 percent lower in sanctuary counties.

Public assistance

Relatedly, there is significantly less reliance on public assistance in sanctuary counties compared to nonsanctuary counties. Public benefits usage—whether it is the percentage of households that receive SNAP, formerly known as food stamps; the percentage of households that receive Supplemental Security Income, or SSI; or the percentage of children under 18 who live in households that receive public assistance—is statistically significantly lower in sanctuary counties compared to nonsanctuary counties. This generally holds true across the entire range of urban-rural classifications.

Beginning with SNAP, the results of the CEM analysis show that the percentage of households that receive SNAP benefits is, on average, 2.6 percent lower in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant. The percentage of households that receive SSI is, on average, 0.9 percent lower in sanctuary counties, and the percentage of children younger than 18 in households that receive public assistance is, on average, 4.9 percent lower in sanctuary counties.

Labor force participation

One indicator of a strong local economy is labor force participation.[23] The labor force participation rate is defined as the proportion of the population that is 16 years and older that is in the labor force, meaning working or are actively looking for a job.

The labor force participation rate is statistically significantly higher in sanctuary counties compared to nonsanctuary counties. This generally holds true across the entire range of urban-rural classifications. The results of the CEM analysis show that the labor force participation rate is, on average, 2.5 percent higher in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant.

Unpacking the data again shows that higher labor force participation rates are driven by whites. The results of the CEM analysis show that white labor force participation is, on average, 2.5 percent higher in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant.

The results for Latino labor force participation are more nuanced. Higher Latino labor force participation is generally concentrated in smaller sanctuary counties compared to smaller nonsanctuary counties. For example, Latino labor force participation is 5.2 percent higher in noncore, rural sanctuary counties compared to noncore, rural nonsanctuary counties. However, Latino labor force participation is 2.7 percent lower in large central metro sanctuary counties compared to large central metro nonsanctuary counties. Given the differences in Latino labor force participation across small and large counties, the average effect obtained in the CEM analysis is that Latino labor force participation is 1.2 percent higher in sanctuary counties, but this result only borders on statistical significance.

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Employment-to-population ratio

The employment-to-population ratio[24] is another indicator of a strong local economy. The employment-to-population ratio is the number of people 16 years and older who are employed divided by the total number of people 16 years and older.

The results when analyzing the employment-to-population ratio mirror the trends we see in the data when it comes to labor force participation. More specifically, the employment-to-population ratio is statistically significantly higher in sanctuary counties compared to nonsanctuary counties. This generally holds true across the entire range of urban-rural classifications. The results of the CEM analysis show that the employment-to-population ratio is, on average, 3.1 percent higher in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant.

Unpacking the data again shows that higher employment-to-population ratios are driven by whites. The results of the CEM analysis show that the white employment-to-population ratio is, on average, 3.2 percent higher in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant. The results for the Latino employment-to-population ratio are generally statistically insignificant, meaning there are no measurable differences between sanctuary counties and nonsanctuary counties.

Unemployment

Unemployment[25]—whether measured by total unemployment or white unemployment—is statistically significantly lower in sanctuary counties compared to nonsanctuary counties.

Beginning with the total unemployment, the results of the CEM analysis show that the unemployment rate is, on average, 1.1 percent lower in sanctuary counties when statistically matching and then controlling for population characteristics, and this result is highly statistically significant. The white unemployment rate is, on average, 0.8 percent lower in sanctuary counties. The data indicate that the Latino unemployment rate is, on average, 1.0 percent higher in sanctuary counties, which again suggests that the economic gains to sanctuary counties do not accrue to Latinos at the expense of whites.

Conclusion

Crime is lower and economies are stronger in sanctuary counties compared to nonsanctuary counties. The data support arguments made by law enforcement executives that communities are safer when law enforcement agencies do not become entangled in federal immigration enforcement efforts. The data also make clear that, when counties protect all of their residents, they see significant economic gains. By keeping out of federal immigration enforcement, sanctuary counties are keeping families together—and when households remain intact and individuals can continue contributing, this strengthens local economies. These effects appear particularly pronounced in smaller counties, as removing one person from the economy of a small population has a larger effect than removing one person from the economy of a large population.

This research represents one of the first systematic analyses comparing sanctuary counties to nonsanctuary counties across a range of social and economic indicators. Of course, further research will be needed to examine differences in outcomes within sanctuary jurisdictions across time, but for now, the findings described here paint a clear portrait: To the extent that localities become entangled in federal immigration enforcement efforts, they put in jeopardy the social and economic gains—from lower crime to a stronger local economy—that come with sanctuary policies.

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About the author

Tom K. Wong is associate professor of political science at the University of California, San Diego. He is the author *of The Politics of Immigration: Partisanship, Demographic Change, and American National Identity*.

NILC is exclusively dedicated to defending and advancing the rights and opportunities of low-income immigrants and their families. Our mission is grounded in the belief that every American—and aspiring American—should have the opportunity to fulfill their full potential regardless of where they were born or how much money they have. Using our deep expertise in a wide range of issues that affect low-income immigrants' lives, we work with communities in courtrooms and legislatures to help advance policies that create a more just and equitable society for everyone.

Acknowledgements

The author thanks the Immigrant Legal Resource Center for providing the underlying FOIA data on sanctuary counties for this analysis. He also thanks Philip E. Wolgin and Tom Jawetz of the Center for American Progress, Melissa Keaney of the National Immigration Law Center, and Lena Graber of the Immigrant Legal Resource Center for their comments and advice. He also thanks Sebastian Navarro, Anjleena Sahni, and Kate Panian for their research assistance.

Appendix of results

Table 1 reports the results of the CEM analysis for all of the indicators described in this report.

TABLE 1
The benefits of being a sanctuary county
Analyzing how sanctuary counties compare with nonsanctuary counties

	SATT	SE	p-value
Crime rate	-35.5	5.9	0.000
Median household income	4352.7	575.1	0.000
Median household income—white, non-Latino	2836.1	568.3	0.000
Median household income—Latino	1328.9	736.4	0.000
Poverty rate	-2.337	0.306	0.000
Poverty rate—white, non-Latino	-1.361	0.222	0.000
Poverty rate—Latino	-2.966	0.721	0.000
Supplemental Nutrition Assistance Program	-2.559	0.296	0.000
Supplemental Security Income	-0.879	0.127	0.000
Children under age 18 in households with public assistance	-4.967	0.548	0.000
Labor force participation rate	2.456	0.345	0.000
Labor force participation rate—white, non-Latino	2.586	0.339	0.000
Labor force participation rate—Latino	1.241	0.741	0.094
Employment-to-population ratio	1.103	0.369	0.000
Employment-to-population ratio—white, non-Latino	1.765	0.359	0.000
Employment-to-population ratio—Latino	0.939	0.733	0.200
Unemployment rate	-1.056	0.159	0.000
Unemployment rate—white, non-Latino	-0.829	0.129	0.000
Unemployment rate—Latino	1.015	0.425	0.017

Note: SATT refers to the sample average treatment effect on the treated; SE refers to the standard errors. All replication data are available upon request.

Source: Authors' analysis of 2015 American Community Survey 5-year data and 2015 FBI Uniform Crime Reports. See Federal Bureau of Investigation, "Uniform Crime Reporting," available at https://ucr.fbi.gov/ (last accessed January 2017); Bureau of the Census, "American Community Survey: 2015 Data Release Schedule," available at http://www.census.gov/programs-surveys/acs/news/data-releases/2015/release-schedule.html (last accessed January 2017).

Table 2 reports the differences in crime rates between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications. As the table shows, the largest differences are in large central metro counties and noncore, rural counties. Large central metro sanctuary counties have 65.4 fewer crimes per 10,000 people than large central metro nonsanctuary counties ($p = .038$). Noncore, rural sanctuary counties have 59.4 fewer crimes per 10,000 people than noncore, rural nonsanctuary counties ($p < .001$).

TABLE 2
Crime is generally lower in sanctuary counties
Comparing crime across sanctuary counties and nonsanctuary counties

Crime rate	Sanctuary	Nonsanctuary	Difference	p-value
Large central metropolitan area	367.5 (n = 27)	432.9 (n = 35)	-65.4	0.038
Large fringe metropolitan area	247.7 (n = 76)	229.0 (n = 238)	-19.6	0.181
Medium metropolitan area	318.4 (n = 74)	288.6 (n = 251)	-29.9	0.078
Small metropolitan area	254.5 (n = 76)	290.2 (n = 217)	-35.7	0.051

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Micropolitan area	256.6 (n = 125)	277.4 (n = 403)	-26.8	0.038
Noncore area	121.8 (n = 230)	187.3 (n = 740)	-59.4	0.000

Source: Authors' analysis of 2015 FBI Uniform Crime Reports, "Uniform Crime Reporting," available at https://ucr.fbi.gov/ (last accessed January 2017).

Table 3 reports the differences in median household income, white median household income, and Latino median household income between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications. As the table shows, median household income, white median household income, and Latino median household income is generally higher across the range of urban-rural classifications in sanctuary counties compared to nonsanctuary counties.

Sanctuary counties in large central metros, large fringe metros, and medium metros tend to see higher median household income and higher white median household income.

Sanctuary counties in small metros, micropolitan counties, and noncore, rural counties see higher median household income, white median household income, and Latino median household income.

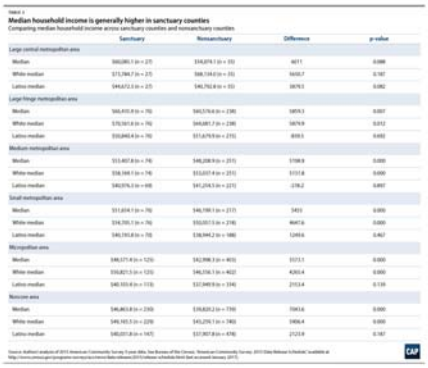

TABLE 3

Median household income is generally higher in sanctuary counties

Comparing median household income across sanctuary counties and nonsanctuary counties

	Sanctuary	Nonsanctuary	Difference	p-value
Large central metropolitan area				
Median	$60,080.1 (n = 27)	$54,079.1 (n = 35)	6011	0.088
White median	$73,784.7 (n = 27)	$68,134.0 (n = 35)	5650.7	0.187
Latino median	$44,672.3 (n = 27)	$40,752.8 (n = 35)	3879.5	0.082
Large fringe metropolitan area				
Median	$66,435.9 (n = 76)	$60,576.6 (n = 238)	5859.3	0.007
White median	$70,561.6 (n = 76)	$64,681.7 (n = 238)	5879.9	0.012
Latino median	$50,840.4 (n = 76)	$51,679.9 (n = 215)	-839.5	0.692
Medium metropolitan area				
Median	$53,407.8 (n = 74)	$48,208.9 (n = 251)	5198.9	0.000
White median	$58,598.1 (n = 74)	$53,037.4 (n = 251)	5151.8	0.000
Latino median	$40,976.3 (n = 69)	$41,234.5 (n = 227)	-258.2	0.897
Small metropolitan area				
Median	$51,834.1 (n = 78)	$46,700.1 (n = 217)	5431	0.000
White median	$54,705.1 (n = 76)	$50,057.5 (n = 214)	4647.6	0.000
Latino median	$40,195.8 (n = 70)	$38,944.2 (n = 188)	1249.6	0.467
Micropolitan area				
Median	$48,577.4 (n = 125)	$42,998.3 (n = 403)	5572.1	0.000
White median	$50,827.5 (n = 125)	$46,556.1 (n = 402)	4265.4	0.000
Latino median	$40,103.8 (n = 113)	$37,949.9 (n = 334)	2153.4	0.139
Noncore area				
Median	$46,863.8 (n = 230)	$39,820.2 (n = 739)	7043.6	0.000
White median	$49,182.5 (n = 229)	$43,259.1 (n = 740)	5906.4	0.000
Latino median	$40,203.8 (n = 147)	$37,907.8 (n = 474)	2123.9	0.187

Source: Authors' analysis of 2015 American Community Survey 5-year data, See Bureau of the Census, "American Community Survey: 2015 Data Release Is Flexible," available at http://www.census.gov/programs-surveys/acs/news/data-releases/2015/release-schedule.html (last accessed January 2017).

Table 4 reports the differences in poverty, white poverty, and Latino poverty between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications. As the table shows, poverty, white poverty, and Latino poverty is generally lower across the range of urban-rural classifications in sanctuary counties compared to nonsanctuary counties.

Sanctuary counties in noncore, rural counties see the most significant differences in poverty, white poverty, and Latino poverty. Latino poverty is 5.7 percent lower ($p < .001$) in noncore, rural sanctuary counties compared to noncore, rural nonsanctuary counties, and white poverty is 3.2 percent lower ($p < .001$).

TABLE 4

Poverty rates are generally lower in sanctuary counties

Comparing poverty rates across sanctuary counties and nonsanctuary counties

	Sanctuary	Nonsanctuary	Difference	p-value
Large central metropolitan area				
Poverty rate	16.7% (n = 27)	16.8% (n = 35)	-0.003	0.821
White poverty rate	9.8% (n = 27)	9.6% (n = 35)	0.002	0.759
Latino poverty rate	24.7% (n = 27)	26.6% (n = 35)	-0.019	0.257
Large fringe metropolitan area				
Poverty rate	10.9% (n = 76)	12.5% (n = 238)	-0.014	0.031
White poverty rate	8.4% (n = 76)	9.7% (n = 238)	-0.013	0.015
Latino poverty rate	20.5% (n = 76)	21.9% (n = 238)	-0.014	0.408
Medium metropolitan area				
Poverty rate	15.8% (n = 74)	16.5% (n = 251)	-0.007	0.285

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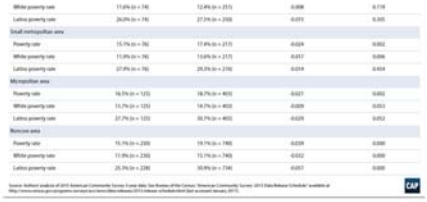

Table 5 reports the differences in public benefits usage between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications. As the table shows, the percentage of households that receive SNAP benefits, formally known as food stamps; the percentage of households that receive SSI; and the percentage of children under 18 who live in households that receive public assistance are generally lower across the range of urban-rural classifications in sanctuary counties compared to nonsanctuary counties.

Sanctuary counties in noncore, rural counties see the most significant differences in public benefits usage. The percentage of households that receive SNAP benefits is 4.9 percent lower ($p < .001$) in noncore, rural sanctuary counties compared to noncore, rural nonsanctuary counties. The percentage of households that receive SSI is 1.9 percent lower ($p < .001$), and the percentage of children under 18 who live in households that receive public assistance is a full 9.0 percent lower ($p < .001$).

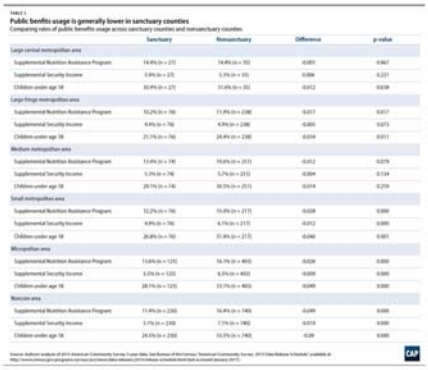

Table 6 reports the differences in labor force participation, white labor force participation, and Latino labor force participation between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications.

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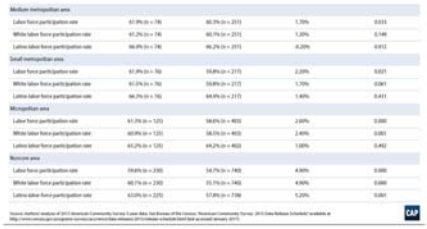


Table 7 reports the differences in the employment-to-population ratio, the white employment-to-population ratio, and the Latino employment-to-population ratio between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications.

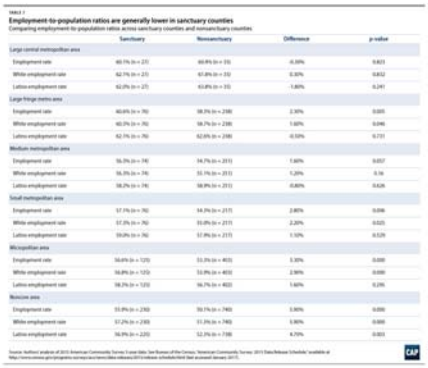
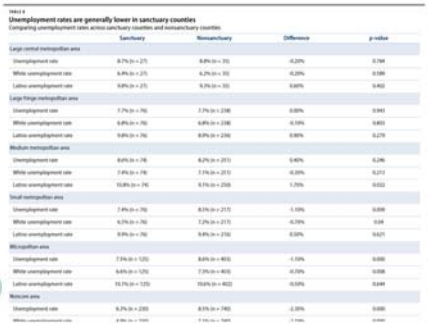


Table 8 reports the differences in unemployment, white unemployment, and Latino unemployment between sanctuary counties and nonsanctuary counties when distinguishing between urban-rural classifications.



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1 More specifically, we define sanctuary counties as counties that do not assist federal immigration enforcement officials by holding people beyond their release date on the basis of immigration detainers. Nonsanctuary counties are those that comply with immigration detainer requests.

2 Lena Graber and Nikki Marquez, "Searching for Sanctuary: An Analysis of America's Counties & Their Voluntary Assistance With Deportations" (San Francisco: Immigrant Legal Resource Center, 2016), available at https://www.ilrc.org/sites/default/files/resources/sanctuary_report_final_1-min.pdf.

3 Ibid.

4 American Civil Liberties Union, "Recent ICE Detainer Cases," available at https://www.aclu.org/other/recent-ice-detainer-cases (last accessed January 2017).

5 International Association of Chiefs of Police, "Enforcing Immigration Law: The Role of State, Tribal and Local Law Enforcement," available at https://www.theiacp.org/portals/0/pdfs/publications/immigrationenforcementconf.pdf (last accessed January 2017).

6 Craig E. Ferrell, Jr. and others, "M.C.C. Immigration Committee Recommendations For Enforcement of Immigration Laws by Local Policy Agencies" (Charlotte, NC: Major Cities Chiefs Association, 2006), available at https://www.majorcitieschiefs.com/pdf/news/MCC_Position_Statement.pdf.

7 U.S. Department of Homeland Security, "Immigration Detainer – Notice of Action," https://www.ice.gov/doclib/secure-communities/pdf/immigration-detainer-form.pdf (last accessed January 2017).

8 See, U.S. Immigration and Customs Enforcement, "Secure Communities," available at https://www.ice.gov/secure-communities (last accessed January 2017).

9 Transactional Records Access Clearinghouse, "Reforms of ICE Detainer Program Largely Ignored By Field Officers," August 9, 2016, available at https://trac.syr.edu/immigration/reports/433/.

10 Letter from Jeh Charles Johnson to Thomas S. Winkowski, Megan Mack, and Philip A. McNamara, "Secure Communities," November 20, 2014, available at https://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf.

11 See, for example: Laurence Benenson, "The Trouble With Immigration Detainers," National Immigration Forum, May 24, 2016, available at https://immigrationforum.org/blog/the-trouble-with-immigration-detainers/.

12 American Civil Liberties Union, "Galarza v. Szalczyk," available at https://www.aclu.org/cases/immigrants-rights/galarza-v-szalczyk?redirect=immigrants-rights/galarza-v-szalczyk (last accessed January 2017).

13 *See Morales v. Chadbourne*, 996 F. Supp. 2d 19 (D. R.I.), *affirmed on appeal*, 2015 WL4385945 (1st Cir. 2015) (holding that plaintiff stated a Fourth Amendment claim where she was held for 24 hours on an ICE detainer issued without probable cause); *Galarza v. Szalczyk*, No. 10-6815, 2012 WL 1080020, at *10, *13 (E.D. Pa. Mar. 30, 2012) (unpub.) (holding that where plaintiff was held for 3 days after posting bail based on an ICE detainer, he stated a Fourth Amendment claim against both federal and local defendants; it was clearly established that the "detainer caused a seizure" that must be supported by "probable cause"), *rev'd on other grounds*, 745 F.3d 634 (3d Cir. 2014) (holding that the County operating the jail, too, may be liable for violating the Fourth Amendment); *Miranda-Olivares v. Clackamas County*, No. 12-02317, 2014 WL 1414305, at *10 (D. Or. Apr. 11, 2014) (holding that plaintiff's detention on an ICE detainer after she would otherwise have been released "constituted a new arrest, and must be analyzed under the Fourth Amendment;" and resulting in a settlement in the amount of $30,100); *Mendoza v. Osterberg*, No. 13-65, 2014 WL 3784141, at *6 (D. Neb. July 31, 2014) (recognizing that "(t)he Fourth Amendment applies to all seizures of the person," and thus, "(i)n order to issue a detainer(,) there must be probable cause") (internal quotation marks, ellipses, and citations omitted); *Villars v. Kubiatowski*, 45 F.Supp.3d 791 (N.D. Ill. 2014) (holding that plaintiff stated a Fourth Amendment claim where he was held on an ICE detainer that "lacked probable cause," and resulting in settlement as to local defendants); *Uroza v. Salt Lake County*, No. 11-713, 2013 WL 6230688, at *5-6 (D. Ut. Feb. 21, 2013) (holding that plaintiff stated a Fourth Amendment

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claim where ICE issued his detainer without probable cause; finding it clearly established that "immigration enforcement agents need probable cause to arrest . . . (and) detainees who post bail should be set free in the absence of probable cause to detain them again," and resulting in settlement as to local defendants in amount of $75,000)

14 Lena Graber, Angie Junck, and Nikki Marquez, "Local Options for Protecting Immigrants" (San Francisco: Immigrant Legal Resource Center, 2016,) available at .

15 San Francisco's City and County Refuge Ordinance provides an example. The ordinance reads, "No department, agency, commission, officer or employee of the City and County of San Francisco shall use any City funds or resources to assist in the enforcement of federal immigration law or to gather or disseminate information regarding the immigration status of individuals" unless required by federal statute or court decision. See: City and County of San Francisco, "San Francisco Administrative Code Chapter 12H: Immigration Status," Sec. 12H.2, available at (last accessed January 2017).

16 After data cleaning and removing duplicates.

17 See, Centers for Disease Control and Prevention, "NCHS Urban-Rural Classification Scheme for Counties," May 6, 2014, available at .

18 See, Federal Bureau of Investigation, "Uniform Crime Reporting," available at (last accessed January 2017).

19 U.S. Census Bureau, "American Community Survey: 2015 Data Release Schedule," August 8, 2016, available at .

20 Stefano M. Iacus, Gary King, and Guiseppe Porro, "Causal Inference without Balance Checking: Coarsened Exact Matching," Political Analysis, August 23, 2011, available at .

21 Models are also estimated while controlling for the natural log of the total population, population squared, the total foreign-born population, and the total foreign-born population squared.

22 Because the coding of sanctuary jurisdictions is relatively recent—January 2014—it is difficult to compare outcomes before and after a jurisdiction began declining detainers or requests for notifications (e.g., a difference-in-differences design).

23 U.S. Census Bureau, "Labor Force Statistics: ACS Employment Status Data by Block Group, 2006-2010," (last accessed January 2017).

24 U.S. Department of Labor, "Labor Force Statistics from the Current Population Survey," available at (last accessed January 2017).

25 U.S. Census Bureau, "Labor Force Statistics: ACS Employment Status Data by Block Group, 2006-2010."

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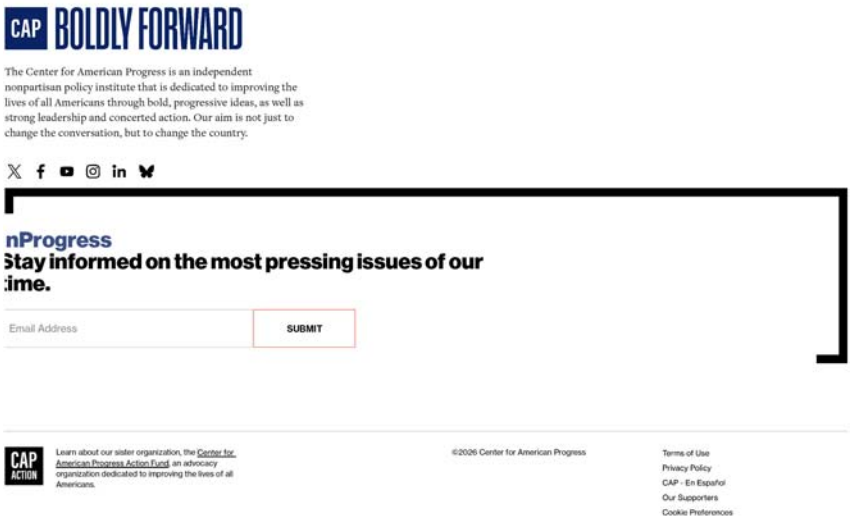
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Do sanctuary policies increase crime? Contrary evidence from a county-level investigation in the United States

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Abstract

Immigration and Customs Enforcement (ICE) has rolled out a series of programs that leverage local and state resources to detain and deport undocumented immigrants. There is little understanding, however, about the public safety consequences of mobilizing local police to enforce immigration law. I use ICE administrative records, Uniform Crime Reports, and American Community Survey population estimates to investigate whether and under what circumstances local immigration enforcement is associated with property crime and violent crime. Results show that crime trends in sanctuary and non-sanctuary counties were not significantly different in the first decade of the 2000s. However, after the proliferation of sanctuary practices around 2014, both property crime and violent crime decreased more in sanctuary counties than non-sanctuary counties, net of other predictors of crime. Further, a pooled cross-sectional analysis of 2013–2016 data shows that sanctuary practices strengthen the inverse relationship between proportion foreign-born Latino and property crime, and reverse the positive relationship between proportion native-born Latino and property crime. I theorize that this occurs because sanctuary practices encourage immigrant political integration, have positive spillover effects to non-immigrant Latinx communities, and increase *social harmony*.

Introduction

Policy makers and researchers in the United States have increasingly been concerned with the entanglement of immigration control with criminal-legal systems (Gupta and Altman 2021; Kohli et al. 2011; Miles and Cox 2014; Stumpf 2006; Treyger et al. 2014). This became a more pressing issue in 2009, when Immigration and Custome Enforcement (ICE) launched the Secure Communities, a program that linked FBI databases with Department of Homeland Security databases to automatically check the citizenship status of anyone arrested, anywhere in the country (Cox and Miles 2013). By January 2013, ICE had activated such data-sharing technology in all state and local jails in the country, allowing ICE to track potential non-citizens, and request law enforcement agencies to detain potential non-citizens for questioning, or be notified upon their release (ICE, 2018:201; Meissner et al., 2013). These are called detainer requests and notification requests, respectively.

By 2014, however, over 200 jurisdictions had withdrawn from participating in Secure Communities (ICE 2015; Ramos 2014). In practice, this meant declining detainer and notification requests (Hausman 2020). Jurisdictions that declined to participate cited concerns that Secure Communities would penalize immigrant victims or witnesses of crimes and thus undermine community-police relationships (Chen 2016). Other concerns included morality, legality, and legitimacy of the program (Chen 2016). Declining notifications and detainers is one important way in which local government restricts police officers enforcement of immigration laws. Other types of initiatives include barring investigations into immigration violations, limiting ICE's access to jails, and limiting disclosure of information (Lasch et al., 2018).

Although Secure Communities was designed to improve public safety, there is no evidence that crime has decreased as a result (Miles and Cox 2014; Treyger et al. 2014). Emerging work on the consequences of immigration enforcement has found that immigrant-friendly legislation, referred to colloquially as sanctuary policies, has either a null or negative association with violent crime and property crime (Amuedo-Dorantes and Deza 2019; Gonzalez-O'Brien et al., 2019; Hausman 2020; Kubrin and Bartos 2020; Lyons et al. 2013; Martínez-Schuldt and Martínez 2019, 2019; Wong 2017). This relationship holds despite researchers operationalizing "sanctuary" in different ways, examining varying units of analysis, at different points in time, and using different analytical techniques (Martínez et al. 2018). Although these studies point to the potential for sanctuary policies to decrease crime, a broader assessment that includes smaller jurisdictions is missing. The current study fills this gap and adds an analysis of whether and to what extent the proportion of Latinos moderates sanctuary policies effect on crime, and an improved measure of sanctuary *practices*, which is a better indicator of actual rather than intended enforcement.

I assembled an original dataset using administrative records and survey data to investigate whether and under what circumstances non-compliance with ICE, which I will refer to as sanctuary practices, is associated with aggregate property crime and violent crime. I also test whether the proportion of foreign-born Latinos and the proportion of native-born Latinos moderate sanctuary

practices effect on crime. This is important because foreign-born Latinos are the population most targeted by immigration enforcement, and because of the mounting evidence that the consequences of immigration enforcement spillover to native-born Latinos (Asad 2020; Barranco and Shihadeh 2015; Elizabeth Aranda, Cecilia Menjívar, and Katharine M. Donato 2014; Martinez-Aranda 2020; Menjívar et al. 2018).

Through a series of negative binomial regression models, I find that crime trends in sanctuary and non-sanctuary counties were not significantly different in the first decade of the 2000s. However, using a conditional fixed effects negative binomial regression model, I find that sanctuary practices are associated with a decrease in both property crime and violent crime within counties over time, findings that are in the same direction, but slightly weaker, when using an unconditional fixed effects model. Further, a pooled cross-sectional analysis of 2013–2016 data shows that sanctuary counties strengthen the inverse relationship between proportion of foreign-born Latino and property crime, and reverse the positive relationship between proportion native-born Latino and property crime. Building on prior research, I theorize that this occurs via three principle mechanisms: immigrant political integration, positive spillover effects of sanctuary practices, and increased *social harmony*. First, immigrant political integration efforts, like expanding immigrants' rights through sanctuary practices, are likely to be adopted at the tail end of immigrant integration and incorporation more broadly (Marrow 2009) and in areas where sheriffs have more positive attitudes towards immigrants (Farris and Holman, 2017). Immigrant-friendly policies and practices encourage more full participation in social, political, and economic life, which increases social organization and is protective against crime (Kubrin and Weitzer 2003; Lyons et al., 2013; Martínez-Schuldt and Martínez 2019; Sampson and Byron Groves, 1989). Second, there is abundant evidence that because of the racialization of illegality (García, 2017, Menjívar et al., 2018) restrictive immigration policies "spill over" to negatively impact native-born Latinos. Thus, efforts to bolster social and political incorporation of immigrants might spill over in positive ways to impact native-born people and communities that have been racialized as illegal despite holding U.S. citizenship.

Third, *social disharmony* is a Du Boisian analytical concept recently re-articulated by Werth (2022) that provides a generative lens through which to make sense of my findings. Social disharmony was developed by W.E.B. Du Bois, and, complements social disorganization theories by focusing on racial and economic exclusion, paired with racialized systems of oppression as causes of crime, rather than eroded mechanisms of informal social control prioritized by social disorganization theorists (Werth 2022). Thus, in addition to bolstering social organization, I theorize that sanctuary practices may help decrease crime by beginning to dismantle racialized systems that marginalize immigrants and people of color.

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Section snippets

Secure Communities and spillover effects

The Secure Communities Program is the most extensive federal-local collaboration to detain and deport undocumented immigrants in US history. The federal government had made prior efforts to mobilize state and local law enforcement agencies to enforce immigration issues, but the scale and scope of the current effort at interior immigration enforcement is unprecedented. Most importantly, once activated, the data sharing component of Secure Communities does not require the buy-in or additional ...

Measures

In order to investigate whether and to what extent sanctuary practice are associated with crime, I assembled an original dataset drawing on multiple sources at the county-level. The main dataset includes 3,105 counties for a total of 12,420 county-year observations from 2013 to 2016, which are over 98% of all 3,143 counties and county-equivalents in the United States. Counties in Alaska, Hawaii, and outlying islands are not included. This period begins in 2013 when the Secure Communities ...

Results

Evidence from this study shows that crime trends in sanctuary and non-sanctuary counties were not significantly different in the first decade of the 2000s. However, an analysis of trends over time shows that sanctuary practices, in the form of declined notification and detainer requests, lead to a post-hoc decline in crime rates. Further, a pooled cross-sectional analysis of 2013–2016 data shows that sanctuary counties strengthen the inverse association between proportion foreign-born Latino ...

Discussion

Scholars and practitioners working in the field of immigration have been increasingly concerned with the linking of criminal and immigration-related databases through restrictive immigration initiatives. While these linkages purport to improve public safety by identifying and removing deportable criminals, there is little evidence that public safety has increased as a result. In fact, evidence from this study shows the contrary, that expansive immigration policies and practices, not restrictive ...

Conclusion

In conclusion, although it was the Trump administration that most forcefully vilified sanctuary policies for protecting criminals and increasing crime, former president Obama also famously stated that immigration enforcement should focus on deporting immigrant "felons and not families" (León 2015). This false dichotomy between the "bad" immigrant felon and the "good" immigrant family reproduces a problematic narrative that sees immigrants as a threatening presence that need to be identified, ...

Recommended articles

References (109)

Raymond E. Barranco *et al.*
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Providing Sanctuary or Fostering Crime? A Review of the Research on "Sanctuary Cities" and Crime

Daniel E. Martínez[1] | Ricardo D. Martínez-Schuldt[2] | Guillermo Cantor[3]

[1] University of Arizona

[2] University of North Carolina, Chapel Hill

[3] American Immigration Council

Correspondence
Daniel E. Martínez, University of Arizona, United States.
Email: daniel.martinez@arizona.edu

Abstract

Recent high-profile, isolated incidents of violent crime committed by deportable noncitizens have led to increased public attention paid to so-called "sanctuary" cities, with some policymakers calling for the eradication of policies limiting local officials' role in the enforcement of immigration law. This current public and political debate provides an opportunity to critically examine the existing literature on immigrant "sanctuaries." We begin by offering a broad definition and description of "sanctuary" policies. We follow by discussing how and why such policies have evolved since the early 1980s. Considering the public safety concerns often articulated in contemporary political discourse, we then offer possible sociological explanations regarding how these policies might either be positively or negatively associated with crime. We subsequently highlight findings from existing empirical research that examines the relationship between the adoption or presence of "sanctuary" policies and crime. The few empirical studies that exist illustrate a "null" or negative relationship between these policies and crime. We conclude by offering possible directions for future research.

1 | INTRODUCTION

The 1986 Immigration Reform and Control Act (IRCA) remains the last major inclusionary comprehensive immigration reform passed in the United States. Although IRCA regularized the immigration statuses of approximately three million people by 1992, it also served as a catalyst for expanded border and interior immigration enforcement (Massey, Durand, & Malone, 2003). In the late 1990s, seasonal migration declined due to increased border enforcement, which in turn led to a dramatic increase in the unauthorized population residing in the United States as fewer migrants returned to their countries of origin. By 2014 the unauthorized population stood around 11.1 million people, 52% of which migrated from Mexico (Passel & Cohn, 2016). Unfortunately for this group and their mixed-status family members, the U.S. Congress has failed to pass comprehensive immigration reform in over 30 years.

Congress' inability to pass immigration reform coincides with major transformations in the way immigration laws are enforced. Whereas the federal government and its relevant agencies—currently, U.S. Immigration and Customs Enforcement (ICE) and U.S. Customs and Border Protection (CBP)—have traditionally been accountable for enforcing

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immigration laws, recent policies and practices, made possible by changes to the Immigration and Nationality Act in 1996, shifted a share of the responsibility to state and local officials. Since 1996, broad enforcement initiatives requiring increased cooperation between the federal government and state and local law enforcement resulted in an expansion of the government's capacity to place individuals in removal proceedings. These enforcement initiatives include the Secure Communities program and the Criminal Alien Program. Additionally, the 287(g) program allowed ICE to enter agreements with local and state enforcement agencies authorizing designated officers to perform immigration enforcement activities. Moreover, pressure mounted in the era following the 9/11 terrorist attacks to further involve state and local law enforcement officials in the policing of immigration laws (Meissner, Kerwin, Chrishti, & Bergeron, 2013). Scholars, legal experts, and nongovernmental organizations have criticized these efforts and raised concerns regarding the legality of the programs, public safety, humanitarian considerations, issues of ethno-racial profiling, civil rights violations[1], and fiscal constraints at the local level (Dreby, 2012; Gardner & Kohli, 2009; Homeland Security Advisory Council, 2011; Lasch, 2013; Provine, Varsanyi, Lewis, & Decker, 2016; Wells, 2004; Wishnie, 2004).

Citing notable concerns, many communities across the country have adopted ordinances that explicitly limit cooperation with the federal government in enforcing immigration laws. These ordinances, colloquially known as "sanctuary" policies, are partly the result of a tension between the federal government's responsibility to enforce immigration laws and the state and local government's obligation to protect the well-being of all residentially present individuals (Wells, 2004). Although local communities adopt "sanctuary" policies to limit cooperation with the federal government, the implementation of these policies does not imply that noncitizens are protected from federal immigration enforcement action, leading some to suggest that the term "sanctuary" is a misnomer (Tramonte, 2011). In fact, many "sanctuary" jurisdictions allow for local assistance and cooperation in the enforcement of federal immigration laws when individuals have been convicted of violent or serious felony offenses[2].

However, "sanctuary" jurisdictions have faced criticism, particularly as related to "law and order" concerns. Public and political attention paid to "sanctuary" policies increased substantially in July 2015 after the tragic death of 32-year-old Kathryn Steinle. Steinle was shot and killed in San Francisco, a well-known "sanctuary" city. The alleged shooter was 45-year-old Juan Francisco López-Sánchez, an unauthorized immigrant from Mexico who had seven prior felony convictions and five deportations from the United States, mostly for nonviolent drug offenses (Littlefield, 2015).

Steinle's murder sparked a divisive political debate regarding the enforcement of immigration laws and the legitimacy of "sanctuary" policies. Following the tragic death of Steinle, several legislative proposals were introduced at the federal level that would increase penalties for unauthorized reentry and limit federal grants available to "sanctuary" cities. These initiatives have largely failed to materialize due to lack of political support. However, two recent versions of these bills successfully passed through the House of Representatives in June of 2017 and are currently waiting to be heard in the Senate (Marcos, 2017). In a similar vein, President Trump signed an executive order in January of 2017 that aims to strip federal funding from "sanctuary" jurisdictions (Executive Order 13768, 2017). Thus, the current public and political discourse surrounding unauthorized immigration in general—and deportable noncitizens more specifically—provides an opportunity to critically examine the existing literature on "sanctuary" policies.

In what follows, we provide a broad definition of so-called "sanctuary" policies and discuss how and why such policies have evolved since the early 1980s. Considering the public safety concerns often articulated in contemporary political discourse, we then offer possible sociological explanations of how these policies might either be positively or negatively associated with crime. We subsequently highlight findings from existing research that examines the association between the adoption or presence of "sanctuary" policies and crime. Our review of the literature identifies notable variation in the definition of "sanctuary" ordinances. In some cases, "sanctuary" policies are strictly defined as local efforts aimed at limiting cooperation with federal enforcement. In other cases, such policies are described as preconditions for immigrant inclusion at the local level. In terms of the relationship between "sanctuary" policies and crime, we find no evidence in the extant literature supporting the assertion that the implementation of such policies fosters crime. We conclude by offering possible directions for future research.

2 | A BRIEF HISTORY OF "SANCTUARY" POLICIES

In the early 1980s, hundreds-of-thousands of Central American immigrants fled violent conflict and political persecution in their countries of origin and sought refuge in the United States. The U.S. federal government often rejected Central American asylum applications during this period. In response, religious organizations began offering "sanctuary" in their places of worship to protect immigrants from deportation and provided other forms of assistance including food, clothing, housing, and social or legal services (Bau, 1994; Ridgley, 2008; Villazor, 2008). This led to a so-called "Sanctuary Movement" throughout the United States in the 1980s (Bilke, 2009; Garcia, 2009; Gregorin, 2011; Villazor, 2010). Following the lead of religious congregations and private individuals, localities enacted municipal ordinances stipulating that city officials would not cooperate with Immigration and Naturalization Service—the agency that administered federal immigration laws and regulations until 2003—in the arrest or deportation of noncitizens in the wake of IRCA, which expanded immigration enforcement into the interior United States (Bau, 1994; Ridgley, 2008).

"Sanctuary" policies continued to expand at the local level with the implementation of federal reforms, including the 1996 Illegal Immigration Reform and Responsibility Act (IIRIRA), the 1996 Antiterrorism and Effective Death Penalty Act, and more recently, the "War on Terror" (Bilke, 2009; Ridgley, 2008). The '96 laws and the War on Terror led to the devolution of immigration enforcement from the federal to the state and local levels (Coleman, 2007; Stumpf, 2006). These pieces of legislation expanded the list of deportable offenses, expanded the category of "aggravated felony," which is a felony class that only applies to noncitizens, beyond murder and trafficking of drugs or firearms[3], and limited "the legal review of immigration and criminal charges brought against undocumented migrants" as well as other immigrants (Coleman, 2007, p. 56).

Additional localities adopted "sanctuary" ordinances during the 2000s in response to federal initiatives such as the 287(g) program, Secure Communities, and the Criminal Alien Program. Whereas 287(g) deputizes local law enforcement officials to enforce immigration law (Meissner et al., 2013), Secure Communities checks the fingerprints of individuals who encounter local law enforcement agents against federal electronic databases (Kubrin, 2014; Waslin, 2011). Similarly, the Criminal Alien Program aims to identify allegedly deportable noncitizens incarcerated in correctional facilities. The Criminal Alien Program typically involves an interview or screening (i.e., an "encounter") of an individual while they are incarcerated to determine whether the individual is potentially removable. The encounter may result in ICE's placing a hold or detainer on the individual (American Immigration Council, 2013).

Because of 287(g), Secure Communities, and the Criminal Alien Program, some localities with existing "sanctuary" policies responded by further limiting their cooperation with federal authorities on matters of immigration enforcement. According to the Immigrant Legal Resource Center, four states, 364 counties, and 39 cities have policies that to some degree limit their cooperation with requests from ICE to hold noncitizens in detention.[4] In addition, the National Immigration Law Center (NILC) notes that 65 cities or local law enforcement agencies have adopted "sanctuary" ordinances (more broadly defined) in some form since 1979, with the majority enacted between 2000 and 2008[5]. In sum, although "sanctuary" policies were initially developed to protect Central American refugees in the 1980s, they have since expanded to limit the role of local law enforcement agencies in the enforcement of federal immigration laws (Ridgley, 2008).

3 | CONTEMPORARY "SANCTUARIES"

How do contemporary "sanctuaries" differ from those established during the earlier Sanctuary Movement in the 1980s? To address this question, Villazor (2008) draws important distinctions between "private" and "public" sanctuaries. Private sanctuaries include religious congregations or nongovernmental organizations that offer food, shelter, or other forms of aid to unauthorized immigrants, while public sanctuaries consist of localities that "limit government employees, particularly local police officers, from inquiring or disseminating information about the immigration status

of immigrants whom they encounter" (Villazor, 2008, p. 148). Given the current political debate surrounding the alleged association between "sanctuaries" and public safety, we use the term "sanctuary" to identify what Villazor refers to as "public sanctuaries."

There is no official federal designation for what constitutes a "sanctuary" (Armenta, 2017; Garcia, 2009). For instance, President Trump's January 2017 executive order entitled "Enhancing Public Safety in the Interior of the United States" describes "sanctuary" jurisdictions as locales that refuse to comply with federal statute 8 U.S.C. 1373, which restricts jurisdictions from adopting policies that prohibit government entities or personnel from exchanging information related to individuals' immigration statuses with ICE or CBP (Executive Order 13768, 2017). On the other hand, the U.S. Department of Justice (DOJ) describes the term as a "jurisdiction that may have state laws, local ordinances, or departmental policies limiting the role of local law enforcement agencies and officers in the enforcement of immigration laws" (2007, footnote 44). As such, the DOJ's broader definition is perhaps a more accurate conceptualization of the ways in which "sanctuary" jurisdictions operate.

To account for variation across geography, some scholars use broader terms such as "sanctuary policy" or "ordinance" instead of "sanctuary city" (Price, 2014; Ridgley, 2008; Walker & Leitner, 2011). Despite this nuance, "sanctuary policies" generally fall into one of three categories: "(1) 'don't ask' policies that limit inquiries related to nationality or immigration status; (2) 'don't enforce' policies that limit immigration-related arrests or detentions; and (3) 'don't tell' policies that limit what information enforcement offices may share with federal officials" (Kittrie, 2006, as cited by Sullivan, 2009, p. 574). However, it is not uncommon for localities to use a combination of these approaches. Because "sanctuary" policies vary across geography (Kittrie, 2006; Pearson, 2015; Sullivan, 2009) as well as in terms of the protections they provide to immigrants, we use the term "limited cooperation policy" in lieu of "sanctuary city" or "sanctuary policy" throughout the remainder of this article.

4 | POTENTIAL LINKS BETWEEN LIMITED COOPERATION POLICIES AND CRIME

In the proceeding sections we identify the possible relationships between limited cooperation policies and crime. For the sake of parsimony, we present the dominant perspectives of both opponents and supporters of limited cooperation policies. Nevertheless, we must emphasize that the implementation of limited cooperation policies at the local level is not merely about whether a particular locality is "pro-immigrant" or "anti-immigrant." This would be an oversimplification, as some jurisdictions do not honor immigration detainers due to concerns over potential civil rights violations as well as financial constraints at the local level (Kittrie, 2006). In other words, a local jurisdiction may not comply with immigration detainers simply because it is not in their best legal or financial interest. However, our aim here is to capture some of the core concerns stemming from opposing sides of the debate.

4.1 | Arguments against limited cooperation policies

Limited cooperation policies have become a heavily contested political and legal issue in recent years. Drawing on a sociological perspective, we explore the possible consequences of limited cooperation policies as well as the logics that link these policies with public safety outcomes. As mentioned, some contemporary opponents often view limited cooperation policies as a public safety issue. In fact, President Trump's executive order, "Enhancing Public Safety in the Interior of the United States," claims that "Sanctuary jurisdictions across the United States willfully violate Federal law[6] [...]. These jurisdictions have caused immeasurable harm to the American people and to the very fabric of our republic" (Executive Order 13768, 2017). However, the executive order does not specify exactly how, if at all, "sanctuary" jurisdictions threaten public safety. Below we discuss a few ways in which limited cooperation policies could potentially be linked to increased crime.

Explanations that hypothesize a positive correlation between limited cooperation policies and crime point toward three notable types of transformation within immigrant communities: behavioral changes, compositional

changes, or changes in the relative population size (see Treyger, Chalfin, & Loeffler, 2014 for similar discussions in the context of the Secure Communities program). Hypothetically, the adoption of limited cooperation policies could bring about *behavioral* changes in the local immigrant population by increasing the likelihood that individuals will engage in crime. For example, noncitizens may perceive there to be a lower risk of deportation in an area where officials refuse to cooperate with the federal government's immigration enforcement agencies. If the risk of deportation or removal from the country serves as a deterrent for immigrant-related crime, then noncitizens may be more inclined to engage in crime in the absence of that threat. Although intuitive, the "behavioral change" hypothesis suffers from two notable weaknesses. First, some have suggested that noncitizens may not fully understand the nuances of limited cooperation policies, especially in contexts where the federal and local governments possess opposing policy orientations towards immigrants (e.g., if a local government is more receptive or inclusive of immigrants compared to more punitive federal enforcement actions) (Kittrie, 2006). Thus, noncitizens are unlikely to substantially alter their behavior across jurisdictions. Second, and more important, even if noncitizens have perfect understandings of limited cooperation policies, local governments often make exceptions for violent and serious felony offenses. Therefore, the most serious (e.g., publicly harmful) offenses should have similar risks of punishment (e.g., removal) across all jurisdictions.

A second possible explanation for the positive association between limited cooperation policies and crime is that such policies bring about *compositional* shifts in local immigrant populations. That is, the existence of limited cooperation policies could *attract* noncitizens that are most likely to engage in crime on day-to-day basis. If the proportion of criminally-inclined noncitizens increased in a specific area, then we would expect crime rates to also increase net of other factors. Obviously, this line of reasoning, like that of the "behavioral change" perspective, assumes that noncitizens across the United States are aware of specific jurisdictions with limited cooperation policies but are not sufficiently informed to understand that such policies do not provide true "sanctuary." In addition, these lines of argument also generally associate immigrants with criminality. Nevertheless, a large body of research relying on both self-report surveys and official statistics reveals that immigrant adults and youths, relative to the general U.S. population, have lower levels of criminality and are consequently less likely to engage in criminal or delinquent behavior (Powell, Perreira, & Harris, 2010; Rumbaut & Ewing, 2007; Ewing, Martinez, & Rumbaut, 2015; Bersani, Loughran, & Piquero, 2014; Vaughn, Salas-Wright, Delisi, & Maynard, 2014).

A third potential connection between limited cooperation policies, immigration, and crime stems from the social disorganization framework. According to this framework, dense community social and institutional network structures inhibit crime and delinquency by fostering informal social control mechanisms such as the supervision of adolescents (Sampson, 2011; Shaw & McKay, 1969). However, structural factors like poverty or residential instability prevent, disrupt, and erode social network ties and weaken local institutions. When social and institutional networks are fractured, communities become characterized by social disorganization, or the inability of the community to maintain order (Sampson, 2011). As such, community crime is expected to increase under the condition of social disorganization (Shaw & McKay, 1969). Thus, even if immigrants have lower levels of criminality—as suggested by much of the extant literature—this framework would suggest that a rapid increase in the immigrant population may burden local institutions, deplete their resources, and limit new and existing community members' ability to effectively communicate and form or maintain social relationships. In other words, rapid social change—stemming from immigration—may destabilize communities and negatively impact informal and formal social control mechanisms resulting in increased crime (Shaw & McKay, 1969; Stowell, 2007).

A substantial body of research tests hypotheses derived from the social disorganization framework that link immigration to crime at aggregate levels. For example, Martinez, Stowell, and Cancino (2008) examined patterns in homicide in neighborhoods, defined as census-tracts, in the cities of San Diego and San Antonio. The authors found that the concentration of recent immigrants is negatively associated with homicide in San Diego neighborhoods overall but found a null relationship in Latino neighborhoods specifically. Similarly, Martinez and colleagues found

no evidence of a relationship between immigrants and crime in San Antonio across Latino communities and in communities overall. These results fail to support the expectations of social disorganization as it relates to immigration and crime.

Some scholars contend that research fails to find evidence in support of the social disorganization framework because researchers predominantly examine the immigration-crime link in traditional immigrant designations like San Diego, San Antonio, or other large metropolitan gateways (Shihadeh & Barranco, 2010). This position argues that traditional destinations possess institutional and social support structures that can readily accommodate influxes of new immigrants and protect against social disorganization. In contrast, newer immigrant destinations, such as cities and counties in North Carolina, lack necessary institutional support networks and are disproportionately likely to experience weakened social organization and thus an increase in violence (Shihadeh & Winters, 2010). However, much of the evidence supporting the claim that immigration leads to social disorganization in new destinations comes from analyses of cross-sectional data, which may suffer from omitted variable bias (Light, 2017). These studies are also limited in that they may not correctly model the temporally dynamic relationship between immigration and crime that is theoretically expected based on the social disorganization framework (Ousey & Kubrin, 2017).

A recent meta-analysis by Ousey and Kubrin's (2017) sheds further light on the question of whether or not there is a macro-level relationship between immigration and crime as well as the degree to which the relationship unfolds over time or differs between contexts of reception. The authors consider a sample of over 50 studies examining the immigration-crime link, which includes over 500 point estimates of an immigration effect. After controlling for various dimensions of study design, their meta-analysis found an average negative—albeit weak—effect of immigration on crime. Furthermore, while there was no significant average relationship between immigration and crime in studies of new destination contexts and for studies that relied on cross-sectional methods, there was a significant *negative* effect of immigration on crime in traditional destinations and in studies that utilized longitudinal analytic designs. These results, coupled with the evidence at the individual-level, strongly suggest that an influx of immigrants *does not* increase crime in communities across the United States, despite the rhetoric presented by opponents of limited cooperation policies.

4.2 | Arguments in favor of limited cooperation policies

Supporters of limited cooperation policies contend that such policies potentially heighten community safety and protect *all* members from criminal victimization. For example, limited cooperation policies may strengthen community-police relations because noncitizens do not feel at risk for removal when they interact with police, including when they are victims of violent crime. As such, noncitizens and immigrant communities may be more willing to seek assistance from law enforcement officials or cooperate with criminal investigations (Sullivan, 2009). A reduction in crime in the context of greater crime reporting and police cooperation may seem counterintuitive. Crime rates, as reflected in official statistics such as the FBI's Uniform Crime Reports, could increase if more crimes are reported to police. However, greater cooperation with police should facilitate the clearance of crimes by arrest, which has been shown to incapacitate and deter would-be criminals (Levitt, 1998). Limited cooperation policies, according to this logic, may contribute to a *reduction* in crime over time through improved cooperation with police and increased arrests.

Lyons, Vélez, and Santoro (2013) situate limited cooperation policies within a broader context of what they describe as "immigrant political opportunities" and argue that localities with "open political opportunities can generate a 'spiral of trust' that improves communication between officials and immigrants, promotes legislation protecting immigrant interests, and generates system-level trust in government on the part of immigrant groups" (p. 609-610). A "spiral of trust" may also be effective in increasing public and informal social controls that facilitate "collective crime control efforts" (Carr, 2005, as cited by Lyons et al., 2013, p. 610). This latter point is particularly important, as

criminologists have highlighted the importance of social organization and collective efficacy in reducing neighborhood crime (Sampson & Graif, 2009; Sampson, Raudenbush, & Earls, 1997).

5 | EXTANT EMPIRICAL RESEARCH ON "SANCTUARY" CITIES AND CRIME

Despite well-publicized political debates offering various forms of conjecture, only four empirical studies to our knowledge systematically examine the relationship between limited cooperation policies and crime (Lyons et al., 2013; Wong, 2017; Gonzalez, Collingwood, & El-Khatib, 2017; Martínez-Schuldt & Martínez, n.d.). Although the authors of these studies conceptualize and operationalize "sanctuaries" differently, examine patterns in crime across different units of analyses, rely on diverse analytic techniques, and focus on distinctive points in time, none of the studies support the claim that "sanctuaries" are more crime-prone than non-sanctuaries. In what follows, we provide a brief review of these four studies.

Wong (2017) compares the overall violent and property crime rates for demographically matched "sanctuary" and "non-sanctuary" counties across the United States. Wong defines "sanctuary" jurisdictions as counties that do not accept immigration detainer requests from ICE and categorizes counties by relying on data provided by ICE through a Freedom of Information Act request. Wong draws on the American Community Survey (ACS)'s 2015 5-year estimates to match "sanctuary" and "non-sanctuary" counties along demographic characteristics (e.g., population size, percentage of the population that is Latino, percent of the population that is foreign-born, etc.). Lastly, Wong derives violent and property crime rates from the FBI's Uniform Crime Reports (UCR). After matching counties and comparing their crime rates, Wong finds that large central metropolitan counties, micropolitan counties, and rural counties with "sanctuary" policies are *safer* than their "non-sanctuary" counterparts. However, similarly matched "sanctuary" and "non-sanctuary" large fringe metropolitan counties, medium metropolitan counties, and small metropolitan counties have crime rates that are not statistically different. Unfortunately, Wong's cross-sectional approach only confidently reveals the existing differences (or similarities) between counties as opposed to longer-term consequences of limited cooperation approaches.

Gonzalez et al. (2017) offer an improvement over Wong's (2017) approach by employing a similar causal-inference matching strategy on U.S. cities in conjunction with multivariate regression (to account for imperfect matching). Specifically, the authors compare overall violent crime, overall property crime, and rape rates in U.S. cities before and after the implementation of "sanctuary" policies as well as between 2000 and 2014, more generally. Unlike Wong (2017), Gonzalez et al. (2017) rely on a more encompassing definition of "sanctuary" cities, and include any "city or police department that has passed a resolution or ordinance expressly forbidding city or law enforcement officials from inquiring into immigration status and/or cooperation with Immigration and Customs Enforcement (ICE)" (p. 2). Overall, Gonzalez et al. (2017) find that "sanctuary" cities do not experience any uniform shift in crime rates in the year following the implementation of a "sanctuary" policy for all three of their outcome measures. Furthermore, Gonzalez and colleagues find no statistically significant differences in crime rates between 2000 and 2014 for their matched cities.

Like Wong (2017), Gonzalez et al.'s (2017) analysis is not without limitations. For example, the authors compare overall violent and property crime rates at the expense of more refined and/or reliable measures (e.g., homicide). It is possible that the effects (positive or negative) of limited cooperation policies are specific to certain types of crime. On a more technical note, the authors employ a regression analysis (post-matching) to compare crime *rates*, opposed to incidents, which may also suffer from bias (Osgood, 2000).

Lyons et al. (2013) draw on data from the National Neighborhood Crime Study (NNCS) and use multi-level statistical techniques to examine the relationships between neighborhood factors, city-level characteristics, and neighborhood violent crime. Specifically, the authors examine the number of homicides and robberies that occur in census-tracts (a proxy for neighborhoods) in 2000. Although not the central focus of the analysis, Lyons et al. (2013) include a city-level measure for "sanctuary" city, which they created by drawing on data provided by NILC.

Lyons and colleagues find that immigrant concentration is associated with a reduction in neighborhood violence, a finding that has been routinely demonstrated in social scientific research and often described as the "immigrant revitalization perspective" (Martinez & Lee, 2000; Ousey & Kubrin, 2017). However, the authors find that the inverse relationship between neighborhood immigrant concentration and crime is stronger in "sanctuary" cities (municipalities with at least one law or formal resolution limiting local enforcement of immigration laws) compared to "non-sanctuary" cities. Lyons and colleagues suggest that the existence of "sanctuary" policies represent one dimension of the city-level concept of "immigrant political opportunities," which they define as "the political receptivity or vulnerability of cities to meeting the needs and demands of immigrant communities" (2013, p. 605). The authors suggest that immigrant political opportunities strengthen the inverse immigrant-crime relationship because there is greater communication and trust between immigrant communities and city institutions (Sandoval, 2009), an interpretation that closely mirrors the justifications proposed by supporters of "sanctuary" policies. We must note that Lyons et al.'s (2013) cross-sectional analysis suffers from limitations similar those discussed regarding Wong's (2017).

Lastly, Martínez-Schuldt and Martínez (n.d.) combine data from the FBI's Uniform Crime Reports, the U.S. Census, and the American Community Survey to examine the relationship between the implementation of limited cooperation policies and incidents of violent crime across 107 U.S. cities between 1990 and 2010. Martínez-Schuldt and Martínez conceptualize and operationalize limited cooperation policies in the same manner as Lyons et al. (2013) and Gonzalez et al. (2017). However, unlike previous approaches, they simultaneously examine variation in refined measures of violence (e.g., homicide and robbery) and utilize longitudinal multivariate regression techniques. Martínez-Schuldt and Martínez's results indicate that cities experience an initial decline in incidents of robbery following the passage of "sanctuary" ordinances, but no changes in homicide. Consistent with Lyons et al.'s (2013) expectations regarding "immigrant political opportunities," the authors also find that "sanctuary" polices strengthen the inverse relationship between immigrant concentration measures and both homicide and robbery.

The analyses presented by Martínez-Schuldt and Martínez too are not without limitations. For example, the authors rely on official measures of crime as opposed to measures that are less susceptible to reporting bias such as those from victimization surveys. Moreover, similar to previous research on limited cooperation policies and crime, Martínez-Schuldt and Martínez focus on general measures of violence instead of ethnoracial-specific measures. As such, they neglect the fact that ethnoracial minority groups are not equally affected by the structural conditions that foster crime and victimization (Peterson & Krivo, 2005). Future researchers may want to consider how, if at all, ethnoracial groups are differentially impacted by the passage of limited cooperation policies.

Ultimately, there is minimal empirical research on the "sanctuary" policies-crime relationship and the four existing studies yield mixed results. However, none of these studies suggest that limited cooperation policies foster crime. Instead, findings support the maintenance and expansion of limited cooperation policies, at least insofar as they foster positive immigrant community-police relations and other immigrant political opportunities. Moreover, as Lyons et al. (2013) suggest "By marginalizing newcomers, creating political cynicism, and instilling mistrust of the police and local authorities, hostile regimes may set in motion the very [criminogenic] processes they fear" (Lyons et al., 2013, p. 624).

6 | DISCUSSION

Findings from the robust literature on the immigration-crime link (Lee & Martinez, 2002; Lee, Martinez, & Rosenfeld, 2001; Martinez, Stowell, & Lee, 2010; Ousey & Kubrin, 2017; Rumbaut & Ewing, 2007; Sampson et al., 1997) as well as those from recent studies examining the relationships between limited cooperation policies and crime are clear (Lyons et al., 2013; Wong, 2017; Gonzalez et al., 2017; Martínez-Schuldt & Martínez, n.d.). Neither immigration nor limited cooperation policies are directly positively associated with higher rates of crime. Despite these consistent and reliable findings, many policymakers continue to question unauthorized migration and limited cooperation policies on the basis of public safety concerns.

This leads us to question the underlying nature of the public discourse surrounding limited cooperation policies, immigration, and crime. Overall, we contend that this discourse has less to do with crime and safety concerns and more with what Chavez (2008) describes as the "Latino Threat" narrative as well as public conceptualizations of what is means to be "real American." That is, the "rule of law" trope often deployed by opponents of limited cooperation policies serves as a color-blind, race-neutral guise for xenophobia and anti-immigrant sentiment, which are ultimately products of a larger racialized social system (Armenta, 2016; Armenta, 2017; Bonilla-Silva, 1997). This indirect and covert manifestation of racism becomes further entrenched and taken-for-granted precisely because certain segments of the population frame unauthorized migration as an assault on the "rule of law" or a "law and order" issue (Rumbaut & Ewing, 2007). We believe this helps explain why certain segments of the polity continue to recycle empirically inaccurate arguments that limited cooperation policies and noncitizens, more broadly, represent public safety concerns.

7 | CONCLUSION

In January of 2017, President Donald Trump signed an executive order entitled "Enhancing Public Safety in the Interior of the United States" (Executive Order 13768, 2017). The executive order targets so-called "sanctuary" jurisdictions by withholding federal grants, except for those funding law enforcement initiatives, until they fully cooperate with the federal government in the enforcement of immigration laws. Recent pieces of legislation proposed at the federal level have followed suite. Political pressure placed on the federal government to crackdown on limited cooperation policies is predicated upon the notion that localities with such ordinances threaten public safety by harboring "deportable" noncitizens. Nevertheless, this assertion is largely based on high-profile, isolated incidents of violent crime committed by deportable noncitizens rather than on empirical evidence. The current public and political discourse on "sanctuaries" inspired us to review the extant literature and pose three interrelated questions: 1) what is a "sanctuary" jurisdiction? 2) How and why have "sanctuary" policies evolved across time? And 3) what, if any, is the link between "sanctuary" policies and crime? In addressing these questions, our brief review of the existing academic literature on local "sanctuary" (i.e., limited cooperation) policies identifies several important findings.

First, although limited cooperation policies largely grew out of the 1980s Sanctuary Movement, these policies have evolved considerably as immigration enforcement has devolved from the federal to the local level. As such, contemporary "sanctuary" jurisdictions are broadly defined as places where state or local governments enact policies that limit local officials' cooperation with federal authorities in the enforcement of immigration laws. These policies broadly consist of "don't ask," "don't tell," and "don't enforce" measures (Kittrie, 2006).

Second, we find that relatively little empirical research examines the impact that local limited cooperation policies have on crime. This is concerning given the recent reliance on anecdotal evidence of violence committed by deportable noncitizens to call for the dismantling of limited cooperation policies through the withholding of federal funds. The studies we are aware of, conducted by Lyons et al. (2013), Gonzalez and colleagues (2017), Wong (2017), and Martínez-Schuldt and Martínez (n.d.) have yielded an inverse or null relationship between limited cooperation policies and crime. For the most part, it appears that jurisdictions with limited cooperation policies are either safer from crime or no different than jurisdictions without such policies (Wong, 2017; Gonzalez et al., 2017). Furthermore, limited cooperation policies may indirectly reduce crime by magnifying the crime-reducing effects of immigration (Martínez-Schuldt & Martínez, n.d.; Lyons et al., 2013; Ousey & Kubrin, 2017).

Our review of the literature suggests that existing evidence supports the expansion of limited cooperation policies. Criminological research has long established the importance of social organization and collective efficacy in reducing neighborhood crime (Sampson et al., 1997; Sampson & Graif, 2009). Ultimately, returning to the work of Lyons et al. (2013), it appears that limited cooperation policies expand immigrants' political opportunities and solidify community-official communication, which builds trust between communities and the officials that they represent and

serve. The result is that this "spiral of trust" strengthens formal and informal social control through community organization, thus reducing crime.

Finally, there exist several avenues for future research on so-called "sanctuaries" and crime. First, there is a clear need for additional longitudinal macro-level research that, 1) relies on more precise measures of crime (e.g., counts of specific types of crime), 2) examines intra-ethnoracial crime, 3) compares different units of analysis, 4) uses different approaches to operationalize "sanctuary" policies, and 5) considers the moderating effects of these policies and other key independent variables on crime. Second, there appears to be a notable dearth in individual-level research that examines the relationships between limited cooperation policies and their effects on, 1) criminal offending, including minor crime, 2) criminal victimization across types of jurisdictions, and 3) criminal victimization reporting. Continued research in these areas will significantly advance the current understanding of the limited cooperation policies-crime nexus and undoubtedly contribute to ongoing policy debates.

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ENDNOTES

[1] See *Miranda-Olivares v. Clackamas County* and *Galarza v. Szalcyk*.

[2] For example, see San Francisco's Due Process for All and Sanctuary 96-16 ordinance.

[3] Anti-Drug Abuse Act of 1988, Pub. L. No. 100-690, 102 Stat. 4181 (November 18, 1988).

[4] For a full list of city and county ordinances and administrative policies as well as state laws that limit compliance of local law enforcement with federal authorities, see Immigrant Legal Resource Center, "Detainer Policies," last updated on August 20, 2015. https://www.ilrc.org/detainer-policies

[5] See http://www.ailadownloads.org/advo/NILC-LocalLawsResolutionsAndPoliciesLimitingImmEnforcement.pdf.

[6] The constitutionality and legality of limited cooperation policies as well as immigration detainers are currently being debated in legal circles and in different circuit courts around the country, therefore it is imprecise to conclude that sanctuary jurisdictions across the United States willfully violate federal law.

ORCID

Daniel E. Martínez http://orcid.org/0000-0001-6773-4269

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Daniel E. Martínez is an Assistant Professor of Sociology at the University of Arizona. He is a co-principal investigator of the Migrant Border Crossing Study, which is a binational research project focusing on unauthorized migrants' border crossing, apprehension, and repatriation experiences. His research and teaching interests include criminology, juvenile delinquency, race and ethnicity, and unauthorized immigration. Martínez also does extensive research on migrant deaths along the US-Mexico border.

Ricardo Martínez-Schuldt is a doctoral candidate in the Department of Sociology at the University of North Carolina at Chapel Hill. His research focuses how sending-states, through consulate offices, facilitate the integration of migrants, as well as how local contexts impact the needs of immigrant populations and enable or constrain consular efficacy. In addition, he conducts research on neighborhood and city-level correlates of crime, with an emphasis on immigration-crime nexus.

Guillermo Cantor is the Deputy Director of Research at the American Immigration Council, where he leads the Council's research efforts and manages the research team. He has authored numerous publications on immigration policy and immigrant integration and regularly appears in English and Spanish-language media. He also currently teaches sociology of migration at Georgetown University. Cantor holds a PhD in Sociology from the University of Maryland, College Park.

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American Immigration Council

FEBRUARY 2025

FACT SHEET

Sanctuary Policies: An Overview

Chicago, IL - July 13, 2019. Photo by Antwon McMullen.

What are so-called "sanctuary" policies?

Over the past decade, an increasing number of states, counties, and cities have adopted a variety of policies intended to promote a greater level of trust and cooperation between local law enforcement and communities with sizeable immigrant populations, regardless of immigration status. These policies include offering English-language classes; issuing municipal identification documents and driver's licenses to all residents; ensuring that immigrants have equal access to bail; establishing policies to make it easier for noncitizen victims of crime to obtain necessary documents from law enforcement agencies in order to pursue certain immigration relief; and training criminal prosecutors and public defenders on the immigration consequences of convictions and plea deals.

One subset of these policies concerns a state's or locality's role in cooperating with federal authorities to enforce immigration law. These laws, policies, or resolutions are sometimes referred to as "sanctuary" policies, although no legal or standard definition of the term exists. There are many reasons jurisdictions adopt sanctuary policies, such as: a desire to protect public safety by allowing immigrants to work with police in reporting and investigating crimes without fear of retribution or potential deportation; allowing state and local governments to determine how they will prioritize and allocate their resources; and shielding local law enforcement agencies from liabilities resulting from local enforcement of federal immigration laws.

Sanctuary policies help immigrant communities with:

- English-language classes
- IDs and driver's licenses
- Access to bail
- Documents to pursue immigration relief

There is no universal definition of a sanctuary policy

Despite the nationwide debate, there is no one clear definition of what it means for a state or local government to adopt sanctuary policies. Sanctuary policies take many forms and generally fall into the following categories:[1]

- policies restricting the ability of state and local police to make arrests for federal civil immigration violations, or to detain individuals on civil immigration warrants;
- policies prohibiting "287(g)" agreements through which Immigration and Customs Enforcement (ICE) deputizes local law enforcement officers to enforce federal immigration law;
- policies that prevent local governments from entering into a contract with the federal government to hold immigrants in detention;
- policies preventing immigration detention centers;
- policies restricting the police or other city workers from asking about immigration status;
- policies restricting the sharing of certain information on immigrants with the federal government;
- policies restricting local police responses to federal immigration detainers; and
- policies refusing to allow ICE into local jails without a judicial warrant.

The common theme behind these categories is that under a sanctuary policy, state and local officials will limit their cooperation with federal immigration officials but do not actively prevent federal officials from carrying out their immigration enforcement duties.

One of the most common forms of sanctuary policy is a restriction on holding immigrants in state or local jails following a "detainer" issued by ICE. A detainer is an official but nonbinding request from ICE that a state or local law enforcement agency maintain custody of an individual for up to 48 hours beyond the time the individual otherwise would have been released so that ICE can arrange to take over custody.[2]

Sanctuary policies are based on the idea that the federal government cannot compel jurisdictions to take part in immigration enforcement

Federal actions intended to force local jurisdictions to perform immigration enforcement are likely unconstitutional. Under the Tenth Amendment of the U.S. Constitution, the federal government "may neither issue directives requiring the States to address particular problems, nor command the States' officers, or those of their political subdivisions, to administer or enforce a federal regulatory program."[3] Courts have repeatedly held that the Tenth Amendment prohibits the federal government from compelling states and localities to participate in immigration enforcement.[4] The Supreme Court has clarified that immigration enforcement is the sole duty of the federal government, and state and local police may only carry out immigration enforcement if specifically authorized to do so by the federal government.[5]

Courts have repeatedly held that the Tenth Amendment prohibits the federal government from compelling states and localities to participate in immigration enforcement.

Compliance with immigration detainers is voluntary, not mandatory. The federal government cannot force local jurisdictions to honor detainers, an interpretation repeatedly upheld by the courts.[6] Some state courts have also ruled that the laws of their state do not provide legal authority for law enforcement agencies to hold people on an immigration detainer.[7] In fact, jurisdictions that do honor detainers can be found liable for unlawfully holding an individual on a detainer without a judicial warrant in violation of the Fourth Amendment, and may be required to compensate individuals for damages.[8]

Sanctuary jurisdictions do not shield immigrants from deportation, and may still share information or otherwise cooperate with federal immigration authorities

Contrary to what many believe, sanctuary policies do not conceal or shelter undocumented immigrants from detection.[9] Nor do they shield immigrants from deportation or prosecution for criminal activities. State and local police still enforce state and local criminal laws against immigrants who are accused of committing a crime in sanctuary jurisdictions. Importantly, the Supreme Court has made clear that "as a general rule, it is not a crime for a[n undocumented immigrant] to remain present in the United States."[10]

Research published in 2020 by the National Academy of Sciences found that sanctuary policies did not prevent the "deportations of people with violent convictions." The research—which analyzed FBI crime data and ICE deportation data—found that the implementation of sanctuary policies between 2010 and 2015 did not affect crime rates in jurisdictions with such policies or result in fewer people with violent convictions being deported.[11]

Austin, Texas, USA - May 29, 2017: A mother and her daughter protest SB 4, an anti-Sanctuary Cities immigration law, outside the Capitol. Photo by Vic Hinterlang.

Jurisdictions that adopt at least one sanctuary policy may still cooperate with federal immigration officials in a variety of other ways. For example:

- Even in sanctuary jurisdictions, officials still send the fingerprints of any person—including an immigrant— booked into a prison or jail to the federal government; the federal government then uses that information to identify noncitizens for potential deportation.[12]
- Sanctuary jurisdictions may rent jail space to the federal government to house immigrant detainees through Inter-Governmental Service Agreements (IGSAs),[13] although some states have tried to prohibit or limit immigration detention in their state.[14]
- Sanctuary jurisdictions may even have policies that direct local law enforcement to, under limited circumstances, either honor requests from ICE to be notified of an individual's release from local custody or comply with immigration detainers. A sanctuary jurisdiction may have, for example, a policy directing local law enforcement to only honor detainers if the individual in question has been convicted of a serious or violent crime. [15]

Sanctuary jurisdictions are in compliance with federal law

While the Department of Justice (DOJ) has recently filed litigation and taken other steps to challenge sanctuary jurisdictions,[16] neither the Constitution nor any federal statute prohibits policies that limit cooperation with federal immigration enforcement.[17]

8 U.S.C. § 1373 is a federal civil statute that prohibits state and local governments from enacting laws or policies that limit communication about "information regarding the immigration or citizenship status" of individuals with the Department of Homeland Security (DHS).[18] The statute also prohibits restrictions on maintaining such information. But a number of courts have found that Section 1373 unlawfully interferes with state and local authority.[19]

In 2017 and 2018, the DOJ sent letters to a number of cities and counties with sanctuary laws, threatening to withhold criminal justice grants unless the jurisdictions confirmed compliance with Section 1373.[20] Numerous cities and counties sued, with all but one court finding that the grants did not authorize imposing such conditions.[21]

Even if lawful, Section 1373 **does not**

- prohibit laws or policies that limit communication regarding criminal case information, custody status, or release dates of individuals in custody;
- mandate that jurisdictions comply with immigration detainers;
- prohibit policies or laws that restrict compliance with detainers; or
- require state or local law enforcement to collect information on immigration or citizenship status—
- nor does it prevent jurisdictions from limiting the collection of such information.[22]

8 U.S.C. § 1324 is a federal criminal statute that makes it a crime to "conceal, harbor, or shield from detection" an undocumented person, and to "encourage or induce" someone to come to the United States in violation of law.[23] While courts have debated the scope of the statute, it has never been applied to a state or local official for declining to cooperate with federal immigration enforcement.[24]

Jurisdictions that do not honor ICE detainers or adopt sanctuary policies are safer and better off economically than those that do

A 2020 study found that, while sanctuary policies "changed the composition of deportations, reducing deportations of people with no criminal convictions by half," such policies did not affect "deportations of people with violent convictions." Moreover, "sanctuary policies also had no detectable effect on crime rates." The study concluded that "sanctuary policies, although effective at reducing deportations, do not threaten public safety."[25]

A 2017 report found a correlation with lower crime rates and higher economic indicators in counties that do not honor ICE detainers when compared to counties that do.[26] The analysis revealed that in the non-detainer counties:

- **Crime is lower.** There are, on average, 35.5 fewer crimes committed per 10,000 people in the non-detainer counties compared to counties that do honor ICE detainers.[27] Crime is defined in the report as the total number of violent crimes (murders, rapes, robberies, and assaults) and property crimes (burglaries, larceny, motor vehicle thefts, and arsons) per 10,000 people. Likewise, a 2016 study found no association between crime rates and policing practices.[28] Multiple studies published since 2017 have confirmed these findings or determined that sanctuary policies have no measurable effect on crime rates.[29] One study in 2020 even found that sanctuary policies reduce domestic violence against Hispanic women.[30] No studies have demonstrated a link between sanctuary policies and increased crime.

- **Median household income is higher.** On average, median household income is $4,353 higher in the non-detainer counties than in counties that honor detainers. This outcome is not driven by income gains among Latinos at the expense of white residents or African Americans. In fact, among white residents, median household income is $2,836 higher in the non-detainer counties.[31]
- **Poverty is lower.** The poverty rate is 2.3 percent lower in non-detainer counties. The rate of poverty among white residents is 1.4 percent lower in non-detainer counties.[32]
- **Reliance on public assistance is lower.** The percentage of households receiving Supplemental Nutrition Assistance Program (SNAP) benefits (formerly known as food stamps) is 2.6 percent lower in non-detainer counties. The percentage of households that receive Supplemental Security Income (SSI) is 0.9 percent lower. The share of children under 18 who receive public assistance is 4.9 percent lower in non-detainer counties.[33]
- **Labor-force participation is higher.** On average, the labor-force participation rate (the proportion of the population 16 years and older that is working or actively looking for a job) is 2.5 percent higher in non-detainer counties. Labor-force participation among white residents is also 2.5 percent higher in non-detainer counties.[34]
- **Employment-to-population ratio is higher.** The employment-to-population ratio is the number of people 16 years and older who are employed, divided by the total number of people 16 years and older. The employment-to-population ratio is 3.1 percent higher in non-detainer counties. The white employment-to-population ratio is 3.2 percent higher in non-detainer counties.[35]
- **Unemployment is lower.** The unemployment rate is 1.1 percent lower in non-detainer counties. The white unemployment rate is 0.8 percent lower.[36] Similarly, a 2016 study found no association between unemployment rates and policing practices.[37]

ENDNOTES

1 See Lena Graber and Krsna Avila, *Growing the Resistance: How Sanctuary Laws and Policies Have Flourished During the Trump Administration* (San Francisco, CA: Immigrant Legal Resource Center, December 2019), 5, https://www.ilrc.org/sites/default/files/resources/2019.12_sanctuary_report-final-12.17.pdf.

2 U.S. Department of Homeland Security, Immigration Detainer - Notice of Action (Form I-247A), updated March 2017, https://www.ice.gov/sites/default/files/documents/Document/2017/I-247A.pdf.

3 See Kate M. Manuel, *Immigration Detainers: Legal Issues*, R42690 (Washington, DC: Congressional Research Service, May 7, 2015), https://fas.org/sgp/crs/homesec/R42690.pdf; *Printz v. United States*, 521 U.S. 898, 926 (1997) (quoting *New York v. United States*, 505 U.S. 144, 188 (1992)) ("The federal government . . . may not compel the States to enact or administer a federal regulatory program.").

4 *United States v. California*, 921 F.3d 865, 891 (9th Cir. 2019) ("California has the right, pursuant to the anticommandeering rule, to refrain from assisting with federal efforts."); *City of El Cenizo v. Texas*, 890 F.3d 164, 178 (5th Cir. 2018) ("[T]he Tenth Amendment prevents Congress from compelling Texas municipalities to cooperate in immigration enforcement."); *Galarza v. Szalczyk*, 745 F.3d 634, 643-45 (3d Cir. 2014) ("[I]mmigration officials may not compel state and local agencies to expend funds and resources to effectuate a federal regulatory scheme.").

5 See *Arizona v. United States*, 567 U.S. 387 (2012)

6 See, e.g., *Galarza*, 745 F.3d at 643-45; *N.S. v. Hughes*, 335 F.R.D. 337, 346 (D.D.C. 2020), *reconsideration denied*, No. 1:20-CV-101-RCL, 2020 WL 4260739 (D.D.C. July 24, 2020); *Esparza v. Nobles Cty.*, No. A18-2011, 2019 WL 4594512, at *9 (Minn. Ct. App. Sept. 23, 2019); *Creedle v. Miami-Dade County*, 349 F. Supp. 3d 1276, 1304 (S.D. Fla. 2018).

7 See, e.g., *People ex rel. Wells v. DeMarco*, 168 A.D.3d 31, 53, 88 N.Y.S.3d 518, 536 (2018); *Lunn v. Commonwealth*, 78 N.E.3d 1143 (Mass. 2017).

8 Luis Ferré-Sadurní, "New York City to Pay $92.5 Million to Improperly Detained Immigrants," *New York Times* (Dec. 18, 2024), https://www.nytimes.com/2024/12/18/nyregion/migrants-detention-settlement-deportation.html?unlocked_article_code=1.vE4.aKem.drUpy7RB3vOD&smid=url-share; National Immigrant Justice Center, "L.A. County Settles Immigrant Detention Suit for $14 Million," (Oct. 13, 2020), https://immigrantjustice.org/press-releases/la-county-settles-immigrant-detention-suit-14-million *Miranda-Olivares v. Clackamas Cnty.*, No. 12-02317, 2014 2014 WL 1414305, at *11 (D. Ore. Apr. 11, 2014); *DelAgua v. Jones*, No. 15-0185 (Eastern District of California, settled 2015); *Valdez-Sandoval v. Walcher* (District of Colorado); *Quezada v. Mink*, No. 10-0879 (District of Colorado, settled 2011); *Harvey v. City of New York*, No. 07-0343 (Eastern District of New York, settled 2009).

9 Erwin Chemerinsky, Annie Lai, and Seth Davis, "Trump can't force 'sanctuary cities' to enforce his deportation plans," *The Washington Post*, December 22, 2016, https://www.washingtonpost.com/opinions/trump-cant-force-sanctuary-cities-to-enforce-his-deportation-plans/2016/12/22/421174d4-c7a4-11e6-85b5-76616a33048d_story.html?utm_term=.9293174c5d44.

10 *Arizona*, 567 U.S. at 407.

11 David K. Hausman, "Sanctuary policies reduce deportations without increasing crime," *Proceedings of the National Academy of Sciences 117*, no. 44 (November 3, 2020): 27,262-27,267, https://www.pnas.org/content/117/44/27262.

12 See ICE Priority Enforcement Program webpage (archived), June 22, 2017, http://www.ice.gov/pep.

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CRIMINOLOGY

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DOES UNDOCUMENTED IMMIGRATION INCREASE VIOLENT CRIME?*

MICHAEL T. LIGHT[1] and TY MILLER[2]
[1]Department of Sociology, University of Wisconsin—Madison
[2]Department of Sociology, Purdue University

KEYWORDS: undocumented immigration, violent crime, immigration enforcement

Despite substantial public, political, and scholarly attention to the issue of immigration and crime, we know little about the criminological consequences of undocumented immigration. As a result, fundamental questions about whether undocumented immigration increases violent crime remain unanswered. In an attempt to address this gap, we combine newly developed estimates of the unauthorized population with multiple data sources to capture the criminal, socioeconomic, and demographic context of all 50 states and Washington, DC, from 1990 to 2014 to provide the first longitudinal analysis of the macro-level relationship between undocumented immigration and violence. The results from fixed-effects regression models reveal that undocumented immigration does not increase violence. Rather, the relationship between undocumented immigration and violent crime is generally negative, although not significant in all specifications. Using supplemental models of victimization data and instrumental variable methods, we find little evidence that these results are due to decreased reporting or selective migration to avoid crime. We consider the theoretical and policy implications of these findings against the backdrop of the dramatic increase in immigration enforcement in recent decades.

Few topics have more criminological significance and public policy salience than understanding the impact of undocumented immigration on violent crime. Although the immigration–crime nexus has been at the fore of criminological inquiry since the Chicago School of the early 20th century (Shaw and McKay, 1942), this issue has taken on added importance over the past two decades as the United States has experienced the largest wave of immigration—in both absolute and relative terms—in its history. Nevertheless, despite substantial research attention to the association between immigration and crime (Martinez and Valenzuela, 2006), significant gaps remain in the literature. Most notably,

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Direct correspondence to Michael Light, Department of Sociology, University of Wisconsin—Madison, 8128 William H. Sewell Social Sciences Building, 1180 Observatory Drive, Madison, WI 53703 (e-mail: mlight@ssc.wisc.edu).

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this body of work has mainly been confined to assessments of the overall or Latino foreign-born populations (Feldmeyer, 2009; Martinez, Stowell, and Lee, 2010; Ousey and Kubrin, 2009; Wadsworth, 2010) because of the paucity of data accounting for unauthorized immigrants separately.[1] As Ousey and Kubrin (2017: 1.9) highlighted, "the problem with these approaches is that they treat immigrants as a homogenous population and fail to account for significant variation across types of immigrants." As a result, research on undocumented immigration remains a substantial lacuna in immigration–crime research.

Indeed, in a recent meta-analysis of the 51 macro-level immigration–crime studies conducted between 1994 and 2014, not one was aimed at explicitly examining unauthorized immigration flows (Ousey and Kubrin, 2017). Since that time, we are aware of only one study in which the association between unauthorized immigration and violence was investigated. In that study, Green (2016) found that undocumented immigration is generally not associated with violent crime, though unauthorized immigration from Mexico may be associated with higher rates of violence. Although informative, several limitations of this study warrant further inquiry. Most notably, the analysis is cross-sectional, thus, limiting both the substantive questions under consideration and the analytical leverage to answer them. Substantively, cross-sectional analysis cannot answer the focal question motivating criminological debates on unauthorized immigration: Has the increase in undocumented immigration increased violent crime? Because unauthorized immigration is necessarily a process that unfolds over time, cross-sectional analyses are ill-suited for use in answering this question. Moreover, the methodological distinction between cross-sectional and longitudinal analysis in immigration–crime research is a salient one. As Ousey and Kubrin (2017: 1.13) noted in their meta-analysis, "our findings underscore the fact that the choice between cross-sectional and longitudinal data and analysis procedures is a critical one that likely impacts findings and conclusions in this area." They concluded that because longitudinal research provides greater analytical rigor, such as superior ability to control for confounding influences, more weight should be given to the findings from longitudinal studies. To date, however, the literature currently lacks a longitudinal assessment of the consequences of undocumented immigration for violent crime (but see Light, Miller, and Kelly, 2017, for an examination of drug and alcohol crimes).

We seek to fill this gap by providing the first longitudinal empirical analysis of the macro-level relationship between undocumented immigration and violent crime. In combining newly developed estimates of the unauthorized population with multiple data sources to capture the criminal, socioeconomic, and demographic context of all 50 states and Washington, DC, from 1990 to 2014, we use fixed-effects regression models to examine the effect of increased unauthorized immigration on violent crime rates.

This analysis is timely given the growth of the undocumented population in recent decades. Between 1990 and 2014, the number of undocumented immigrants more than tripled, from 3.5 million to 11.3 million (Krogstad, Passel, and Cohn, 2016), accounting for more than a third of the increase in the total foreign-born population over this period. This wave of immigration generated substantial public angst regarding the criminality of unauthorized immigrants, leading to immigration reforms and public policies intended to reduce the purported crimes associated with undocumented immigration (Bohn,

1. To avoid redundancy, we use the terms "undocumented" and "unauthorized" interchangeably throughout this article.

Lofstrom, and Raphael, 2014). Indeed, the presumptive link between unauthorized immigration and violent crime has become a core assertion in the anti-immigration narrative in public, political, and media discourse (Chavez, 2008) and has been at the center of some of the most contentious immigration-reform policies in recent years (e.g., Arizona SB 1070, 2010).

Moreover, concerns over illegal immigration have arguably been the federal government's highest criminal law enforcement priority in recent decades. Between 1986 and 2008, the number of U.S. Border Patrol officers increased 5-fold while the budget for border enforcement increased 20-fold (Massey, Pren, and Durand, 2016). As a result, today the U.S. government spends more on immigration enforcement agencies (U.S. Customs and Border Protection and U.S. Immigration and Customs Enforcement) than it does on all other principal criminal law enforcement agencies combined, including the FBI, DEA, Secret Service, Marshal's Service, and ATF (Meissner et al., 2013). Reductions in crime and violence have been primary justifications for this dramatic development (Department of Homeland Security Immigration and Customs Enforcement [DHS ICE], 2009). Yet, this vast apparatus of criminal justice machinery has been built up to increase public safety with scant empirical evidence that undocumented immigration and violence are linked (positively or negatively).

The results of our analysis not only inform this contentious policy debate, but they also provide us with an opportunity to adjudicate competing theoretical perspectives on the immigration–crime link. Although the weight of the evidence supports the immigrant revitalization perspective, whereby immigration is said to reduce crime and violence by attracting immigrants with low criminal propsensities, strengthening local economies, and bolstering processes of informal social control[2] (Lyons, Vélez, and Santoro, 2013), others argue that social disorganization better captures the contemporary immigration–crime relationship (Shihadeh and Barranco, 2013). This perspective may be especially relevant for the unauthorized population who, unlike their documented counterparts, are hindered from effectively forming economic and social ties as a result of their lack of legal standing in the community. It is important to note, however, that almost none (save Green, 2016) of the immigration–violence research to date has systematically examined the undocumented immigrant population, despite the fact that the patterns of authorized and unauthorized immigration in recent decades have not been uniform. For example, of the 10 states that experienced the largest percent increase in undocumented immigrants between 1990 and 2000, only two of them were also in the top 10 for relative increases in lawful immigrants.[3]

The remainder of this article is organized as follows. The following sections briefly explicate the contrasting theoretical perspectives linking immigration and violence. Given that these perspectives have been given ample treatment elsewhere (Ousey and Kubrin, 2009, 2017), we focus our discussion on the applications of theory to unauthorized

2. In the past, researchers have used terms such as the "immigrant concentration view" (Desmond and Kubrin, 2009), the "ethnic community model" (Logan, Zhang, and Alba, 2002), the "enclave hypothesis" (Portes and Jensen, 1992), the "community resource perspective" (Feldmeyer, 2009), and the "Latino paradox" (Sampson, 2008) to describe these effects. Regardless of the term, for our purposes, what matters is they make the same directional hypothesis: Immigrants provide prosocial benefits in ways that reduce the prevalence of violent crime.

3. Authors' calculations of data from the Center for Migration Studies.

immigration specifically. We then turn to the data, research design, and results. Last, we discuss the implications of the findings in the context of contemporary debates on the criminogenic consequences of unauthorized immigration.

MARGINALIZATION, DISORGANIZATION, AND VIOLENCE

Between 2005 and 2010, state legislatures enacted more than 300 anti-immigration laws (Gulasekaram and Ramakrishnan, 2012), including regulations that deny public benefits, services, and health care to unauthorized immigrants, as well as laws that punish employers who hire undocumented workers and landlords who rent to unauthorized immigrants (Varsanyi, 2010). For undocumented immigrants, these laws compound the fact that they are denied almost all forms of federal assistance and, by definition, have no political representation. According to Menjívar and Abrego (2012), the cumulatively injurious effects of immigration laws on the daily lives of unauthorized immigrants represent a form of "legal violence" or what Kubrin, Zatz, and Ramirez (2012) called "state-created vulnerabilities." Regardless of the term, the lack of legal standing may have several important implications for criminal behavior.

First, as a result of their formal exclusion from the labor market, undocumented immigrants may experience intense economic deprivation (Hall, Greenman, and Farkas, 2010; Massey, 2007). In 2007, the poverty rate among unauthorized adults was double that of U.S.-born adults (Passel and Cohn, 2009) and recent research findings show that undocumented immigrants face a "double disadvantage" in the U.S. labor force: first by being disproportionately driven into the secondary labor market and second by paying a wage penalty within these occupations (Durand, Massey, and Pren, 2016). Moreover, because of their marginal labor market skills and lack of economic assets upon arrival (Jargowsky, 2009), many undocumented immigrants may be channeled into structurally disadvantaged areas (Hagan and Palloni, 1999) and, therefore, differentially exposed to criminogenic conditions such as entrenched poverty. As a result, undocumented immigrants may have limited legitimate opportunities for upward mobility and thus may turn to illegitimate economic pursuits, such as robbery (Baker, 2015; Ousey and Kubrin, 2009). In this vein, illicit drug markets are of particular concern when considering the criminogenic consequences of undocumented immigration. Although recent empirical evidence suggests that trends in undocumented immigration are not associated with increased drug crime (Light, Miller, and Kelly, 2017), undocumented immigrants are commonly perceived to be participants in illegal drug markets. Furthermore, trends in illegal drug activity are associated with changes in crime rates (Baumer et al., 1998; Ousey and Lee, 2002). To the extent that drugs offer opportunities denied in the legitimate labor market, undocumented immigrants may be more heavily involved in illicit drug markets, which in turn increases crimes like robbery, murder, and overall violence.

Because unauthorized immigrants face exclusionary governmental policies, a second factor potentially contributing to increased crime concerns the inability of undocumented immigrants to form effective ties with government officials. That is, because undocumented immigrants are forced to live "shadowed lives" (Chavez, 2013) for fear of detection, unauthorized communities may feel socially isolated and cynical of the legal and political system (Kirk et al., 2012). As a result, rather than involving government authorities, undocumented immigrants may turn to violence as a form of dispute resolution (Black, 1983), though this may not be reflected in official crime statistics due to

underreporting within undocumented communities, a point to which we return in the analysis. In addition, the inability to engage fully in civic and political life may undermine undocumented immigrants' ability to organize collectively around common goals, such as crime reduction (Lyons, Vélez, and Santoro, 2013). In this regard, this perspective adjoins classic arguments on the social disorganizing effects of rapid immigration (Shaw and McKay, 1942), by which immigration was thought to increase crime by undermining social networks and institutions necessary for regulating behavior. Although contemporary researchers question this thesis for immigrants generally (Lee and Martinez, 2009), social disorganization theory may be more applicable for undocumented immigrants given their constant status as "internal exiles" (Simon, 2007) with limited access to mainstream political and economic institutions.

The discussion on unauthorized immigration and crime tends to focus on the behavior of the undocumented, however, another important consideration is the effect of immigration on U.S. workers. Much of the controversy surrounding undocumented immigration concerns economic threats to the U.S. labor force (Chavez, 2008), and the results of recent research explicitly link contemporary immigration to increased violence due to competition for low-skilled jobs (Shihadeh and Barranco, 2010). These effects may be even more pronounced for unauthorized immigration (compared with overall immigration), given that the undocumented are almost entirely relegated to the low-skilled labor market (Passel and Cohn, 2009). Hence, even if undocumented immigrants are not more crime prone, unauthorized immigration may increase violence by economically disadvantaging low-skilled U.S. workers.

Lastly, population age structure may connect undocumented immigration and violence. The age distribution of the unauthorized immigrant population is remarkably different from that of the U.S.-born and lawful immigrant population, with unauthorized immigrants far more likely to be young adults (18–24 years of age; Passel and Cohn, 2009)—a life-course period in which violence peaks. Consequently, unauthorized immigrants may increase crime rates by shifting the age composition toward a more "violence-prone" age profile.

Taken together, there are multiple theoretical reasons to expect undocumented immigration to increase violent crime. Skeptics, however, often point out that during the same period the United States experienced substantial growth in the unauthorized population, it simultaneously witnessed the most dramatic reductions in criminal violence in half a century (Sampson, 2008). Thus, rather than viewing immigration (both documented and undocumented) as criminogenic, these contrasting trends have motivated a competing body of research on the potential benefits of immigration.

SELECTION, NETWORKS, AND IMMIGRANT REVITALIZATION

In direct contrast to the marginalization and disorganization perspectives, researchers have recently provided considerable theoretical reasoning to anticipate that undocumented immigration would decrease crime. The first concerns the selective nature of immigration. Many immigrants are driven by the pursuit of economic and educational opportunities for themselves and their families (Chavez, 2013), and clandestine migration requires a substantial amount of motivation and planning. As such, undocumented immigrants may be selected on attributes that predispose them to low criminal propensity, such as high motivation to work and ambition to achieve (Butcher and Piehl, 2007;

Stowell et al., 2009). Related to this point, unauthorized immigrants, much more so than lawful immigrants, have strong incentives to avoid criminal involvement for fear of detection and deportation. In both scenarios, increases in undocumented immigration should decrease violent crime over time.

The economic benefits associated with undocumented immigration are other factors possibly linking the unauthorized population to lower violence. Despite the oft-repeated claims that immigration harms the U.S. economy, researchers have suggested that immigration (including undocumented immigration) is a net economic positive by increasing tax revenue (Gardner, Johnson, and Wiehe, 2015), infusing new business and social capital (Light and Gold, 2000), and filling employment niches that complement native-born labor sectors (Hinojosa-Ojeda, 2010). Indeed, despite their legal exclusion from the labor market, a full 93 percent of working-age unauthorized immigrant men were in the labor force in 2009 (Donato and Armenta, 2011).

In addition, existing ethnic enclaves can provide important economic resources to aid the integration of the undocumented (Portes and Jensen, 1992) as well as social networks capable of bolstering processes of informal social control (Feldmeyer, 2009). This process is often referred to as the immigrant revitalization thesis (Lee and Martinez, 2002), whereby the influx of immigrants is said to strengthen organizations and institutions (e.g., schools, churches, and social clubs). These organizations, in turn, help shelter immigrants from economic deprivation and other social problems by reinforcing social cohesion and bonding children to mainstream institutions (Ley, 2008; Theodore and Martin, 2007).

Moreover, the common practice of "chain migration" characteristic of undocumented immigration (Massey, 1990) further buttresses immigrant social capital networks that provide key resources and social support systems for unauthorized newcomers, such as transportation assistance, childcare, and housing (Portes and Rumbaut, 2006). In short, the immigrant networks that sustain the unauthorized migration process also help facilitate the economic and social integration of undocumented immigrants, thus, minimizing the effects of economic disadvantage, providing an umbrella of social control, and potentially reducing the frequency of violence.

Undocumented immigration may also help decrease criminal violence through a process of cultural diffusion. Cultural adaptations to concentrated disadvantage—such as Anderson's (1999) *Code of the Streets*—have been identified as major contributing factors to higher rates of violent crime (Sampson and Wilson, 1995). According to Sampson (2012), introducing outsiders who do not share the cultural expectations or social meaning of the "code of the street" can have a suppressing effect on violence. Thus, to the extent that immigrant cultures are less encouraging of violence, both lawful and undocumented immigration may contribute to lower rates of violent crime by leading to "greater visibility of competing nonviolent mores" that affect not just immigrants "but diffuse through social interactions to depress violent conflict in general" (Sampson, 2012: 257–8).

Finally, the number of police officers may play a role in the relationship between crime and undocumented immigration. As indicated, increases in undocumented immigration are associated, at least in part, with public fear of criminal behavior. States may react to these fears by hiring more police officers in an effort to assuage concerns and deter criminal behavior. By increasing formal social control, larger police force sizes have been shown to decrease crime (Levitt, 2004). Thus, undocumented immigration may be correlated with decreases in crime through increased formal social control (e.g., police force size).

DATA, METHOD, AND LOGIC OF ANALYSIS

We examine the competing theoretical perspectives outlined earlier by using multiple data sources collected annually at the state level (including Washington, DC) between 1990 and 2014. Our crime measures come from the FBI Uniform Crime Reports (UCR) program, which counts all serious offenses reported to the police. Our measures of the undocumented population come from two different sources: The Center for Migration Studies and the Pew Research Center (described in detail later). In addition, data on an array of socioeconomic, demographic, and criminogenic characteristics were collectively derived from the U.S. Census, the Bureau of Labor Statistics, the National Prisoner Statistics, and the Centers for Disease Control and Prevention (CDC). These interrelated sources of data provide a comprehensive longitudinal resource for examining the undocumented immigration–violence relationship. Sources, measurement properties, and descriptive statistics for all variables used in the analysis are shown in table 1.

After accounting for missing data, the final sample consists of 1,209 state-years (50 states plus Washington, DC, over 24 years of data).[4] This collection of data offers several advantages for this study. First, UCR data are the most commonly used criminal justice statistics (Mosher, Miethe, and Phillips, 2002) with evidence of reliable reporting of serious felonies over time that accurately track victimization trends (Blumstein, Cohen, and Rosenfeld, 1991; Lauritsen, Rezey, and Heimer, 2016). Second, the data coverage is available for the full study period (1990–2014), providing an opportunity to examine the longitudinal criminological consequences of the dramatic growth in unauthorized immigration in recent decades. Third, the use of state-level analyses has considerable precedent in both immigration and criminological research, including studies on the etiology of crime (Duggan, 2001), the immigration process generally (Massey and Capoferro, 2008), and unauthorized immigration specifically (Durand, Massey, and Capoferro, 2005). Moreover, research findings show that many of the operative mechanisms linking macro-structural conditions to crime are invariant across cities, metropolitan areas, and states, including those that directly tap relevant theoretical constructs and measures of the social disorganization framework, such as structural disadvantage (e.g., poverty), family structure, and population composition (Land, McCall, and Cohen, 1990). Thus, the utilization of state-level data is useful for informing the contrasting theoretical perspectives motivating this study. Finally, state-level analysis is apt because it decreases the amount of sampling variability in the unauthorized estimates (compared with smaller units of analysis) and captures the entire at-risk population for the outcome of interest (crime rates) as well as the total undocumented population. As such, this analysis provides an opportunity for us to examine the undocumented immigration–violence link across the entire United States and to make generalizable conclusions that are not limited to select jurisdictions (e.g., Chicago). This point is especially significant for although

4. Incarceration information is missing for Washington, DC, after 2001, after the city abandoned its prison system, and for one year in Nevada (13 total state-years). Also, data are missing on gun availability for one year in DC as a result of data suppression protocols from the CDC. Lastly, police officer information is missing for West Virginia for 2014. The results are entirely unchanged when we drop these measures from the analysis and include these years of data (results available on request).

Table 1. Descriptive Statistics for Dependent and Explanatory Variables, 1990–2014

Measures	Coding and Description	*Between States* Mean	*Within States* Mean	Source
Dependent Variables				
(ln) Violent Crime Index	Violent crimes known to the police (per 100,000)—logged	5.97 (.52)	.00 (.21)	FBI Uniform Crime Reports
(ln) Homicide rate	Homicides (per 100,000 in the population)—logged	1.51 (.61)	.00 (.28)	FBI Uniform Crime Reports
(ln) Robbery rate	Robberies (per 100,000 in the population)—logged	4.51 (.85)	.00 (.24)	FBI Uniform Crime Reports
(ln) Aggravated assault rate	Aggravated Assaults (per 100,000 in the population)—logged	5.51 (.55)	.00 (.24)	FBI Uniform Crime Reports
(ln) Rape rate	Rapes (per 100,000 in the population)—logged	3.50 (.29)	.00 (.18)	FBI Uniform Crime Reports
Focal Measures				
Undocumented immigrants—CMS	Estimated proportion of population that is undocumented	1.93 (1.57)	.00 (.76)	Center for Migration Studies
Undocumented immigrants—Pew	Estimated proportion of population that is undocumented	2.13 (1.52)	.00 (.87)	Pew Hispanic Center
Covariates				
Lawful immigrants	Proportion of population that are lawful foreign-born residents	6.45 (4.73)	.00 (.92)	IPUMS
Poverty[a]	Proportion of population that is in poverty	13.19 (3.21)	.00 (1.87)	U.S. Census Bureau / American Community Survey
Low educational attainment[a]	Proportion of the population over 25 without a high school degree	15.35 (3.91)	.00 (3.39)	U.S. Census Bureau / American Community Survey
Single parent children[a]	Percent of children born to single mothers	34.54 (6.43)	.00 (4.57)	U.S. Census Bureau / American Community Survey
Racial composition[a]	Proportion of the population that is non-Hispanic black	10.65 (10.58)	.00 (.65)	U.S. Census Bureau / American Community Survey
Unemployment	Proportion of civilian population over 16 that is unemployed	5.74 (.94)	.00 (1.62)	U.S. Bureau of Labor Statistics

(*Continued*)

Table 1. Continued

Measures	Coding and Description	*Between States* Mean	*Within States* Mean	Source
Manufacturing	Percent employed in the manufacturing industry	12.57 (4.67)	.00 (2.60)	U.S. Bureau of Labor Statistics
Managerial / professional	Percent employed in professional or managerial occupations	31.89 (3.38)	.00 (4.55)	U.S. Bureau of Labor Statistics
Crime prone ages (18–24)	Percent of population age 18–24	9.93 (.67)	.00 (.56)	U.S. Census Bureau / American Community Survey
Urban population	Percent of the population living in urban areas	71.97 (14.63)	.00 (2.30)	U.S. Census Bureau
Incarceration rate	Number of people incarcerated (per 100,000 in the population)	385.69 (166.49)	.00 (90.15)	BJS National Prisoner Statistics
Police per capita	Number of police officers (per 100,000 in the population)	215.63 (63.41)	.00 (19.20)	UCR Police Employee Data
Gun availability	Proportion of suicides perpetrated with a gun	55.48 (12.16)	.00 (5.01)	CDC WONDER Underlying Cause-of-Death Mortality Files
Drug activity	Drug overdose mortality rate (per 100,000)	9.07 (2.67)	.00 (4.91)	CDC WONDER Underlying Cause-of-Death Mortality Files

NOTE: Number of observations = 1259. Std. deviations reported in parentheses.
[a]Combined using principal component analysis to create index of Structural Disadvantage.

much of the previous immigration–crime research has utilized data from smaller units of analysis (e.g., cities), immigration policy is overwhelmingly the purview of the federal government. Thus, understanding the more general effect of undocumented immigration on crime is paramount for informing policy discussions.

DEPENDENT VARIABLES

We analyze the four Part I offenses that comprise the *violent crime index* (per 100,000): *murder*, *robbery*, *aggravated assault*, and *rape*.[5] Most of the immigration–crime research has been aimed at examining violent crime (Ousey and Kubrin, 2017), and the violent crime index is one of the most widely used standards for assessing patterns of violent offending (Mosher, Miethe, and Phillips, 2002). Following common practice, we log each crime measure to reduce positive skewness. Throughout the analysis, we first examine the overall violent crime rate and then investigate each crime category separately.

Focal Measure

The lack of criminological research on undocumented immigration is mainly a result of data constraints. Until recently, researchers have not had access to trustworthy estimates of the undocumented population (Warren and Warren, 2013). We use state-level estimates from two reliable and respected sources of the undocumented immigrant population—The Center for Migration Studies (CMS) and the Pew Research Center (Pew). From both sources, we calculate *the proportion of the total population that is undocumented*. Although state-level undocumented estimates are available from CMS for the full study period, Pew estimates are only available intermittently up until 2005. For this reason, we use CMS data in our main analysis and we replicate the results with the available Pew figures in the methodological appendix provided in the online supporting information.[6] The utilization of estimates from two independently derived sources serves as a robustness check for the analytical approach.

Pew Research Center Estimates. The Pew counts are perhaps the most widely used estimates of the undocumented immigrant population by news outlets, academics, and policy makers. In calculating the unauthorized estimates, Pew uses a residual methodology based on Census Bureau data. Variations of the residual method have been extensively used and are generally accepted as the best current estimates of the unauthorized population (Passel and Cohn, 2009). Indeed, independent research using various methods of triangulation, including death and birth records, have substantiated the general accuracy of the residual methodology (Bachmeier, Van Hook, and Bean, 2014; Van Hook, 2016). Stated briefly, this method involves subtracting the number of authorized, or documented immigrants, from the total foreign-born population. The remainder, or residual, is then the estimated number of potentially unauthorized immigrants. Because this residual count of possible unauthorized immigrants often overestimates the actual count, they use

5. In 2011, the definition of rape was changed in the UCR program. To ensure comparability over time in this measure, we use the "legacy" definition of rape in this study.

6. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2018.56.issue-2/issuetoc.

probabilistic methods[7] based on demographic, social, economic, and geographic characteristics of immigrants to classify them as lawful or unauthorized (Passel and Cohn, 2015). State-level estimates of the undocumented population were available from Pew reports for the following years: 1990, 1995, 2000, and then 2005–2014. We linearly interpolate the intervening years to account for the missing Pew estimates.

Center for Migration Studies Estimates. Like the Pew, the CMS uses the residual method based on census data but significantly improves on this technique by accounting for the components of population change (Warren and Warren, 2013).[8] The CMS methodology involves four key steps. First, like the Pew probabilistic methodology, the CMS applies logical edits when calculating residuals (Warren, 2014). These logical edits serve as tools to identify as many lawful resident respondents as possible and are derived from survey responses that are unlikely to apply to someone who is of unauthorized status (e.g., occupations that require legal status and those that receive public benefits that are restricted to legal residents). Second, the CMS calculates independent population controls by country of origin for unauthorized residents, a feature unique to the CMS estimates (Warren, 2014: 308). This second stage is important because the percentage of undocumented immigrants among the foreign-born population can vary considerably based on national origin. Third, with the population controls from step two, final selections are made of individual respondents to be classified as undocumented. Lastly, these estimates are adjusted by the factors that influence yearly fluctuations in the unauthorized population: emigration rates, under-count rates, removals, adjustments of unauthorized to lawful status, and mortality rates.

In addition to providing full data coverage for the study period, there are multiple advantages and sources of validation regarding the veracity of the CMS estimates. For one, unlike alternative sources, the CMS routinely includes estimates of the annual net change in unauthorized populations (undocumented immigrants leaving and entering in a given year), as opposed to simply focusing on entrants. In addition, the CMS estimates produce smaller ranges of sampling error than other sources, thus, providing greater precision for estimating any effects of undocumented immigration on crime. Finally, the CMS methodology has been empirically vetted through the peer-review process (Warren and Warren, 2013; Warren, 2014).

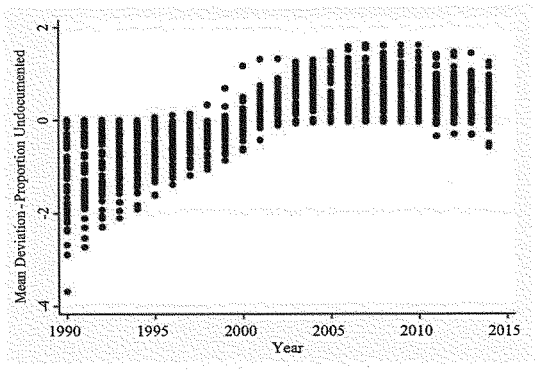
The estimates from both Pew and CMS are notably similar and consistent across years (within-state $r = .93$). The limited variability between Pew and CMS data offers suggestive evidence that they are accurately measuring the unauthorized populace. As Warren (2014: 309) stated, "the close correspondence between estimates derived from such disparate approaches indicates that they are measuring approximately the same population."

We provide a graphic summary of the CMS measure in figure 1. Because we focus our analysis on *within-state change* (detailed later), figure 1 shows the annual distribution of mean deviations in the proportion undocumented (i.e., how each state deviates from its mean over time). This figure demonstrates a uniform increase in the proportion

7. For details on these probabilistic methods, see "Appendix C: Methodology" in Passel and Cohn (2015).

8. For more information on the additional estimates and assumptions used by CMS to calculate the unauthorized population, see Warren and Warren (2013).

Figure 1. Annual Mean Deviations in Estimated Proportion Undocumented (CMS), 1990–2014



ABBREVIATION: CMS = Center for Migration Studies.

undocumented between 1990 and 2014, offering little evidence of significant outliers in the focal independent measure.

INDEPENDENT VARIABLES

To help understand the undocumented immigration–violence relationship, we include a host of important structural factors that have featured prominently in macro-criminological research. Particularly significant among the controls is the *lawful immigrant population* (proportion of the population that are lawful immigrants).[9] During the same period undocumented immigration increased, there was marked growth in lawful immigration as well. We illustrate these trends in figure 2. In line with our focus on within-state change, this scatterplot shows the correlation between changes in undocumented immigration (y-axis) and changes in lawful immigration (x-axis). Although not identical, these trends unsurprisingly track one another ($r = .66$; $p < .01$).[10] Therefore, to separate the effects of unauthorized immigration from general migration trends, accounting for the lawful immigrant population is critical.

A second salient consideration includes measures that capture the degree of *structural disadvantage* (Peterson and Krivo, 2005). In line with previous immigration–crime

9. This measure is calculated as the total foreign-born population minus the undocumented population.

10. This raises potential multicollinearity concerns, but several factors bolster our conclusions. First, as demonstrated in table 2, even after accounting for lawful immigration, undocumented immigration has an independent influence on violence. Second, the consequence of multicollinearity is lack of efficiency, not bias (Berry, 1993). Third, the results replicate across multiple analytical specifications (see the methodological appendix in the online supporting information). Fourth, the variance inflation factors among the covariates are less than 5, which is below the recommended cutoff (10). Lastly, an independent source of variation in unauthorized immigration is identified in the first-stage model of our instrumental variable analysis.

variable analyses that model the selective migration patterns of undocumented immigrants; and a variety of supplemental models and sensitivity analyses. Indeed, of the 57 point estimates reported throughout our analysis (including in the online supporting information), not one shows a positive association between undocumented immigration and violent crime. Such findings diverge from the cross-sectional results reported by Green (2016). In this regard, we exhibit a familiar pattern in immigration–crime research: The results of cross-sectional analyses tend to demonstrate a weak positive immigration–crime association, whereas the results of longitudinal analyses more often show significantly larger, negative effects. Given the more rigorous research design longitudinal analyses afford, we concur with Ousey and Kubrin (2017: 1.13) that "the stronger, negative, and statistically significant association that emerges from ... longitudinal studies may be due more weight than the weak and nonsignificant association that emerges from ... cross-sectional studies."

Although the pattern in the data is clear, not all of the effects are significant. Thus, we interpret these results with appropriate caution and identify several fruitful avenues for future research. The notable distinctions between the weighted and unweighted regressions suggest that the effect of undocumented immigration on violent crime may vary across different types of states. Thus, a logical extension of this article would be to explore the undocumented–violence nexus across different contexts. This approach would align with current efforts to examine variation in the relationship between Latino immigration and crime across traditional and new immigrant-receiving communities (Shihadeh and Barranco, 2013). Related to this point, though the use of state-level data helps ameliorate concerns regarding sampling variability, states likely miss important community-level processes. Thus, as methods for enumerating the unauthorized population improve, researchers would do well in the future to consider the macro-level influence of undocumented immigration on crime at more proximal units of analysis, such as cities or neighborhoods. Within this vein, they should consider how the context of reception might moderate the undocumented–crime nexus. For example, Lyons, Vélez, and Santoro (2013) showed that immigration reduces criminal violence more in cities with pro-immigration policies, such as "sanctuary" policies that formally limit local law enforcement cooperation with immigration authorities, than in cities with a less receptive political climate for immigrants. Similar analyses specific to undocumented immigration, however, await future data collection efforts as longitudinal information on the unauthorized population at lower levels of aggregation is currently unavailable.

Additionally, extending this line of inquiry beyond violent crime is an important consideration for further research. Property offenses may be particularly interesting as there are theoretical reasons to suspect undocumented immigration may have divergent effects on violent and property crime. For example, economic theory suggests that unauthorized immigrants may be criminally motivated by financial gain if few legitimate economic options are available to them. In line with this view, Baker (2015) found that the legalization of nearly 3 million unauthorized immigrants from the Immigration Reform and Control Act of 1986 resulted in significant decreases in property crime. By implication, with increasing numbers of policies and practices at the state and federal levels aimed at economically marginalizing undocumented immigrants, it is plausible to expect an increase in property crime as a result.

Another important area for further inquiry concerns the social *processes* through which undocumented immigration influences violent crime. Although we accounted for a

multitude of macro-social constructs in our analysis, future research is needed to explicate the mechanisms linking unauthorized immigration and violence. Given our findings demonstrating the stability of the undocumented–violence association with and without covariates, we think more explicit focus on difficult-to-measure processes such as informal social control, cultural penetration, and selection may be useful for understanding how undocumented immigration affects criminal violence.

Lastly, it should be noted that we did not examine the impact of transnational criminal organizations that operate along the border (e.g., drug cartels). Rather, in line with virtually all immigration–crime research, we examined how the settlement of foreign-born individuals affects crime in the United States.

Mindful of these limitations, we nonetheless provide insight into an important criminological question by offering suggestive evidence that undocumented immigration (independent of overall immigration) may have contributed to the U.S. drop in recent decades. This finding has significant theoretical and policy implications. In reference to criminological theory, our results run directly counter to predictions rooted in economic marginalization and social disorganization. Originally articulated to explain the high rates of crime in the Polish immigrant communities of Chicago at the beginning of the 20th century, more and more researchers have recently questioned the thesis that today's immigrants disrupt community organization and increase crime (Sampson, 2008). Nevertheless, few had explicitly looked at undocumented immigrants. Given the tremendous economic, social, and legal barriers undocumented immigrants face, this is a stringent test of the marginalization–disorganization perspectives. That is, if certain aspects of contemporary immigration increase crime by destabilizing communities through economic hardship, we should observe this relationship when examining undocumented immigrants. Our results, however, cast doubt on traditional social disorganization's proposed process in which immigrant concentration undermines community organization. Rather, our results align more with the immigrant revitalization thesis, whereby the influx of low crime-prone undocumented immigrants combined with their supporting immigrant networks provide social and economic benefits to communities, thus, reducing the prevalence of violence.

In reference to public policy, at the most basic level, our study calls into question one of the primary justifications for the immigration enforcement build-up. Debates about the proper role of undocumented immigrants in U.S. society will no doubt continue, but they should do so in light of the available evidence. For this reason, any set of immigration policies moving forward should be crafted with the empirical understanding that undocumented immigration does not seem to have increased violent crime. This analysis also speaks to the unintended consequences of border enforcement. Although immigration enforcement may have "backfired" by increasing the population of undocumented immigrants (Massey, Pren, and Durand, 2016), this policy blunder has not come at the expense of public safety. This finding provides clarifying context for why the most ambitious policies aimed at removing "criminal aliens" have not yielded sizeable reductions in crime. For example, the Immigration and Customs Enforcement's Secure Communities (S-Comm) Program was designed specifically as a crime-fighting initiative to identify and deport criminal aliens through state and local collaboration with federal immigration authorities. Despite the fact that by 2013 S-Comm was active in nearly every county and the deportation of aliens with criminal records increased substantially under the program, the results of comprehensive analyses revealed no impact of S-Comm on violent crime (Miles and Cox, 2014; Treyger, Chalfin, and Loeffler, 2014). Our results help explain why;

undocumented immigrants do not increase violence. It is for this reason that other policies specifically targeting unauthorized immigrants, such as Arizona's SB 1070 (2010) and Alabama's HB 56 (2011), are unlikely to deliver on their crime reduction promises.

Although ardent skeptics may remain unconvinced, the weight of the evidence presented here and in supporting work challenges claims that unauthorized immigration endangers the public. At a minimum, the results of our study call into question claims that undocumented immigration increases violent crime. If anything, the data suggest the opposite.

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Michael Light is an assistant professor of sociology at the University of Wisconsin, Madison. His research is primarily focused on crime, punishment, and immigration.

Ty Miller is a PhD candidate in sociology at Purdue University and an incoming assistant professor of sociology and criminology at Winthrop University. His research interests include crime, public policy, substance use, and immigration.

SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Methodological Appendix

Appendix Table 1. First-Stage Effect of the Undocumented Population in 1980 on the Concentration of Undocumented Immigrants between 1990 and 2014: First-Difference Regression Model

Appendix Table 2. Fixed Effects Models of Undocumented Immigration on Crime: Robustness Checks

Appendix Table 3. First-Difference Models of Undocumented Immigration on State-Level Violent Victimization Rates, 2000–2012

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IMMIGRATION AND THE RECENT VIOLENT CRIME DROP IN THE UNITED STATES: A POOLED, CROSS-SECTIONAL TIME-SERIES ANALYSIS OF METROPOLITAN AREAS*

JACOB I. STOWELL
Department of Criminal Justice and Criminology
University of Massachusetts–Lowell

STEVEN F. MESSNER
KELLY F. MCGEEVER
LAWRENCE E. RAFFALOVICH
Department of Sociology
University at Albany–State University of New York

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A good deal of research in recent years has revisited the relationship between immigration and violent crime. Various scholars have suggested that, contrary to the claims of the classic Chicago School, large immigrant populations might be associated with lower rather than higher rates of criminal violence. A limitation of the research in this area is that it has been based largely on cross-sectional analyses for a restricted range of geographic areas. Using time-series techniques and annual data for metropolitan areas over the 1994–2004 period, we assess the impact of changes in immigration on changes in violent crime rates. The findings of multivariate analyses indicate that violent crime rates tended to decrease as metropolitan areas experienced gains in their concentration of immigrants. This inverse relationship is especially robust for the offense of robbery. Overall, our results support the

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hypothesis that the broad reductions in violent crime during recent years are partially attributable to increases in immigration.

Academics and political pundits have puzzled over the dramatic reduction in crime in the United States that has been observed over the last decade or so. With crime rates approaching 40-year lows, the drop in crime has been recognized as a welcome, yet largely unexpected, phenomenon. In searching for explanations for this pattern, factors ranging from increased levels of incarceration to the legalization of abortion have been cited as possible explanations (Levitt and Dubner, 2005: 117–46; see also Blumstein and Wallman, 2000). Despite widespread speculation, however, empirical research has yet to identify the leading causal mechanisms behind this fortuitous social change.

In an attempt to cast new light on this discussion, the current research focuses on a potentially important causal factor: the degree to which immigration may have contributed to the observed reductions in crime. Traditionally, both criminological theory and conventional wisdom have held that immigration is a social process that promotes, rather than suppresses, crime (Martinez, 2002). Taking a historical view of eras preceding the recent crime drop, Gurr (1989: 76) argues that "America's three great crime waves can be linked to immigration . . ." [and that] ". . . episodes of violent crime wound down as immigrants were incorporated into the expanding economy."[1] The belief that immigrants are responsible for crime has filtered into the public consciousness and has been, and continues to be, used to fuel nationalistic/isolationist rhetoric and anti-immigration movements (Brimelow, 1996; Lamm and Imhoff, 1985).

The longstanding and widely held notion that immigration leads to crime seems to be at odds with recent trends at the national level. According to the Bureau of Justice Statistics, continuing a decade-long decline, national violent victimization rates reached an all-time low in 2002. Furthermore, the overall decrease in violent crime is not attributable to large declines in one particular type of offending, as the rates of homicide, rape, robbery, and assault have all experienced substantial reductions.[2] At the same time that violent crime rates have been falling dramatically, immigration to the United States has been thriving. Since 1990, the size of foreign-born population grew by 56 percent, which is an addition of nearly 11 million individuals.[3] The fall in crime rates has thus been accompanied by the influx of large numbers of foreign-born individuals.

1. Gurr (1989) and Brimelow (1996) argue that America's "three great crime waves" began in 1800, 1850, and 1960; each lasted between 20 and 30 years.
2. Bureau of Justice Statistics (http://www.ojp.usdoj.gov/bjs).
3. U.S. Census Bureau American FactFinder (http://factfinder.census.gov/home/saff/main.html?_lang=EN).

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In addition to the evidence provided by these simple trends, an emerging body of scholarship has raised questions about whether immigration is related to increased levels of violent crime (Hagan and Palloni, 1998; Lee, 2003; Lee and Martinez, 2002; Martinez, 2002; Martinez and Lee, 2000). Using neighborhood-level data, Martinez and colleagues consistently find little empirical support for the immigrant/crime link. Indeed, as Martinez and Lee (2000: 486) argue, "in many cases, compared with native groups, immigrants seem better able to withstand crime-facilitating conditions than native groups." Findings such as these have led some researchers to take seriously the notion that immigration may contribute to *lower* crime rates. For example, Sampson and Bean (2006: 21) have recently speculated that although it has been largely overlooked, "the broad reduction of violence in the United States over the past decade was due in part to increasing diversity and immigration" (see also Sampson, 2006).

When considering the connection between immigration and crime, it is important to note that Sampson and colleagues do not attribute the crime drop solely to the fact that immigrants themselves are generally less likely to engage in criminal behavior, although evidence exists to support the claim of lower levels of offending for immigrant populations. In his study of Chicago neighborhood residents, Sampson (2006:A27) reports that immigrants were "45 percent less likely to commit violence than were third-generation Americans." To the extent that a similar pattern is observed more generally, it stands to reason that the increase in immigration has contributed to the crime drop because the *immigrants* themselves commit fewer criminal acts. Another intriguing aspect of this relationship, according to Sampson, is the fact that *communities* in which immigrants settle are also less violent; this benefit extends to native-born residents. Sampson (2006:A27; see also Sampson and Bean, 2006) reports that "living in a neighborhood of concentrated immigration is directly associated with lower levels of violence" for non-Latino whites and blacks as well. It is not yet clear, however, the degree to which this latter, macrolevel immigrant buffer is associated with the broad reductions in crime.

The purpose of this study is to examine systematically the extent to which immigration helps explain recent changes in serious crime rates in the United States. We go beyond previous research by estimating dynamic models using pooled, cross-sectional time-series data for a relatively large sample of metropolitan areas during the 1994–2004 period. Given that the theoretical speculations about the impact of recent immigration on criminal violence have been cast primarily in terms of "change," dynamic models are most appropriate for assessing these hypotheses. In short, we attempt to cast new light on the role of increased immigration in the widely observed, but poorly understood, crime decline in the United States over recent years.

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THEORY AND PREVIOUS RESEARCH

THE CHICAGO SCHOOL AND CLASSICAL SOCIAL DISORGANIZATION THEORY

The social disorganization perspective associated with the Chicago School has provided the theoretical foundation for most of the extant research on immigration and crime (see Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002, 2008; Martinez, Stowell, and Cancino, 2008; Stowell, 2007). The theoretical link to this topic is clear given that this perspective represents "arguably . . . the most important and well-known assessment of the immigration and crime experience in the United States" (Martinez, 2008: 501). Because more comprehensive discussions of the theory can be found in the literature, we provide an abridged review here (see Bursik and Grasmick, 1993; Kornhauser, 1978).

Shaw and McKay (1969 [1942]) provide the classic application of the insights of the Chicago School for an understanding of crime. Adhering to the principles of urban ecology, Shaw and McKay argue that structural characteristics of urban neighborhoods, and not the characteristics of their residents, make them particularly susceptible to the commission of delinquent acts. Social disorganization theory identifies three primary structural characteristics (economic deprivation, residential instability, and racial/ethnic heterogeneity) that can be used as proxies to gauge the extent to which an area is socially disrupted (see Sampson and Groves, 1988: 780).

When considering the Chicago School tradition, it is important to note the particular attention disorganization theorists pay to what they perceive to be the disruptive force of immigration. Indeed, social disorganization theory holds that increased disorder and, by implication, rates of crime are especially likely during periods of large-scale immigration. Disorganization theorists argue that community formation is undermined by an increase in language and cultural differences maintained by a diverse immigrant population (Thomas and Znaniecki, 1920). Moreover, informal networks of monitoring and social control prove difficult to establish because the areas into which immigrant groups settle are often resource poor and residentially unstable. Thus, the theory speaks directly to why we would expect higher rates of violent crime in areas with large proportions of immigrants and during times with higher levels of immigration.

RESEARCH ON IMMIGRATION

In contrast with the disorganization perspective's assertion of a positive relationship between immigration and crime, noncriminological research on immigration portrays the process of immigration in a more positive light and offers insight into the possibility of an inverse association

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between immigration and crime (Kao and Tienda, 1995; Model, 1995; Ogbu, 1991; Palloni and Morenoff, 2001; Zhang and Sanders, 1999). Indeed, immigration scholars refer to the fact that immigrants (and particularly Latinos) fare better than native-born groups on several health-related outcomes (i.e., infant birth weight as well as adult and infant mortality) as a paradox (or the "Latino paradox") (see Palloni and Morenoff, 2001; Sampson and Bean, 2006). According to the literature, these advantages are unexpected "given the relatively high levels of disadvantage" experienced by many immigrants in this country (Sampson and Bean, 2006: 20).

A leading explanation for the observed differences is selectivity theory, which posits that immigrants who decide to come to the United States do so for the opportunity to improve their life chances. Central to the selectivity theory is the belief that "persons who migrate are more ambitious, talented, and diligent than those who do not" (Model, 1995: 538). Furthermore, selectivity explanations suggest that in addition to these dispositional qualities, migrants are also more likely to have access to stronger social networks, which "provide information, facilitate the move, and ease the process of adjustment and adaptation" (Palloni and Morenoff, 2001: 160; see also Hagan, Levi, and Dinovitzer, 2008). Therefore, the claims regarding immigrants made by selectivity theory are at odds with those typically associated with classical social disorganization theory. The selectivity hypothesis also suggests that immigration may not be associated with the disruption of ecological dynamics as cited by the Chicago School theorists. Therefore, we believe that theories of immigration offer strong reasons to anticipate an inverse relationship between immigration and crime.

RESEARCH ON IMMIGRATION AND CRIME

Published studies examining the impact of immigration on crime in American society emerged nearly a century ago (see Martinez, 2002; Martinez and Lee, 2000; Rosales, 1999). The early research on this topic, despite their methodological limitations, raised questions about whether immigration promoted higher levels of crime. As rates of immigration have increased over the past decade, criminologists have shown a renewed interest in understanding the extent to which patterns of criminal behavior are tied to the influx of foreigners (Martinez, 2002, 2008; Martinez and Valenzuela, 2006). With the increased interest in this topic, the growing body of research literature on immigration and crime contains studies that focus on several distinct substantive dimensions and different levels of analysis.

Individual-level studies consistently report that immigrants are less likely to be involved in or to be institutionalized for criminal involvement than native-born individuals (see Butcher and Piehl, 1998a; Hagan and

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Palloni, 1998; Sampson, 2008; Sampson and Bean, 2006). Furthermore, research also confirms an interesting pattern, whereby the likelihood of criminal involvement increases across immigrant generations (see Hagan, Levi, and Dinovitzer, 2008; Hagan and Palloni, 1999). According to Hagan, Levi, and Dinovitzer (2008: 101), although the effect "wears off" over time spent in the United States, first-generation and, to a lesser extent, second-generation immigrants "enjoy some protection, or resilience, from crime."

Research also fails to find support for the increased criminality of undocumented immigrants; this group is portrayed as the most crime-prone segment of the foreign population (Hickman and Suttorp, 2008; Nevins, 2002; Sampson, 2008: 29). In a study of immigrants released from the Los Angeles County Jail, Hickman and Suttorp (2008: 77) conclude that "deportable aliens (a combination of both undocumented immigrants and those with an expired visa status) [do not represent] a unique threat to public safety." Most of the recent criminological research on immigration, however, examines the association between immigration and crime using macrolevel data. Because the current study follows this tradition, the following review focuses on the impacts of immigration on aggregate levels of crime. A schematic summary of the relevant contemporary research is provided in table 1.[4]

The work of Martinez and colleagues represents the most extensive research program on the macrolevel connection between immigration and crime (Lee, 2003; Lee and Martinez, 2002; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Martinez and Lee, 2000; Martinez, Stowell, and Cancino, 2008; Nielsen, Martinez, and Lee, 2005). The research conducted by Martinez has focused on both neighborhood- and city-level influences on violent crime, and particularly on levels of homicide. The examination of this question at different levels of aggregation certainly contributes to a more comprehensive understanding of the impact of immigration on violent crime. Furthermore, although much of the existing research relies on cross-sectional analytical designs, the findings reported by Martinez and colleagues have implications for the current study because they provide the most systematic tests of the immigrant/crime link to date.

In *Latino Homicide* (2002), Martinez compares neighborhood structural conditions and homicide rates between Latino and non-Latino racial groups in five American cities (Miami, El Paso, San Diego, Chicago, and Houston). Using racially and ethnically disaggregated victimization data, he finds that rates of Latino homicide victimization tend to be higher than

4. For more comprehensive reviews of contemporary and historical research on immigration and crime, see Martinez (2002, 2006) and Peterson and Krivo (2005).

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Table 1. Summary of Contemporary Research on Immigration and Crime

Author	Measure of Immigration	DV	Impact	Data	Design	Study Period
Metropolitan Level						
Butcher and Piehl (1998b)	% Recent immigrant (<1 year)	Overall and violent crime rates	NS	National sample (N = 43)	Cross sectional/longitudinal	1980–1990
Hagan and Palloni (1998)	% Undocumented immigrants	Overall, violent, property arrest rates	+/NS	Sample from 5 western states (N = 34)	Cross sectional	1980
Reid et al. (2005)	% Foreign-born noncitizens % Recent immigrant (<10 years) % Foreign-born Asian % Foreign-born Latino English proficiency	Homicide, robbery, larceny, burglary rates	–/NS	National sample (N = 150)	Cross sectional	2000
Phillips (2002)	% Foreign-born	Race-specific homicide rates	NS	National Sample (N = 191)	Cross sectional	1990
Neighborhood Level						
Lee (2003)	% Recent immigrant (<10 years)	Homicide	–/NS	El Paso, Miami, San Diego	Cross sectional	1985–1994
Lee and Martinez (2002)	% Recent immigrant (<10 years)	Homicide	No regressions	Miami	Cross sectional/Descriptive	1985–1995
Lee, Martinez, and Rodriguez (2000)	Latino immigration	Homicide	No regressions	Miami, El Paso	Cross sectional/Descriptive	1985–1994
Lee, Martinez, and Rosenfeld (2001)	% Recent immigrant (<10 years)	Homicide	–/NS	Miami, El Paso, San Diego	Cross sectional	1990
Martinez (2002)	% Recent immigrant (<10 years)/ethnicity	Various homicide types	No regressions	Miami, El Paso, San Diego, Chicago, and Houston	Longitudinal/descriptive	1980–1994
Martinez and Lee (2000)	% Foreign-born/Ethnicity	Homicide	No regressions	National/city trends	Longitudinal/descriptive	1980–1994
Martinez, Stowell, and Cancino (2008)	% Recent immigrant (<10 years)	Homicide	–/NS	San Diego, San Antonio	Cross sectional	2000
Morenoff and Astor (2006)	Immigrant generation	Self-reported nonlethal violence	–	PHDCN	Cross sectional	1995–2002
Sampson, Morenoff, and Raudenbush (2005)	Immigrant concentration	Self-reported nonlethal violence	–	PHDCN	Cross sectional	1995–2002
Sampson (2008)	Immigrant concentration	Self-reported male violence	–	PHDCN	Cross sectional	1995–2002
Sampson and Morenoff (2004)	Immigrant generation	Homicide	–/NS	PHDCN	Cross sectional	1995
Sampson, Raudenbush, and Earls (1997)	Immigrant concentration	Perceived violence, victimization, homicide	+/–/NS	PHDCN	Cross sectional	1995
Stowell (2007)	Immigration measures based on ethnicity	Total, expressive, instrumental violence	–/NS	Miami, Houston, Alexandria (VA)	Cross sectional	2000
City Level						
Martinez (2000)	Immigrant concentration	Latino homicide rates	+/NS	National sample (N = 111)	Cross sectional	1980

ABBREVIATIONS: DV = dependent variable; NS = not significant

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similar rates for non-Latino whites and lower than those for non-Latino blacks. The methodology employed in this analysis is largely descriptive, but Martinez points out that Latinos are victims of homicides at lower rates than non-Latino blacks, even though both groups live in structurally similar neighborhoods. Indeed, Martinez (2002: 141) argues that rates of Latino homicide are lower than expected given the levels of structural disadvantage for many Latino neighborhoods.

Quantitative evidence in the research literature indicates a negative effect of immigration on violent crime. Again using racially disaggregated homicide data for three cities, Lee, Martinez, and Rosenfeld (2001; see also Lee, 2003) find that the size of the recently arrived immigrant population in Miami is inversely related to levels of homicide. For the other two cities under investigation (El Paso and San Diego), the authors report that immigration has nonsignificant effects in all but one regression analysis. The authors interpret the combination of negative and null findings as nonsupportive of either theoretical expectations or popular sentiment regarding the association between immigration and crime (Lee, Martinez, and Rosenfeld, 2001: 571). Similarly, in a study of Chicago neighborhoods that collected observational data using the systematic social observation method, Sampson and Raudenbush (1999) report an inverse association between community levels of concentrated immigration and robbery rates.[5] The research mentioned above does find some support for social disorganization theory in that social structural factors such as poverty and residential instability have significant positive effects on violence.

Sampson and colleagues have also begun to conduct research on immigration using the data collected as part of the Project on Human Development in Chicago Neighborhoods (PHDCN) (see Sampson, 2008; Sampson and Bean, 2006; Sampson, Morenoff, and Raudenbush, 2005). For example, in an analysis of the racial/ethnic differences in levels of criminal involvement, Sampson, Morenoff, and Raudenbush (2005) conclude that both individual- and community-level indicators associated with immigration predict lower levels of criminality. Specifically, Sampson, Morenoff, and Raudenbush (2005: 231) find that the negative "contextual effect" of living in immigrant communities emerged "even after a host of other factors, including the immigrant status of the person, were taken into account." In addition, Sampson and Bean (2006: 21) argue that although levels of criminal participation differ across generations, their data suggest that "it is no longer tenable to assume that immigration and diversity

5. The "concentrated immigration" variable is an index composed of percent Latino, percent foreign born, and the density of children (percent of persons aged 6–15 years).

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automatically lead to social disorganization and consequently crime" (see also Sampson, 2006).

Although research has shown that immigration is not associated with higher levels of crime across neighborhoods, other studies examine this relationship using larger area units. In their study of the impacts of noncitizen and illegal immigration on 1980 metropolitan arrest rates, Hagan and Palloni (1998) find that the size of the illegal immigrant population is unrelated to either violent or property arrest rates (see also United States Commission on Immigration Reform, 1994). Although Hagan and Palloni (1998: 380) report that the number of noncitizens is related to an increase in property arrests, they conclude that "there is no compelling or systematic evidence at the SMSA [Standard Metropolitan Statistical Area] level that immigration causes crime." Similarly, in a study of 43 metropolitan areas, Butcher and Piehl (1998b) find that whereas immigrants generally tend to settle in areas with high crime rates, changes in the size of the immigrant population between 1980 and 1990 are unrelated to changes in either overall or violent crime rates. Most recently, in an analysis of metropolitan-level data, Reid et al. (2005) find immigration to be either unrelated or inversely related to levels of violent and nonviolent crime.

A series of studies also shows that Latino-specific homicide rates are influenced by the social structural conditions of cities. Indeed, the city-level studies that focused on Latino violence were among the first to call attention to the important role that immigration may have in shaping patterns of lethal violence. In research that compared victimization rates in Miami and El Paso, Lee, Martinez, and Rodriguez (2000) find that killings involving Latinos are distributed similarly across homicide types (i.e., acquaintance, intimate, and stranger) in both cities. The authors conclude that the higher number of homicides committed in Miami is reflective of the more violent urban and regional contexts in that area of the country. Similarly, using a sample of 111 U.S. cities, Martinez (1996) finds that economic inequality among Latinos is associated with increased levels of Latino homicide victimization. A similar finding is reported by Martinez (2000), who reports that immigration is associated with increased levels of felony homicides (i.e., those committed during the commission of another crime). Martinez (2000) concludes that the increase in felony homicides in areas with large Latino immigrant populations is likely the result of increased contact with other non-Latinos (see also Blau, 1977). It is important to note a more general trend in these studies; the social structural characteristics are related to homicide, and this pattern is consistent with the expectations of social disorganization theory.

Two general conclusions can be drawn from the research on the effects of immigration on crime. First, the weight of the evidence suggests that immigration is not associated with increased levels of crime. To the extent

that a relationship does exist, researchers often find a negative effect of immigration on levels of crime, in general, and on homicide in particular. This finding runs counter to the theoretical logic of the classical social disorganization perspective. Second, the criminological research on immigration also finds partial support for the theory, which is evidenced by the strong and significant impacts of other social structural characteristics on levels of crime. Just as social disorganization theory predicts, indicators such as poverty and residential instability emerge as significant predictors of crime. In light of these contrasting results, immigration researchers generally interpret the unexpected negative impact of immigration on crime as a limitation of social disorganization theory.

LIMITATIONS OF PREVIOUS RESEARCH

As mentioned, most prior immigration/crime research has used a combination of neighborhood-level data available for decennial years and has independently collected crime measures. This approach has advanced knowledge on this topic considerably by casting new light on the associations between immigration and crime. Still, it is not clear the extent to which temporal variations in criminal violence may be linked to the recent surge in immigration. Yet as displayed in table 1, only one study employing multivariate modeling has incorporated longitudinal analyses, and this study is based on a relatively small sample of cases—43 metropolitan areas (Butcher and Piehl, 1998b). Moreover, this study examines change in crime from 1980 to 1990, which thereby misses completely the decade of large-scale immigration and the dramatic crime decline.

In addition, more recent immigrant settlement patterns indicate the need to broaden the geographical scope of research on immigration and crime. The work of Martinez and colleagues, which comprises the bulk of the research literature, focuses largely on the influence of immigration on homicide victimization across neighborhoods in a small number of Sunbelt cities (Miami, El Paso, San Diego, and Houston) (Lee, 2003; Lee and Martinez, 2002; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Martinez and Lee, 2000; Nielsen, Martinez, and Lee, 2005; Stowell, 2007; for exceptions, see Martinez, 1996, 2000). Clearly, focusing scholarly attention on such areas is warranted given the fact that they continue to attract a disproportionate share of the foreign-born population. Recent census data indicate that approximately two thirds of foreigners reside in one of six states (California, Texas, Florida, Illinois, New York, and New Jersey).[6]

6. Migration Policy Institute (http://www.migrationinformation.org/datahub/maps.cfm).

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Moreover, because Latinos represent the bulk of the immigrant population, it is appropriate that studies of immigration and crime concentrate on areas that tend to be traditional destinations for foreign-born Latinos.

Yet despite the continued settlement of immigrants among relatively few states, there is also the need for scholars to be sensitive to changes to this general immigration trend. In part, this is driven by the diversity of the contemporary immigrant stream, which is well documented in prior research (Alba et al., 1999; Kleniewski, 1997; Massey, 1995). Furthermore, recent research on immigrant settlement patterns has found that, increasingly, foreign-born individuals are not choosing to live in traditional gateway states (i.e., California and New York). A recent article in the *New York Times* reports that the size of the immigrant populations in Midwestern and Southern states is growing at a high rate (Lyman and Goodman, 2006). According to the 2006 American Community Survey, between 2000 and 2006, the size of the foreign-born population in ten states grew by at least 40 percent.[7] It is not yet known the degree to which the growth in the immigrant population in these areas has had a meaningful effect on their levels of criminal violence. Therefore, obtaining a more comprehensive understanding of whether immigration facilitated the observed decline in crime requires researchers to consider not only areas classified as traditional immigrant destination cities but also those that are emerging as such.

THE CURRENT STUDY

This study seeks to advance current knowledge on the relationship between immigration and crime by addressing several limitations of previous research. Most notably, we employ a longitudinal analytical design to test whether changes in levels of immigration are systematically associated with the recent reductions in crime in the United States. Previous claims by scholars in the field imply dynamic influences of immigration on crime. For example, Martinez (2006: 12) argues that although research has yet to reveal "a definitive causal connection . . . over time it appears that more immigrants means less violence." Prima facie evidence certainly supports this argument; yet whether a statistical relationship exists, net of other social structural conditions, remains an open question. Furthermore, we use data for a broad array of geographic areas, which may cast new light on how nontraditional immigration settlement patterns may influence the relationship between immigration and crime (see Frey, 2006; Lyman and Goodman, 2006).

7. The ten states with the fastest-growing foreign-born populations are Delaware, South Carolina, Nevada, Georgia, Tennessee, Alabama, Arkansas, North Carolina, and Indiana.

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DATA

The data used in this study are collected from two sources. The independent variables are constructed using annual data collected during the Current Population Survey (CPS), as provided by the Integrated Public Use Microdata Series (IPUMS) (see King et al., 2004).[8] The CPS provides data for individuals aged 15 years and older from a national probability sample of approximately 50,000 households (see Noonan, Smith, and Corcoran, 2007). More specifically, this study makes use of the March Annual Demographic file and Income Supplement, which contains information on an array of demographic and socioeconomic factors. Additionally, what makes the CPS data particularly well suited for our purposes is that the data also identify the metropolitan area in which a respondent resides. Using the geographic identifiers, we were able to generate metropolitan-level estimates for each indicator to be included in the regression models. The indicators of criminal behavior were constructed using the Federal Bureau of Investigation's (FBI's) Uniform Crime Reports (UCR). The UCR data for 1994–2004 were extracted from the Interuniversity Consortium for Political and Social Research. Specifically, all violent crime counts known to police, and the corresponding population totals, were collected for the 103 metropolitan areas previously identified.

We recognize that sampling variability introduces some error in metropolitan-level estimates, particularly in areas with small sample sizes. In their discussion of the CPS data, the Census Bureau indicates that "estimates for the smaller metropolitan areas (those with populations under 500,000) should be used with caution because of the relatively large sampling variability associated with these estimates."[9] For this reason, only metropolitan areas that had a population of 500,000 or more in 2000 are included in our sample. Using this population selection criterion, our sample includes 103 metropolitan regions (see appendix A). The demographic compositions of the metropolitan areas in our sample are highly correlated with their corresponding decennial census values (see appendix B), which suggests that sampling error is not likely to have unduly influenced the estimates in our sample.

Because immigrants tend to settle in large urban areas, our sample of metropolitan areas includes a large share of the nation's foreign-born

8. The IPUMS-CPS is derived from the CPS conducted by the U.S. Census Bureau. An advantage of IPUMS-CPS data is that they are "harmonized" to ensure compatibility over time (http://cps.ipums.org/cps/).

9. Minnesota Population Center data on the population statistics of metropolitan areas in Minnesota: http://cps.ipums.org/cps-action/variableDescription.do?mnemonic=METAREA.

population. Again using decennial census data for the purposes of comparison, our sample covers nearly 90 percent of all immigrants who reside in a metropolitan area and more than 84 percent of all foreign-born individuals living in the United States (89.2 percent and 84.2 percent, respectively). Our sample also includes similar shares of the recent immigrant population, accounting for 89.1 percent of the individuals living in a metropolitan region and 83.8 percent of those who arrived during the 1990s. This stable pattern clearly indicates that the largest metropolitan areas continue to attract and to retain most of those born outside of the United States.

Before describing the measures, the rationale for our focus on metropolitan areas warrants continued discussion.[10] This study departs from most existing research on immigration and crime, which examines the impact of immigration on neighborhood levels of violence (see Martinez, 2006). Although neighborhood-level analyses are obviously valuable and are within the tradition of the Chicago School, such analyses to date have encompassed an important but limited portion of the territory in which immigrants reside. More central to the current study is the fact that the bulk of previous research on this topic has been cross-sectional in nature, as noted above. These limitations are understandable given practical considerations of data availability. Tract-level information on immigrant populations from the U.S. Census is available only for decennial years, and it would not be feasible to generate estimates from annual survey data at this level of geography. In addition, crime data for urban neighborhoods must be secured from local police agencies, which becomes a monumental task for many urban communities. Data for many metropolitan areas from Census and FBI sources, in contrast, are available to permit pooled, cross-sectional time-series analyses of the impact of changes in immigration on changes in crime rates.

We also note that there is precedent in the literature for analyses of the relationship between immigration and crime at the metropolitan level (see Butcher and Piehl, 1998b; Hagan and Palloni, 1998; Phillips, 2002; Reid et

10. The selection of the unit of analysis for macrolevel research on homicide has long been the subject of debate (e.g., Bailey, 1984; Land, McCall, and Cohen, 1990; Messner, 1982). Focusing specifically on homicide, Parker, McCall, and Land (1999: 118–9) adopt a strong position, positing that "the unit of analysis *is not* [original emphasis] a relevant concern for studies of homicide" and concluding that "any general theory of homicide should generate empirical invariant findings regardless of the level of analysis examined." We adopt a more moderate position and acknowledge that certain macro units might be more desirable than others depending on the substantive objectives of the research and the hypothesized causal processes. As explained in the text, our decision to use metropolitan areas is driven by pragmatic considerations. Data for these units allow us to conduct dynamic analyses with refined time resolution (annual change) for a reasonably diverse and relatively large sample of ecological units.

al., 2005). For example, using a sample of 191 metropolitan areas, Phillips (2002) reports that immigration is unrelated to white, black, and Latino homicide rates. Most recently, using a national sample of 150 metropolitan areas, Reid et al. (2005) find a series of negative and null effects of immigration on both violent and nonviolent crime rates. Although the metrolevel studies conducted by Butcher and Piehl (1998b) and Hagan and Palloni (1998) employ data for earlier decades (1980 and 1990), their results offer little support for the notion that immigration is positively associated with levels of criminal deviance. It is worth noting that even though metrolevel studies represent a small subset of the research on immigration and crime, the findings from these studies reveal that factors such as economic disadvantage and residential instability are related to higher levels of crime. Such results mirror closely those presented in the studies that concentrate on smaller geographic areas (see Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002, 2006; Martinez and Lee, 2000; Morenoff and Astor, 2006; Sampson and Bean, 2006; Stowell, 2007).

MEASURES

DEPENDENT VARIABLES

The dependent variables for the analysis are the FBI's violent crime index and the constituent elements of the index (i.e., rates of criminal homicide, robbery, forcible rape, and aggravated assault). The violent crime rate is the sum of the four violent offenses. The overall and specific crime rates are expressed per 100,000 based on population totals as provided from the UCR.

INDEPENDENT AND CONTROL VARIABLES

Our primary independent variable, concentrated immigration, is a measure that has been used extensively by Sampson and colleagues (Morenoff and Sampson, 1997; Sampson and Morenoff, 2004; Sampson, Raudenbush, and Earls, 1997). It is composed of the summed z-scores for the percent Latino and the percentage of the population who are foreign born. In the current study, we employ a general measure of immigration based on the IPUMS "nativity" variable, which includes all individuals born outside of the United States.[11]

11. We recognize that the issue of undocumented immigration is often conflated with immigration more generally. We explored the possibility of including a proxy for the number of undocumented immigrants in our models, following a methodology that has been used in previous research (see Hagan and Palloni, 1998; Passel, 2005). However, because the annual estimates tend to be unstable, we could not include such a measure in our regression models. As Passel (2005: 10) argues, because of "inherent uncertainties in the residual technique, the difference in

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We also collected data on multiple variables that have been previously identified as having an effect on crime rates: population (logged); racial composition (percent of the population who are non-Latino black); age/sex structure (percent of the population who are males aged 18–24 years); educational attainment (percent of population 25 years and older with more than a high-school education); employment status (percent of individuals identified as in or not in the labor force who are unemployed); family formation (percent of total families headed by unmarried females with at least one child present); divorce rate (percent of the population aged 15 years and older who are divorced); poverty (percent of persons living in poverty); and the mean household income. The adult/child ratio has also been identified as a possible measure of informal social control. We created the adult/child ratio as the number of individuals aged 18 years and older divided by the number of individuals aged less than 18 years. In addition, residential stability has been linked to crime rates and is measured by those who moved (percent of the population who are living in a different house than they were 1 year ago) and those who live in owner-occupied housing units. Any regional effects are captured by dummy variables for the broad U.S. Census regions (South, West, and Northeast, with Midwest serving as the reference category).

We include two final control variables that have been identified as potentially important factors for explaining the dramatic crime drop in the United States in the 1990s—police size and a proxy for firearm availability (Levitt, 2004; Rosenfeld, 2002). Police size is measured as the total number of sworn officers per 1,000 population based on the 1994–2004 UCR program: Police Employee (LEOKA) Data. Researchers on firearms generally agree that, in the absence of accurate administrative data or representative survey data, the most reliable proxy measure of firearm availability is the proportion of suicide deaths where guns were used (Azrael, Cook, and Miller, 2004; Cook and Ludwig, 2006; Kleck, 2004).[12] We constructed such a proxy measure using data from the Multiple Cause of Death Mortality Data from the National Vital Statistics System of the National Center for Health Statistics.[13]

successive annual estimates of the unauthorized population is not a valid measure of growth." The development of more reliable estimates of this segment of the foreign-born population is certainly an important issue that should be addressed in future research.

12. Kleck (2004) reports that the suicide proxy measure is valid for cross-sectional research but advises against using any of the standard proxies for longitudinal analyses. In contrast, Azrael, Cook, and Miller (2004) as well as Cook and Ludwig (2006) conclude that the percentage of suicides involving guns is a useful proxy for both kinds of analyses.

13. Another factor that has been cited as a likely cause of both the rise and fall of violent crime is the operation of crack cocaine markets (Blumstein, Rivara, and

Many of our covariates are highly correlated, which creates a collinearity issue. We therefore followed standard procedures and conducted principle component analysis on residential stability, percent female-headed household, percent poverty, percent with more than a high-school education, percent young males, mean household income, percent non-Latino black, percent unemployed, and the percent divorced. We repeated the factor analysis for each of the 11 time periods. An economic resources factor emerges from the analysis consistently. Variables that load highly (average factor loadings over the 11 years in parentheses) include the percent not in poverty (.81), percent with more than a high school education (.78), mean household income (.88), and percent female-headed households (.67).[14] With an average eigenvalue of 2.50, this explains, on average, 62.45 percent of the variation in the construct. We created an economic resources index using the factor scores.

Residential stability variables (percent owner occupied and percent of the population who moved) are also highly correlated. We therefore computed an index using the summation of the *z*-scores. All other variables are entered into the regression as single-item covariates. Finally, all variables (save the regional dummies) are expressed as first differences for change–change dynamic time-series models. Descriptive information is provided in appendix C.

STATISTICAL PROCEDURES

Unit–root tests indicate that MSA-specific crime rates are difference–stationary processes, so they are measured as annual change (Raffalovich,

Rosenfeld, 2000). In an effort to control for this variable, we collected data on arrest rates for the sale and manufacture of cocaine from the UCR Program County-Level Detailed Arrest and Offense Data for our study period. Unfortunately, after aggregating to the Metropolitan Statistical Area (MSA) level, we found that this measure contains significant missing data for strategic cases in a study of the effects of immigration. Specifically, data are not available for the seven Florida MSAs for 1996–2000. Alabama and South Carolina MSAs had missing data for 1994, Chicago failed to report data from 1995 on, and the Milwaukee MSA did not report arrest data for 1998–2000. Despite the missing data, we estimated equations for the reduced sample and found no significant effects of the cocaine variable on changes in the violent crime index or the rates for the specific violent offenses. The only substantive difference between models with and without the cocaine measure concerning the immigration variable is that the effect on the violent crime index, although still negative, falls below statistical significance. The significant negative effect on robbery rates remains in the analyses with the cocaine measure. See Ousey and Lee (2007) for evidence indicating that the effect of cocaine markets on homicide rates has attenuated over time.

14. To be consistent with the other factors in the resource index, the polarity of the female-headed household was adjusted. As originally constructed, the factor loading for female-headed households was –.67.

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1994). Because we have data for a relatively large number of MSAs (103) but a short time-span (11 years), we estimate random-effect variable parameter regression models (Goldstein, 1995; Longford, 1993; Snijders and Bosker, 1999):

$$\Delta C_{i,t} = B_{i,0} + \sum B_{ki} X_{kit} + \sum B_m Z_{mi} + \sum B_r X_{rit} * Z_{ri} + \varepsilon_{i,t} \quad (1)$$

where:

$\Delta C_{i,t}$ is annual change in the MSA-specific crime rate for MSA i in year t,

$X_{ki,t}$ are independent variables that vary across both MSAs and time,

Z_{mi} are independent variables that vary across MSAs but are constant over time,

$X_{rit}*Z_{ri}$ are cross-level interactions, and $\varepsilon_{i,t}$ is the stochastic error.

B_{ki} *and* B_m are parameters to be estimated.

$B_{i,0}$ *and* B_{ki} are assumed to have multivariate normal distributions across MSAs and the error process is initially assumed to be AR(1):

$$e_{i,t} = p_i\, e_{i,t-1} + v_{i,t} \quad (2)$$

We use full-information maximum likelihood to estimate all models. Regarding the data used in the current study, with 103 cross-sectional units and 10 time points after computing first differences, the number of cases for analyses would be 1,030 were data available for all variables. CPS data were not available for Orange County, CA, for 1994–1995, which reduces the N to 1,028. Data were also missing on police size for Worcester, MA, for the years 1994–1999. Given that police size serves as a control variable and not as a strategic independent or dependent variable, we replaced the missing values on police size with the average value for the years with data for Worcester. Analyses without imputed values (available upon request) yield highly similar results to those reported in the tables.

RESULTS

We begin the analysis with an examination of the trends in selected variables of primary interest for our sample of metropolitan areas, as depicted in figure 1. The top panel of the figure displays the trend in violent crime index averaged across all 103 MSAs for the 1994–2004 period. The data tell a familiar story. For the metropolitan areas in the sample, the rates of violent crime exhibit a dramatic decline throughout the 1990s, with some leveling off thereafter.

The trend for the immigrant concentration index, which is displayed in the bottom panel of figure 1, is virtually an inverted image of that for violent crime rates. Consistent with the pattern widely reported for the nation at large, the relative size of the Latino/foreign-born population shows a sharp and sustained increase during the 1990s, which continues

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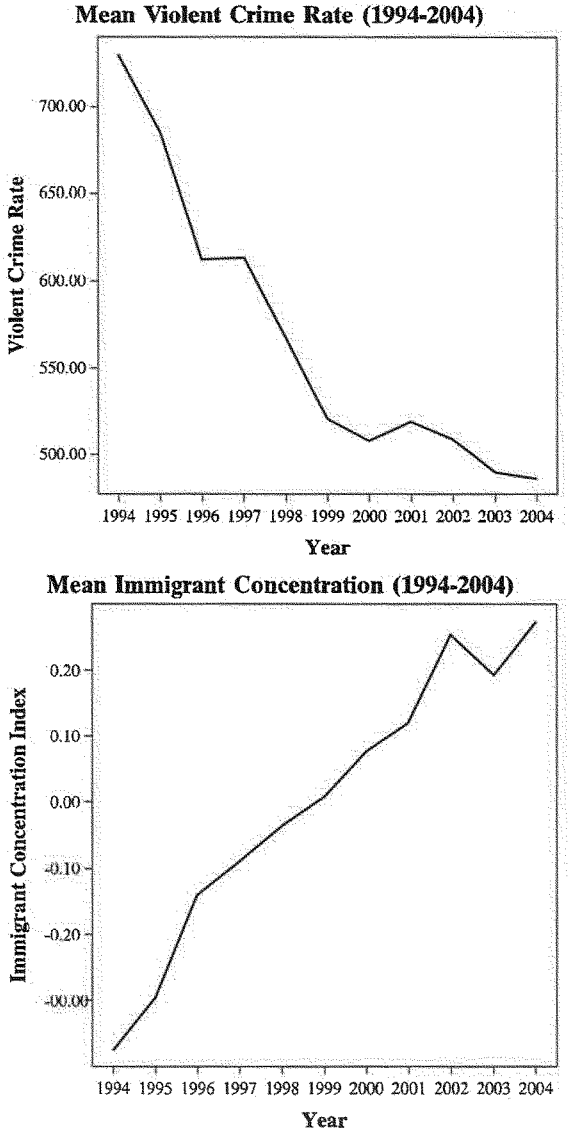
Figure 1. Longitudinal Trends in Violent Crime and Immigrant Concentration



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with a brief interruption into the twenty-first century. These simple graphs thus lend credibility to the hypothesis that the growth in immigration played a role in the recent crime decline in the United States.

Table 2 reports the results of the dynamic regression modeling. For each of our dependent variables, we report two equations. The first is a baseline model that includes only the impact of changes in immigrant concentration on changes in the violent crime rates. The second equation adds the control variables. The coefficients for the immigration variable are for the most part similar across models, and we thus focus our discussion on the results in the fully specified models.

The coefficients for the immigrant concentration index are always negative in sign, and they reach statistical significance for the violent crime index (–22.913, $p < .05$), the robbery rate (–9.223, $p < .001$), and aggravated assault (–13.253, $p < .05$). In other words, for these offenses, increases in the relative size of the Latino/foreign-born population are associated with significant decreases in criminal violence.

Although the immigration index yields statistically significant effects for the violent crime index, robbery rates, and rates of aggravated assault, our regression models indicate that the overall role of immigration for the crime decline is modest, accounting for just over 6 percent of the observed crime drop (6.16 percent). This number was derived by comparing the predicted values of the change scores for these offenses under two scenarios: 1) with the value of change in the immigration concentration index set at 0 (no change) and 2) with the value of change in immigration set at its mean pooled over the entire sample of MSAs/years. In these prediction models, all other variables have been set at their respective means. This effect is consistent across offense categories, where the changes in levels of violence also averaged about 6 percent.

The consistent impact of changing immigration, although modest with respect to the crime decline for a given year, is nevertheless nontrivial. Table 3 reports the predicted changes in the index rate of violent crime, the robbery rate, and the aggravated assault rate based on the equations in table 2 for differing levels of the immigration concentration index. As before, all other predictors are fixed at their means. The results show that those MSAs at the high end of the distribution for immigrant growth are estimated to have experienced a much greater drop in violent crime than those at the bottom end (contrast especially the predicted values at the tails of the distributions). For example, we observe that a change in immigration concentration at 2 standard deviations above the mean translates into an average of 40.5 fewer violent crimes (per 100,000), compared with a reduction of 8.1 violent crimes (per 100,000) in areas that experienced changes in immigrant concentration at 2 standard deviations below the mean. This pattern is consistent for both robbery and aggravated assault

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Table 2. Dynamic Regression of Violent Crime Rate and its Components (N = 1028)

	Change in Violent Crime Rate	Change in Violent Crime Rate	Change in Robbery Rate	Change in Robbery Rate	Change in Homicide Rate	Change in Homicide Rate
Change immigration concentration index	–20.01* (8.563)	–22.913* (8.871)	–8.218** (3.072)	–9.223** (3.176)	–.333# (.180)	–.296 (.186)
Change economic resources factor	—	–1.171 (6.774)	—	–2.997 (2.413)	—	.030 (.144
Change stability index	—	–2.164 (2.938)	—	–.165 (1.038)	—	.070 (.063)
Change percent non-Latino black	—	–.452 (.854)	—	.205 (.304)	—	.027 (.018)
Change percent young males	—	1.188 (2.080)	—	.150 (.740)	—	.013 (.044)
Change percent unemployed	—	2.729* (1.268)	—	1.437** (.444)	—	.071* (.028)
Change percent divorced	—	–.218 (1.590)	—	–.241 (.564)	—	.018 (.034)
Change adult/child ratio	—	–37.041 (113.53)	—	–56.552 (40.295)	—	–2.299 (2.422)
Change in population (ln)	—	–11.456 (35.053)		–12.386 (12.589)		–.848 (.732)
Change in percent of suicides by fircarms	—	–.225 (.449)	—	–.145 (.157)	—	–.012 (.010)
Change in officers per 1,000 population	—	5.216 (7.608)	—	2.925 (2.739)	—	.099 (.157)
South	—	–9.466 (6.733)	—	–2.216 (2.552)	—	–.180 (.129)
West	—	–11.337 (7.350)	—	–2.381 (2.786)	—	–.062 (.140)
Northeast	—	2.197 (7.505)	—	–1.616 (2.843)	—	.127 (.143)
Constant	–22.94** (2.509)	–17.037** (5.470)	–8.866** (.960)	–6.942** (2.067)	–.303*** (.048)	–.254* (.105)
AR(1)	–.233*** (.030)	–.248*** (.030)	–.146*** (.031)	–.170*** (.032)	–.396*** (.029)	–.412*** (.029)
–2 log likelihood	12290.3	12277.9	10179.0	10159.3	4404.1	4385.3

NOTES: Standard errors are in parentheses.
# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

rates, which declined sharpest in areas with the largest increases in immigration concentration (–15.9 and –23.3 events per 100,000, respectively).

Turning to the control variables (see table 2), only the unemployment measure exhibits a significant effect on changes in rates of criminal

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Table 2 (continued). Dynamic Regression of Violent Crime Rate and its Components (N = 1028)

	Change in Aggravated Assault Rate	Change in Aggravated Assault Rate	Change in Rape Rate	Change in Rape Rate
Change immigration concentration index	–11.640#	–13.253*	–.077	–.319
	(6.129)	(6.357)	(.610)	(.631)
Change economic resources factor	—	2.236	—	–.165
		(4.844)		(.481)
Change stability index	—	–2.253	—	.029
		(2.095)		(.208)
Change percent non-Latino black	—	–.744	—	.001
		(.611)		(.061)
Change percent young males	—	1.024	—	–.193
		(1.487)		(.148)
Change percent unemployed	—	.735	—	.219*
		(.901)		(.090)
Change percent divorced	—	.075	—	–.018
		(1.135)		(.113)
Change adult/child ratio	—	17.740	—	3.296
		(81.086)		(8.058)
Change in population	—	1.023	—	2.836
		(25.145)		(2.493)
Change in percent of suicides by firearms	—	–.016	—	–.032
		(.318)		(.032)
Change in officers per 1,000 population	—	1.120	—	.856
		(5.464)		(.542)
South	—	–6.492	—	–.504
		(4.924)		(.484)
West	—	–8.585	—	–.243
		(5.376)		(.528)
Northeast	—	3.204	—	.479
		(5.488)		(.539)
Constant	–13.190***	–9.330*	–.570**	–.549
	(1.824)	(3.996)	(.180)	(.393)
AR(1)	–.211***	–.218***	–.225***	–.233***
	(.030)	(.031)	(.030)	(.031)
–2 log likelihood	11,600.4	11,588.7	68,56.3	6,840.2

NOTES: Standard errors in parentheses.
# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

violence. The coefficient for this variable is consistently positive and is statistically significant in all models with the exception of that for aggravated assault. In other words, for our sample of metropolitan areas, decreasing unemployment is generally associated with falling violent crime rates. This finding runs counter to some previous assessments of the role of unemployment in the crime decline. In his review of the literature, Levitt (2004) concludes that the strong economy in the 1990s might have made a modest contribution to the drop in property crimes, but it had little effect on

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Table 3. Predicted Change in Violent Crime Rates at Differing Levels of Change in the Immigration Index[a]

Level of Change in the Immigration Concentration Index	Offense Type		
	Violent Crime Index	Robbery	Aggravated Assault
+2 SD	–40.5	–15.9	–23.3
+1 SD	–32.3	–12.3	–18.6
Mean	–24.2	–9.4	–13.9
–1 SD	–16.2	–6.2	–9.3
–2 SD	–8.1	–2.9	–4.6

[a] All other independent variables fixed at their means.

changes in violent crime.[15] Arvanites and DeFina (2006) arrived at a different conclusion about the importance of a strong economy for crime reduction, but they also detect no impact of unemployment rates per se. Using state-level data and a broader measure of economic activity than the conventional unemployment rate (i.e., the inflation adjusted gross state domestic product), they find that business cycle growth reduced robbery rates (and other index property crime rates). The unemployment rate, in contrast, exerts no significant effect on changes in crime rates in their analyses. Rosenfeld and Fornango (2007) similarly conclude that general economic conditions contributed to the crime drop but that the relevant conditions are not adequately captured by the unemployment rate. In their pooled, cross-sectional time-series analyses for major regions in the United States between 1970 and 2003, they find a significant effect of an indicator of consumer confidence on robbery rates (rising confidence accompanies decreases in robbery rates) but a negative effect of the unemployment rate, which they interpret with reference to "opportunity" dynamics (2007: 751).

The reasons for these discrepant results concerning the impact of changing levels of unemployment on changing crime rates across studies are unclear. They could reflect differing units of analysis (e.g., MSAs versus states and regions) and/or differences in model specification. In any event,

15. Conklin (2003: 172) assigns a greater role to economic factors, suggesting that "the improving employment situation [in the 1990s] reinforced the downward trend in crime rates, even if it did not cause the initial decline." Rosenfeld (2002: 30) similarly proposes that "there seems little doubt that the record drops in unemployment rates, including those for minority teenagers during the economic boom of the 1990s, contributed in some way to the crime decline over the same period."

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our findings for unemployment in conjunction with those for related economic indicators in recent studies suggest that continued inquiry into the impact of macroeconomic conditions on crime rates is amply justified (see Rosenfeld and Fornango, 2007: 765).

The analyses thus far have operationalized the relative size of the immigrant population by means of the immigration concentration index to be consistent with past research. The two components of the index—percent foreign born and percent Latino—are of course conceptually distinct. It is thus instructive to disaggregate the index and to reestimate the regressions to determine whether one or the other component seems to be "driving" the effects observed in table 2.

The results of the disaggregated analyses, reported in table 4, indicate that changes in both percent foreign born and percent Latino are significantly associated with changes in robbery rates, which is consistent with the findings for the composite index. The results for the violent crime index also suggest that each component contributes to the crime decline, although the measure of percent Latino only reaches a level of significance at the .10 level with a two-tailed test (.05 with a one-tailed test). The pattern for aggravated assault is different. For this offense, the coefficient for percent Latino falls below statistical significance, whereas the coefficient for percent foreign born is only significant at the .10 level (two-tailed test). Two conclusions are suggested by the disaggregated analyses. The use of the composite index tends to offer greater support for claims of a salutary effect of immigration on changing levels of criminal violence than the use of the components in isolation. In addition, the effects of immigration seem to be most robust for the violent offense of robbery.[16]

Finally, to illustrate the implications of the research design, we have conducted a standard cross-sectional analysis to compare the results with those of our dynamic model. For these purposes, we computed the average rate smoothed over a 3-year period centered on the 2000 decennial census year. Given the highly skewed nature of the violent crime rates expressed as levels rather than as difference scores, we transformed them for the cross-sectional analyses by applying natural logs. We then used point estimates of the predictor variables for the smoothed 1999–2001 period to predict the logged violent crime rates.

The results of the cross-sectional analyses are reported in table 5. Considering the control variables first, the picture is generally consistent with

16. In a separate analysis (available upon request), we also estimated models with random intercepts and/or slopes, and models with intercepts, slopes, AR(1) parameters, and error variances that differed among regions. In no case did these variable parameter models have significant random effects or significantly affect model log-likelihoods. The effects in table 2 do not differ among MSAs.

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Table 4. Dynamic Results with Disaggregated Immigrant Concentration Components ($N = 1,028$)

	Change in Violent Crime Rate	Change in Violent Crime Rate	Change in Robbery Rate	Change in Robbery Rate	Change in Homicide Rate	Change in Homicide Rate	Change in Aggravated Assault Rate	Change in Aggravated Assault Rate	Change in Rape Rate	Change in Rape Rate
Change percent foreign born	−2.850*	—	−1.040*	—	−.042	—	−1.654#	—	−.077	—
	(1.227)		(.440)		(.026)		(.879)		(.087)	
Change percent Latino	—	−1.844#	—	−.865*	—	−.018	—	−1.052	—	.020
		(1.064)		(.378)		(.023)		(.760)		(.075)
Change economic resources factor	.405	−.895	−2.312	−3.039	.048	.040	3.150	2.410	−.156	−.103
	(6.716)	(6.844)	(2.394)	(2.437)	(.143)	(.145)	(4.802)	(4.892)	(.477)	(.485)
Change stability index	−2.112	−1.750	−.109	−.035	.069	.076	−2.224	−2.017	.022	.040
	(2.941)	(2.935)	(1.040)	(1.037)	(.063)	(.063)	(2.096)	(2.092)	(.208)	(.208)
Change percent non-Latino black	−.275	−.326	.288	.237	.029	.030	−.641	−.667	<.000	.010
	(.846)	(.857)	(.301)	(.305)	(.018)	(.018)	(.604)	(.612)	(.060)	(.061)
Change percent young males	.929	1.449	.056	.270	.009	.016	.874	1.174	−.200	−.197
	(2.084)	(2.091)	(.742)	(.743)	(.044)	(.044)	(1.490)	(1.494)	(.148)	(.148)
Change percent unemployed	2.666*	2.557*	1.409**	1.373**	.071*	.069*	.694	.640	.221*	.212*
	(1.268)	(1.267)	(.444)	(.443)	(.028)	(.028)	(.900)	(.900)	(.090)	(.090)
Change percent divorced	−.182	−.027	−.215	−.158	.017	.022	.094	.212	−.022	−.012
	(1.591)	(1.588)	(.565)	(.563)	(.034)	(.034)	(1.136)	(1.133)	(.113)	(.113)
Change adult/child ratio	−25.885	−55.732	−52.825	−64.764	−2.120	−2.512	24.210	6.996	3.715	3.275
	(113.89)	(113.81)	(40.467)	(40.367)	(2.428)	(2.426)	(81.325)	(81.236)	(8.076)	(8.064)

NOTES: Standard errors are in parentheses.

# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

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Table 4. Dynamic Results with Disaggregated Immigrant Concentration Components (N = 1,028) (continued)

	Change in Violent Crime Rate	Change in Violent Crime Rate	Change in Robbery Rate	Change in Robbery Rate	Change in Homicide Rate	Change in Homicide Rate	Change in Aggravated Assault Rate	Change in Aggravated Assault Rate	Change in Rape Rate	Change in Rape Rate
Change in population	-10.681	-19.295	-12.403	-15.387	-.819	-.960	1.505	-3.521	2.980	2.697
	(35.155)	(34.944)	(12.635)	(12.552)	(.734)	(.728)	(25.215)	(25.052)	(2.499)	(2.481)
Change in percent of suicides by firearms	-.237	-.226	-.149	-.145	-.012	-.012	-.022	.017	-.032	-.032
	(.449)	(.449)	(.157)	(.157)	(.010)	(.010)	(.319)	(.319)	(.032)	(.032)
Change in percent of officers per 1,000 population	5.012	5.033	2.838	2.849	.096	.097	1.011	1.003	.857	.844
	(7.609)	(7.623)	(2.742)	(2.744)	(.157)	(.158)	(5.464)	(5.470)	(.541)	(.542)
South	-9.469	-9.919	-2.242	-2.387	-.179	-.186	-6.493	-6.757	-.496	-.514
	(6.727)	(6.747)	(2.548)	(2.562)	(.129)	(.129)	(4.922)	(4.928)	(.483)	(.484)
West	-11.600	-11.764	-2.520	-2.528	-.064	-.069	-8.737	-8.842	-.238	-.262
	(7.340)	(7.366)	(2.781)	(2.797)	(.140)	(.140)	(5.370)	(5.380)	(.528)	(.528)
Northeast	2.263	1.640	-1.614	-1.841	.129	.120	3.243	2.878	.489	.469
	(7.500)	(7.520)	(2.840)	(2.854)	(.144)	(.143)	(5.486)	(5.492)	(.539)	(.539)
Constant	-17.208**	-17.389**	-7.029**	-7.058**	-.256*	-.260*	-9.426*	-9.534*	-.544	-.563
	(5.463)	(5.481)	(2.064)	(2.075)	(.105)	(.105)	(3.992)	(3.999)	(.392)	(.393)
AR(1)	-.250***	-.246***	-.173***	-.166***	-.412***	-.413***	-.220***	-.218***	-.233***	-.232***
	(.030)	(.030)	(.032)	(.032)	(.029)	(.029)	(.031)	(.031)	(.031)	(.031)
-2 log likelihood	12,279.2	12,281.6	10,162.1	10,162.5	4,464.0	4,387.1	11,589.5	11,591.1	6,839.7	6,840.4

NOTES: Standard errors are in parentheses.

# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

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Table 5. Cross-Sectional Regression of Violent Crime Rate and its Components (N = 103)

	ln Violent Crime Rate		ln Robbery Rate		ln Homicide Rate		ln Aggravated Assault Rate		ln Rape Rate (n=102)	
Immigration concentration index	.033	(.038)	.065	(.041)	.040	(.039)	.034	(.044)	−.099*	(.044)
Economic resources factor	−.152*	(.059)	−.113#	(.063)	−.191**	(.061)	−.171*	(.068)	−.068	(.068)
Stability index	−.040	(.035)	−.014	(.037)	−.022	(.036)	−.035	(.040)	−.127**	(.040)
Percent non-Latino black	.015*	(.006)	.033***	(.006)	.033***	(.006)	.006	(.007)	−.005	(.007)
Percent young males	−.042	(.046)	−.032	(.048)	.007	(.047)	−.045	(.052)	−.042	(.052)
Percent unemployed	−.014	(.025)	−.011	(.027)	.004	(.026)	−.015	(.029)	−.031	(.029)
Percent divorced	.030	(.026)	.039	(.028)	.027	(.027)	.026	(.030)	.026	(.030)
Adult/child ratio	.190	(1.632)	1.833	(1.735)	3.028#	(1.680)	−.298	(1.879)	−2.930	(1.862)
ln population	.046	(.066)	.105	(.070)	.157*	(.068)	.041	(.076)	−.072	(.077)
Percent of suicides by fircarms	−.009	(.006)	−.007	(.006)	.009	(.006)	−.008	(.007)	−.009	(.007)
Officers per 1,000 population	.088	(.055)	.141*	(.059)	.117*	(.057)	.069	(.064)	.081	(.064)
South	.338*0	(.152)	−.097	(.161)	−.146	(.156)	.592**	(.175)	.303#	(.173)
West	.301*	(.149)	.219	(.158)	.189	(.153)	.395*	(.172)	.199	(.171)
Northeast	−.250	(.158)	−.349*	(.168)	−.320#	(.163)	−.151	(.182)	−.403	(.185)
Constant	5.225***	(1.619)	1.591	(1.721)	−4.302*	(1.667)	5.199**	(1.865)	6.666**	(1.948)
R^2	.500		.517		.619		.475		.388	

NOTES: Standard errors are in parentheses.
# $p < .10$ (two-tailed test); * $p < .05$; ** $p < .01$; *** $p < .001$.

theoretical expectations and past research. MSAs with high levels of economic resources tend to have relatively low violent crimes rates, whereas areas with large non-Latino black populations and those located in the South and West have relatively high violent crime rates.

The key findings in table 5 are those for the immigration concentration index. The coefficients for this variable are statistically nonsignificant and positive in all case but one—the rape rate. Our results are consistent with the bulk of previous research in that there is little support for the claim that immigration is positively associated with levels of most forms of criminal violence for the year 2000 (see Martinez, 2002, 2006; Sampson, 2006). Our dynamic models add continued support for this conclusion, as *increases* in the relative size of the Latino/foreign-born population during a period of exceptionally high immigration are significantly associated with *decreases* in the violent crime index, robbery rates, and aggravated assault rates.

SUMMARY AND CONCLUSIONS

The largely unexpected, yet precipitous, reductions in violent crime rates the United States has experienced over the past two decades have received much scholarly and popular attention. Despite the increased attention to this topic, quantifying the factors most responsible for the observed crime drop has proven challenging. Over this same period, the size of the foreign-born population in this country has grown exponentially, prompting some to hypothesize that the two trends are more than merely coincidental. Specifically, Sampson and colleagues have argued that immigration represents a credible, although counterintuitive, social process that has contributed to the reductions in crime (Sampson, 2006; Sampson and Bean, 2006). The objective of this research has been to assess whether there was a meaningful temporal association between immigration and crime. More specifically, we have examined the degree to which *changes* in immigration were associated with *changes* in levels of criminal violence across metropolitan areas.

The finding of primary interest in this study is that of an inverse relationship between changes in immigration and changes in the violent crime index, the robbery rate, and the aggravated assault rate, net of other demographic and social structural controls. This effect is particularly pronounced and robust for the offense of robbery. Although the contribution of immigration to the overall crime drop for these offenses is modest, metropolitan areas exhibiting high levels of immigration are estimated to have fared much better than those with comparatively low growth in the size of the immigrant population. Considered as a whole, our results support the hypothesis of Sampson and colleagues that the broad reductions in crime over recent years are partially attributable to increases in diversity and immigration (see Sampson, 2008; Sampson and Bean, 2006).[17]

17. With a nonexperimental design, it is possible that unmeasured exogenous factors may account for both the increase in immigration and the reduction in violence. For example, improvements in the economy are a likely factor that may attract immigrants (because of enhanced employment prospects) and partially mitigate conditions conducive to violent crime. In such a scenario, then, the relationship between immigration and violence is an artifact of broader social processes. We do not perceive this as a viable explanation for our findings. The immigration effect in our models is net of unemployment, which is significantly related to changing criminal violence in the expected positive direction. We also control for changes in a wide range of socioeconomic characteristics, including poverty and mean income, in our deprivation factor. We thus concur with Sampson (2008: 31), who argues that "economics can't easily explain the story . . . [economic factors such as] real wages were declining and inequality increasing throughout the 1990s by most accounts, which should have produced increases in crime." Similarly, Sampson also notes that rates of immigration have remained high early this decade even after the sharp declines in the stock market.

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More generally, we concur with Sampson and Bean's (2006: 21) argument that "in today's world it is no longer tenable to assume that immigration and diversity automatically lead to social disorganization and consequently crime." Yet we recognize that this position is at odds with both popular perceptions and traditional theoretical accounts of the relationship between immigration and crime. Consistent with the findings presented in previous research, the results from this study illustrate the inability of social disorganization theory, at least in its classical formulation, to explain the immigration/crime relationship in a contemporary urban context (see Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Stowell, 2007).

Given that the current wave of immigration is unlike those in past eras, in terms of both its volume and its racial/ethnic diversity, perhaps this limitation is not entirely unexpected. On their own, the results from this study may suggest that the inverse impact of immigration may be driven by the unique characteristics of the foreign-born population currently entering the United States. That is to say, the streams of immigrants entering today are somehow different (i.e., less criminogenic) than those of past eras. We do not believe that the weight of the evidence supports such a conclusion. Rather, we agree with Hagan and Palloni (1999: 617) who argue that the notion of the immigrant/crime link continues to be an "area of popular misperception and policy distortion," as it has been since the early 1900s (see also Martinez, 2002; Rosales, 1999). Taking a wider perspective, it seems that the results presented in the current study are consistent with many of its predecessors in finding that the crime-producing effects of immigration are overstated.

One strength of the current study, we believe, is that it is one of the first to examine the dynamic influence of immigration on violent crime in the 1990s. As such, our findings bear more directly on claims about causal processes, which to date have remained largely speculative. The relationship between immigration and crime is complex, however, and this study also leaves unanswered several important questions pertaining to some intricacies of this association. In the following discussion, we identify limitations of the current study and recommend topics to be pursued in future research on this subject.

To a large extent, by incorporating a longitudinal analytical design and a national sample, this study extends current knowledge on "whether" immigration contributed to reductions in violent crime, but it does not provide much insight into "how" this process operates. The mechanisms through which immigration may buffer against levels of violence can be explained in many ways. Perhaps the most straightforward account entails a purely compositional effect; this effect is highly consistent with the immigrant selectivity thesis. For reasons enumerated above, immigrants may be

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less prone to criminal violence than the native born. Violent crime rates should accordingly fall as the relative size of the immigrant population grows.

Another possibility, however, is that immigration alters social conditions in a manner that reduces levels of offending among immigrants and the native born alike. Most notably, it may be that immigrant communities are characterized by higher levels of collective efficacy, which in turn results in lower levels of crime (Sampson and Raudenbush, 1999; Sampson, Raudenbush, and Earls, 1997). Cross-sectional studies that have made use of the PHDCN data collected in Chicago seem to support this notion of a contextual effect (see Morenoff and Astor, 2006; Sampson and Bean, 2006). Current knowledge on this topic will be enhanced by research that examines the impact of immigration on levels of collective efficacy over time.

Another potential contextual mechanism through which immigration may lead to lower levels of crime is by influencing structural changes in the areas into which immigrants settle. In other words, it may be that immigration may promote positive changes in the "general character" of their communities, which would result in lower levels of crime (see Bursik, 2006: 26). This point is consistent with the "Immigration Revitalization Perspective" described by Martinez and colleagues, which argues that "immigration may encourage new forms of social organization that mediate disorganizing influences in inner city communities" (Lee, 2003: 131; see also Martinez, 2006). Following the logic of this perspective, it would be expected that immigration would be inversely related to the macrolevel conditions known to be associated with violence (i.e., poverty and unemployment).

We note that an assessment of the respective compositional and contextual contributions made by immigration to the reduction in criminal violence will require different data and a different design from that used in the current study. A standard technique for demonstrating contextual effects is to disaggregate the data according to relevant characteristics of the population. Thus, in the current case, differentiating between the compositional and contextual explanations for the observed association between immigration and violent crime would require data on offending specific to immigrant status (immigrants versus the native born). Although collecting the necessary data for such an assessment for relatively large and representative samples is a formidable task, analyses based on such data would shed considerable light on the mechanisms linking immigration and criminal violence.

The noted shortcomings notwithstanding, our study represents a formal test of the somewhat controversial hypothesis that growing immigration contributed meaningfully to the drop in violent crime. Although the

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impact of immigration on receiving societies has been of interest to criminologists and social scientists dating back more than a century, the arrival of newcomers continues to be perceived in a negative light. Despite the strong rhetoric, few researchers have rigorously tested the dynamic causal connection between immigration and crime. Our study shows that the contemporary surge in immigration has been associated with the heralded decline in selected forms of criminal violence. Indeed, although it is still not counted among the "usual suspects" used to account for the crime drop, the current study underscores the fact that immigration is a viable explanatory factor (Sampson and Bean, 2006: 21). It is our hope that these results can be used to inform both public policy discussions and academic research initiatives in the years ahead.

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Jacob I. Stowell is an assistant professor in the Department of Criminal Justice and Criminology at the University of Massachusetts–Lowell. His research interests include immigration, immigrant mortality, and neighborhood effects. His current research focuses on studying the spatial and temporal impacts of immigration on levels of violent criminal behavior. Recent articles have been published in the *American Journal of Sociology*, the *ANNALS of the Academy of Political and Social Science*, and *Social Science Quarterly*.

Steven F. Messner is the Distinguished Teaching Professor of Sociology, University at Albany, State University of New York. His research focuses on social institutions and crime, understanding spatial distributions and trends in crime, as well as crime and social control in China. In addition to his publications in professional journals, he is coauthor of *Crime and the American Dream* and *Perspectives on Crime and Deviance,* and he is coeditor of *Theoretical Integration in the Study of Deviance and Crime* and *Crime and Social Control in a Changing China.*

Kelly F. McGeever is a PhD candidate in the Department of Sociology at the University at Albany, State University of New York. Her research interests include communities and crime, Global Information Systems applications and the spatial analysis of crime, and sentencing outcomes. Her dissertation tests "not in my backyard" (NIMBY) arguments about

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the negative effects of group quarters, such as halfway houses and correctional facilities, on neighborhood quality. Her most recent publication appeared in the *Journal of Quantitative Criminology*.

Lawrence E. Raffalovich is an associate professor in the Department of Sociology at the University at Albany, State University of New York. His research interests include social inequality, economic sociology, and quantitative research methods. Recent publications include (with Glenn D. Deane, David Armstrong, and Hui-shien Tsao) "Model selection procedures in social research: Monte-Carlo simulation results" in the *Journal of Applied Statistics* 35:1093–114 and (with Elena Vesselinov) "The power of property in comparative perspective" in *Research in Social Stratification and Mobility* 20:361–84.

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Appendix A. Metropolitan Areas

Akron, OH
Albany-Schenectady-Troy, NY
Albuquerque, NM
Allentown-Bethlehem-Easton, PA
Ann Arbor, MI
Atlanta, GA
Austin-San Marcos, TX
Bakersfield, CA
Baltimore, MD
Baton Rouge, LA
Bergen-Passaic, NJ
Birmingham, AL
Boston, MA-NH
Buffalo-Niagara Falls, NY
Charleston-North Charleston, SC
Charlotte-Gastonia-Rock Hill, NC-SC
Chicago, IL
Cincinnati, OH-KY-IN
Cleveland-Lorain-Elyria, OH
Colorado Springs, CO
Columbia, SC
Columbus, OH
Dallas, TX
Dayton-Springfield, OH
Denver, CO
Detroit, MI
El Paso, TX
Fort Lauderdale, FL
Fort Wayne, IN
Fort Worth-Arlington, TX
Fresno, CA
Gary, IN
Grand Rapids-Muskegon-Holland, MI
Greensboro—Winston-Salem—High Point, NC
Greenville-Spartanburg-Anderson, SC
Harrisburg-Lebanon-Carlisle, PA
Hartford, CT
Honolulu, HI
Houston, TX
Indianapolis, IN
Jacksonville, FL
Jersey City, NJ
Kansas City, MO-KS
Knoxville, TN
Las Vegas, NV-AZ
Little Rock-North Little Rock, AR
Los Angeles-Long Beach, CA
Louisville, KY-IN
Tucson, AZ
Tulsa, OK
Vallejo-Fairfield-Napa, CA
Ventura, CA
Washington, DC-MD-VA-WV
McAllen-Edinburg-Mission, TX
Memphis, TN-AR-MS
Miami, FL
Middlesex-Somerset-Hunterdon, NJ
Milwaukee-Waukesha, WI
Minneapolis-St. Paul, MN-WI
Mobile, AL
Monmouth-Ocean, NJ
Nashville, TN
Nassau-Suffolk, NY
New Haven-Meriden, CT
New Orleans, LA
New York, NY
Newark, NJ
Norfolk-Virginia Beach-Newport News, VA-NC
Oakland, CA
Oklahoma City, OK
Omaha, NE-IA
Orange County, CA
Orlando, FL
Philadelphia, PA-NJ
Phoenix-Mesa, AZ
Pittsburgh, PA
Portland-Vancouver, OR-WA
Providence-Fall River-Warwick, RI-MA
Raleigh-Durham-Chapel Hill, NC
Richmond-Petersburg, VA
Riverside-San Bernardino, CA
Rochester, NY
Sacramento, CA
St. Louis, MO-IL
Salt Lake City-Ogden, UT
San Antonio, TX
San Diego, CA
San Francisco, CA
San Jose, CA
Sarasota-Bradenton, FL
Scranton—Wilkes-Barre—Hazleton, PA
Seattle-Bellevue-Everett, WA
Springfield, MA
Stockton-Lodi, CA
Syracuse, NY
Tacoma, WA
Tampa-St. Petersburg-Clearwater, FL
Toledo, OH
West Palm Beach-Boca Raton, FL
Wichita, KS
Wilmington-Newark, DE-MD
Worcester, MA-CT
Youngstown-Warren, OH

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Appendix B. Correlation of Decennial Census Data and CPS Data for 103 Metropolitan Areas, 2000

	Correlation
Total Population	.997***
Racial/Ethnic Composition	
% Non-Latino white	.956***
% Non-Latino black	.933***
% Latino	.976***
% Asian	.971***
Nativity Composition	
% Foreign born	.950***
% Recent immigrant	.895***

*$p < .05$; **$p < .01$; ***$p < .001$.

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Appendix C. Descriptive Statistics (N = 1,131)[a]

	Mean	Standard Deviation	% Change 1994–2004
Dependent Variables (per 100,000 Population)			
Violent crime rate	567.61	266.86	–33.37
Robbery rate	183.24	104.16	–38.32
Homicide rate	6.55	4.35	–37.49
Aggravated assault rate	348.71	183.73	–31.74
Rape rate	29.12	12.74	–17.68
Independent Variables[b]			
Percent first-generation immigrant	11.38	9.92	52.16
Percent Latino	13.06	16.27	27.13
Percent in poverty	12.11	5.86	–19.67
Percent with less than a college degree	72.88	5.16	–7.79
Household income	$53,487.99	$11,671.85	51.86
Percent female-headed household	15.20	4.77	–10.26
Percent home ownership	65.82	9.44	10.21
Percent moved in the last 5 years	16.17	5.16	–19.70
Population	1,648,900	1,609,390	21.07
Percent non-Latino black	13.14	10.16	–7.15
Percent young males	4.85	1.37	–.10
Percent unemployed	5.46	2.66	–16.05
Percent divorced	9.56	2.40	5.32
Adult/child ratio	.74	.04	1.48
Percent guns used in suicide	51.61	13.03	–16.53
Number of officers per 1,000 population	2.30	.93	29.09
Constant (N = 103)	**Frequency**		
Northeast	22		
South	36		
Midwest	21		
West	24		

NOTES: Please see text for detailed information about index construction.

[a] Because of the use of first differences, the N for the regression analyses is 1,028.

[b] The first eight independent variables listed are the components used in three indices.

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COMMENTARY

Immigration and Mental Health

Why Are Immigrants Better Off?

SINCE 1970, more than 5 million Mexicans have immigrated to the United States, the largest number of immigrants from one country to the United States in recent history. People of Mexican descent now make up almost one third of the total population of Texas and California.[1]

See also page 771

The study by Vega et al reported in this issue of the ARCHIVES is an important and timely piece, coming when Mexican immigrants in California are the target of adverse societal attitudes and governmental policies affecting benefits, university access, and bilingualism. Paradoxically, the study shows that despite socioeconomic disadvantages, Mexican immigrants have a much better mental health profile than people of Mexican descent born in the United States. Carried out on a specific population in northern California, the study was elegantly designed and employed state-of-the-art methods and instrumental measures that were further refined during the study. The study's major finding, that place of birth had a more profound influence on the prevalence of psychiatric disorders than traditional demographic risk factors such as age, sex, or socioeconomic status, is intriguing. According to these data, for virtually each disorder studied, Mexican immigrants had about half the prevalence rate of people of Mexican descent born in the United States. The study illustrates the importance of cross-cultural research in advancing our knowledge of risk and protective factors for mental disorders and reminds us that the study of the mental disorders of specific ethnic groups can generate hypotheses that apply to general psychiatry. Also, the relevance of international collaboration in psychiatric research is shown by the successful use of the Mexico City sample to help interpret the results of the study.

The fact that Mexican immigrants had better mental health profiles than people of Mexican descent born in the United States in the study by Vega et al challenges long-standing tenets in psychiatry and psychology such as the damaging psychological effects of migration and the positive psychological effects of acculturation, or "Americanization," and may appear contrary to conventional wisdom. Historically, the experiences of "emigration, immigration and Americanization" have had a significant effect on the field of psychiatry, shaping key concepts such as Erickson's "identity" and "identity crisis."[2] The early studies highlighted the stresses of migration and the disadvantages faced by the immigrants, contrasting these with the positive effects of assimilation into mainstream society of the United States.[3-5] Some of these findings may reflect biases inherent to studies that were done long before diagnostic criteria and structured instruments became available.

The study by Vega et al nicely complements and updates results of the Epidemiologic Catchment Area study that took place in Los Angeles in the 1980s.[6] Although immigration was not the main focus of our studies then, we were surprised to observe that the prevalence of most disorders examined was significantly lower in Mexican immigrants than in people of Mexican descent born in the United States. For example, the prevalence of alcohol and other drug problems in the US-born subjects was more than 3 times that found in the subjects born in Mexico.[7] Given the harsh realities faced at every step by immigrants from Mexico to the United States, we argued then that the unexpected findings might be explained by a bias caused by an immigration of only the strongest, most resilient people from Mexico to the United States, a "migration of the fittest."[7]

In the 1990s, the National Comorbidity Survey[8] included a sample that represented the national Hispanic population, but found no major differences between Hispanics and the general population. The rates of mental disorders for both Hispanics and non-Hispanics found in that study are significantly higher than the rates for the Mexican immigrants reported in the study by Vega et al. However, the diversity of the sample, its lack of regional focus, and inclusion of English speakers only limit the scope of the National Comorbidity Survey data on Mexican Americans.

In another study, Swanson et al[9] used a self-assessment questionnaire, which included a depression scale and specific queries on the usage of alcohol and other drugs, in classrooms to gather information on depression and drug use from a large sample of young people (aged 11-19 years) from 6 towns on both sides of the Texas/Mexico border. The results of that study showed that despite less favorable socioeconomic conditions, adolescents living on the

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Mexican side of the border had a significantly lower prevalence of depression, thoughts of suicide, and drug abuse than their counterparts in the United States. For example, the prevalence of drug use was 4 times higher on the United States side of the border than on the Mexican side (21% vs 5%).

Recently, our group completed a study[10] of 1500 users of primary care services in California, about half of them foreign-born (Mexicans and Central Americans), with the Composite International Diagnostic Interview and the RAND-Medical Outcomes Study 36-Item Short Form Health Survey (SF-36) as a functional measure. We found that compared with patients born in the United States, immigrants had a significantly lower prevalence of depression and posttraumatic stress disorder and better physical functioning scores despite their lower socioeconomic status.

Mexican immigrants have advantages other than better mental health over people of Mexican descent born in the United States, according to the results of several studies on the outcomes of pregnancy in various ethnic groups. For example, children born to Mexican immigrant mothers have lower mortality rates and higher birthweight rates than children born to US-born mothers of Mexican descent,[11,12] again a paradox, since Mexican immigrants are poorer, less educated, and have less access to health care. Other important findings include the observation that the nutrient intake of Mexican immigrant women is significantly better than that of US-born women of Mexican descent,[13] and that Mexican immigrant women are significantly less likely than US-born women of Mexican descent to test positive for drugs at the time of delivery.[14]

Thus, there is rising evidence in support of a negative effect of "acculturation" on the mental health of Mexican Americans, and a positive effect of the retention of Mexican cultural traditions. The results are from large, well-designed studies of general populations and primary care samples that included careful translation and adaptation of instruments, and therefore they seem credible. While the possibility remains that some of these findings may be due to "measurement error" (eg, misinterpretation of questions, language and translation problems, and lack of cross-cultural equivalence) or "response bias" (related to issues of social desirability, social approval, and acquiescence), I strongly suspect that the real reason for the advantages that Mexican immigrants have may be due to a "protective" or "buffering" effect of traditional culture. Traditionally, Hispanic families have been described as close-knit, extended family networks that offer a great deal of support.[15] Also, compared with whites, Mexican Americans have a higher proportion of 2-parent families and lower rates of divorce and separation.[16] Retention of cultural traditions may also contribute to healthier habits (eg, better eating and less drug use) that may lead to better health and mental health outcomes.

Finally, the most salient difference between immigrants and natives in these studies is in the area of substance abuse, particularly with drugs other than alcohol. Drug abuse is an average of 4 times higher among people of Mexican descent born in the United States than among Mexican immigrants. This may be due to the greater availability, easier access, and relative acceptability of recreational drug use in the United States, and may in part explain the higher rates also seen for mood and anxiety disorders among people of Mexican descent born in the United States. Clearly, more research is needed to pursue many of these intriguing possibilities.

Javier I. Escobar, MD
Department of Psychiatry
University of Medicine and Dentistry of New Jersey
Robert Wood Johnson Medical School
Piscataway, NJ 08854-5635

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Sociological Criminology and the Mythology of Hispanic Immigration and Crime

Author(s): John Hagan and Alberto Palloni

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Sociological Criminology and the Mythology of Hispanic Immigration and Crime*

JOHN HAGAN, *Northwestern University and American Bar Foundation*

ALBERTO PALLONI, *University of Wisconsin*

Our sociological knowledge of crime is fragmented and ineffective in challenging and correcting mistaken public perceptions, for example, linking immigration and crime. These misperceptions are perpetuated by government reports of growing numbers of Hispanic immigrants in U.S. prisons. However, Hispanic immigrants are disproportionately young males who regardless of citizenship are at greater risk of criminal involvement. They are also more vulnerable to restrictive treatment in the criminal justice system, especially at the pre-trial stage. When these differences are integrated into calculations using equations that begin with observed numbers of immigrants and citizens in state prisons, it is estimated that the involvement of Hispanic immigrants in crime is less than that of citizens. These results cast doubt on the hypothesis that immigration causes crime and make more transparent the immigration and criminal justice policies that inflate the rate of Hispanic incarceration. This transparency helps to resolve a paradox in the picture of Mexican immigration to the United States, since by most measures of well-being, Mexican immigrants are found to do as well and sometimes better than citizens.

"Criminology," Sutherland and Cressey (1978:3) write in the opening paragraph of their classic text, ". . . includes within its scope the processes of making laws, of breaking laws, and of reacting toward the breaking of laws." For Sutherland and Cressey, these processes are "three aspects of a somewhat unified sequence of interactions" and "this sequence of interactions is the subject matter of criminology." This formulation has proven partly prophetic in that sociological criminology continues to be subdivided into studies of law making and breaking and reacting to law breakers, but the *unification* that Sutherland and Cressey sought in these sequentially related subfields is not reflected in the integration of criminological research. For example, there is no systematic connection between studies of the causes of criminal *behavior* and the *punishment* of this behavior. A result is that our knowledge of crime is fragmented and ineffective in challenging and correcting mistaken beliefs. It is still the case, as Sutherland and Cressey (4) lamented, that "The average citizen is confronted by a confusing and conflicting complex of popular beliefs and programs with regard to crime."

Sutherland highlighted immigration and crime as an area of popular misperception and policy distortion in early versions of his text (1924, 1934), and these problems persist. For example, it currently is the case that immigration and criminal justice policies which appear neutral in relation to Hispanic immigrants actually bias and distort public perceptions of immigration and crime by inflating Hispanic rates of imprisonment. The challenge is to make these unrecognized and therefore unforeseen consequences of policies and practices visible to the public and to policymakers alike. We can advance this goal by heeding Sutherland and Cressey's advice to combine our consideration of the causes of criminal behavior *and* its punishment—

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punishment which in this case results in high levels of Hispanic imprisonment. In doing so we can explain prison statistics that link immigration and crime, and thereby demonstrate the relevance and potential of a more coherent and better integrated sociological criminology.

Immigration and Crime Through Time

Sutherland was critical of both data and writings that, early in this century, purported to show higher rates of crime among foreign-born immigrants than among the native-born (see Immigration Commission 1911; Industrial Commission 1901). Sutherland argued that acculturation into American society rather than immigration was causally associated with crime. In support of this position he reported evidence that second-generation immigrants had higher rates of crime than first-generation immigrants, that immigrants who came to America as children were imprisoned at higher rates than immigrants who came as adults, and that immigrants to America had higher rates of serious crime than their counterparts in their native countries (see also National Commission on Law Observance and Enforcement 1931). In each of these instances, Sutherland's argument was that rather than immigration causing increases in American crime, exposure to the forces of acculturation more slowly caused immigrants, and more notably their children, to become more like native-born citizens in their criminal involvements. He further worried that official crime statistics that were unadjusted for age and sex, and also prone to police and court error and bias, were dubious resources for reaching conclusions about immigration and crime.

Nonetheless, public beliefs causally connecting immigration with crime were widespread and they coincided with historically high rates of immigration through the first decades of this century. These concerns about crime ultimately helped to justify restrictive immigration policies. Public perceptions of immigrant alcohol use and public drunkenness in association with fears of crime facilitated the passage of Prohibition, and Congressional acts in 1921 and 1924 substantially reduced the numbers of immigrants admitted to the United States. Concerns about immigration focused on Southern and Eastern Europeans during this period, although statistical analyses more generally compared native-born whites with foreign-born Americans.

Over time, American criminologists adopted a somewhat more sophisticated, reformulated version of what Michael Tonry (1997:20) has called the "not the foreign born but their children" view of immigration and crime. This reformulation was built on early American research which concluded that while immigrant parents were hard-working and conformist in their dedication to the achievement of longer-term rewards, their children were "caught between two worlds" and more likely to turn to crime in response to their unrealized and perhaps unrealizable goals for rapid advancement. This reformulated, multigenerational model was an improvement over the earlier "immigration causes crime" hypothesis, but Tonry's review of more recent research demonstrates that this model is also simplistic and only partly true, even for the self-selected U.S. economic migrants whose experience it accurately describes. For example, Tonry convincingly argues that differences in immigrant cultural backgrounds and reasons for migration, as well as structural circumstances that contextualize migration and settlement experiences, powerfully condition relationships between immigration and crime. A result is that many but not all disadvantaged minority youth, including some but not all children of disadvantaged immigrants, disproportionately fall into the clutches of the criminal justice system.

Still, recurring political pressures threaten to bring the simpler, more sweeping stereotype that immigration causes crime back into prominence. During the 1996 U.S. Presidential election, a *Wall Street Journal* story (Seib 1996) led with the headline, "Backlash Over Immigration Has Entered Mainstream This Year," and noted that the public now felt immigration was tied to "a fraying of the social order" involving problems of crime and drugs. A survey of such stories in major newspapers across the United States led Tanton and Lutton (1993) to conclude

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that "under current immigration laws and procedures, frighteningly large numbers of newcomers see crime as their avenue to the American Dream" (see also O'Kane 1992). It is unclear how this analysis identified what the "newcomers saw," but further evidence of what the public believes about immigrants and crime appeared in a report for the U.S. Commission on Immigration Reform. In this report, Bean et al. (1994:3) confirmed that: ". . . many people believe that undocumented aliens are the source of the increase in serious crime . . . and that the increasing number of undocumented aliens is due to the U.S. Government's inability to control the border." Citing the rising costs of crime, drugs and other problems allegedly associated with an explosion in the number of these new immigrants, Patrick Buchanan promised as part of his 1996 Presidential campaign that "I will stop illegal immigration cold by putting a double-linked security fence along the 200 miles of the border where millions pour in every year" (cited in Dillon 1997).

In 1996 Congress passed the Illegal Immigration Reform and Immigrant Responsibility Act and increased the budget of the Immigration and Naturalization Service (INS). Much of the latter funding was dedicated to enforcement, giving the INS a bigger budget than the Federal Bureau of Investigation and a larger work force than the Departments of State or Labor. Since then, political rhetoric surrounding immigration has softened, but the harshness and size of the law enforcement apparatus remains, as do the underlying perceptions and stereotypes on which U.S. immigration policy often is based (Ojito 1998).

This paper argues that mistaken perceptions linking Mexican immigration and criminal behavior are perpetuated by government reports of growing numbers of Hispanic immigrants in U.S. prisons (Scalia 1996; Wunder 1995). Reports of Hispanic imprisonment obscure policies and practices that inflate Hispanic incarceration. That is, high Hispanic incarceration rates are the product of specific immigration and criminal justice policies and practices. Sociological criminology, Sutherland would argue, has an obligation to show how this is so, that is, to demonstrate how the "sequence of interactions" resulting from immigration and criminal justice policies and practices yield Hispanic incarceration rates that misinform our understanding of immigration and crime. We first will show how these unadjusted rates misinform our impressions of immigration and crime; then we will show how these rates can be recomputed to correct misconceptions and make the results of the policy making process more visible.

The first step in making incarceration rates for immigrants more meaningful involves adjusting for age. The desirability of adjusting for age may seem obvious, but the necessity of doing so is slightly more obscure. American immigration law and its implementation is at least in part a reflection of the demand for low wage, unskilled labor (Calavita 1994). This labor is disproportionately done by young men, and young men are also more prone to involvement in crime. Immigration policy that allows relatively free movement of young Hispanic men into the United States is therefore not neutral with regard to crime, and the extent to which the demand for inexpensive young male labor structures the pool of immigrants can be made apparent in incarceration rates by adjusting for sex and age.

The second step in making incarceration rates for immigrants more meaningful involves adjusting for pretrial detention policy and practices which are also less neutral than they might seem. The problem is that pretrial detention decisions operate to the systematic disadvantage of members of immigrant groups. "This is a perplexing problem," Tonry (1997:16) observes, "because practices that are justifiable in their own terms, such as detaining those people who seem least likely to remain in the jurisdiction and appear for trial, produce outcomes, greater proportions of minority offenders in confinement, that are widely seen as regrettable." Being detained before trial increases the probability that a prison sentence will be imposed after trial (Hood 1992; Petersilia and Turner 1986). So pretrial detention is a second source of bias that should be considered in the adjustment of incarceration rates.

Tonry (1995) notes that to *consciously* incarcerate minority and immigrant offenders disproportionately for reasons such as being young and male, or having been detained before trial, would be seen as wrong in all Western countries. "To maintain policies which foresee-

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ably have that effect," Tonry (1997:17) further explains, "is not exactly the same thing but deserves more hard ethical scrutiny than typically it receives." Making the effects of immigration and criminal justice policies and practices on incarceration rates visible is an important means of advancing this goal of ethical scrutiny.

Immigration and Imprisonment in America

On its face, there might seem good reason to link issues of immigration and crime in late twentieth-century America. Between 1960 and 1990 annual immigration to the U.S. and the U.S. homicide rate both nearly doubled (from 1.7 to 3.0 per thousand and from 4.8 to 8.3 per hundred thousand population respectively). Rates of imprisonment also sky-rocketed during this period, with federal prison populations more than doubling between 1980 and 1990, largely due to increased drug arrests. Although extended time series are not available, it is clear that the number of immigrants in U.S. federal, state and local prisons also increased during this period (Scalia 1996). However, these parallel trends are easily misinterpreted.

First, the upturn in homicides and imprisonments beginning in the 1960s had much more to do with the growing numbers of young native, white and black Americans in the crime-prone years of late adolescence and early adulthood, than it did with immigration. Second, the growing proportions of noncitizens in prison populations, especially Hispanic and Mexican immigrants, had much more to do with the increased entrance into the United States of disproportionately young immigrants and their vulnerability to pre-trial detention, conviction, and imprisonment, than with the overrepresentation of crime among immigrants. None of this is apparent from prison statistics alone, which, as we explain below, have become the primary lens through which growing concerns about immigration and crime are filtered in the 1990s.

Two pronounced waves of immigration in the beginning and closing years of this century elevated the American public's concerns about immigration and crime. In between, from the mid-1930s to the mid-1960s, concern about immigration and crime declined and nearly disappeared. A reflection of this is that Sutherland and Cressey eliminated completely the lengthy discussion of immigration and crime that had been an important part of earlier editions of their classic text in the eleventh edition. With the exception of occasional panics about Italian American involvement in organized crime (Cressey 1969), fears about immigration and crime gradually faded into the background of public and criminological concern. Earlier waves of European immigrants assimilated into American society and became citizens, while immigration and crime both declined through the 1950s, and public fears subsided.

As public pressure and concern diminished, police and court officials began to acknowledge that they could not obtain good information on the immigration status of arrestees, and they stopped collecting and reporting this information. Although today efforts increasingly are made to reinvolve earlier phases and lower levels of the criminal justice system in tracking illegal immigrants, it remains the case that the largest number of immigrants are identified at imprisonment, when greater amounts of time and effort can be invested in their detection (McDonald 1997). Especially since the Violent Crime Control and Law Enforcement Act of 1994 released unprecedented funds to reimburse states and localities for the costs of incarcerating illegal aliens, correctional institutions have developed an acute interest in the identification of noncitizens. In December, 1996, the Federal government released $495 million, with half this sum going to California and almost $13 million set aside for Los Angeles County jails alone (Lacey 1996). Correctional systems therefore have developed a strong financial interest in determining inmates' immigration status. Several reports released in the 1990s indicate that the numbers of legal and illegal immigrants incarcerated in U.S. prisons have reached record levels that exceed the representation of immigrants in the U.S. population (McDonald 1997; Scalia 1996; Wunder 1995).

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Problems with Prison Statistics

Prison statistics on immigration and crime cannot be taken at face value. There are a number of problems with prison statistics that should be acknowledged—beyond the already noted (and later to be further developed) point that these statistics represent an end stage in a system that is separated from the commission of crimes by many steps. Putting this point aside for the moment, we first note that there is no single, national enumeration of prisoners in the United States. Our knowledge of incarcerated offenders is based on periodic surveys of prison inmates, such as that conducted at the state level in 1991 (see Bureau of Justice Statistics 1993) and through state and federal departments of corrections in 1995 (see Wunder 1995). The latter survey of correctional agencies reached high levels of coverage on some matters, but encountered bureaucratic problems on others, for example, the California Department of Corrections (the biggest prison system and largest recipient of federal funds in the U.S.) reported national origins of "immigrants" based on data for citizens as well as noncitizens.

Meanwhile, the 1991 state survey reported that more than four percent, or 31,300 state prison inmates, were not U.S. citizens. The 1995 survey of departments of corrections indicated that more than seven percent, or 71,294 state and federal prison inmates were not U.S. citizens. The latter figure is larger in part because it includes federal prisoners, and also because the survey was conducted two years later following a growth spurt in prison populations. However, there are further problems with the size of the latter figure. A recent report on federal prisoners (Scalia 1996) indicates that there were less than 19,000 noncitizens in federal prisons in 1994, more than 7,000 fewer noncitizens than indicated in the *Corrections Compendium* report from one year earlier and introduced above (i.e., see Wunder 1995). The same report indicates in its first table that about 14,000 noncitizens were serving a sentence of imprisonment in a federal prison in 1991, while the last table in this report indicates that 9,916 noncitizens were inmates in federal prisons in 1991. Thus, we are left with a range of estimates that between four and seven percent of prison inmates in the United States are immigrants, compared with estimates that legal immigrants constitute between four and five percent of the U.S. population (Isbister 1996), with perhaps as much as one percent more of the U.S. population being illegal immigrants (Passel and Woodrow 1984). Of course, this tells us nothing about whether specific immigrant groups are over-or under-represented in prisons. The most systematic and comprehensive published data on the national origins of noncitizens in prisons is found in the 1991 Survey of State Prisons: nearly half of the immigrants surveyed in state prisons (47%) were from Mexico; about another quarter (26%) were from Latin and Caribbean countries. Together, these figures indicate that a large majority of inmates are of Hispanic origin. About 45 percent of the legal immigrants in the U.S. are Hispanic in origin (Heer 1996: Table 5.4). Of course, Hispanics are a heterogeneous group, coming not only from Mexico, but also from Cuba, the Dominican Republic, Columbia, El Salvador, Guatemala, and other countries in South America and the Caribbean Islands.

If we calculate rates of imprisonment for various immigrant groups, using estimates of the stock of recent immigrants from the various countries as our base, we find that there is substantial variation in rates of imprisonment among Hispanics. Before presenting these rates it should be emphasized that our calculations are tentative, because in addition to the difficulties in obtaining prison statistics already noted, there is also since 1990 no well developed enumeration of the stock of legal aliens—much less illegal aliens who have an obvious interest in remaining undetected—both of which would ideally be included in the denominators of calculated crime rates. In 1981, Congress eliminated the registry that identified all aliens living in the United States, and in its absence we therefore use census based counts of foreign-born persons who entered the U.S. between 1980 and 1990.

Calculated on the basis of the Survey of State Prisons, the imprisonment rate for U.S. citizens is about 3.5 per thousand population. This rate is used as the base for calculating ratios

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Table 1 • *Ratios of Immigrant to Citizen Incarceration Rates in U.S. State Prisons*

Immigrant Group	Observed Ratios	Ratios for Males 15–34	Adjusted Ratios			
			Age (Ψ)	Detention (π)	Conviction (μ_1)	Sentencing (μ_2)
Mexico	1.983	1.046	1.064	.337	.204	.197
Cuba	4.813	3.029	3.081	.977	.592	.572
El Salvador	1.034	.582	.592	.188	.111	.110
Dominican Republic	4.402	2.780	2.218	.703	.426	.412
Jamaica	2.335	1.513	1.554	.493	.298	.289
Columbia	2.479	1.719	1.749	.554	.336	.325
Guatemala	1.175	1.613	1.641	.520	.315	.305

presented in the first column of Table 1 for the most frequently imprisoned immigrant groups in U.S. state prisons. These figures suggest that immigrants from Cuba and the Dominican Republic are incarcerated at rates between four and five times those of citizens, that immigrants from Mexico, Jamaica and Columbia are incarcerated at rates from two to two and a half times those of citizens, and that immigrants from Guatemala and El Salvador are incarcerated at about the same rate as citizens. Recall that the circumstances of immigration from Cuba and the Dominican Republic have been shaped quite uniquely by political forces that have led many individuals with backgrounds in crime to migrate to the United States: for example, when Castro allowed over 100,000 people, including many prison inmates, to leave Cuba in 1980. The point is that in such situations the immigrants are not a cross-section of the sending population, and indeed are highly selected. A result is that there is considerable variability in Hispanic rates of imprisonment, and it is therefore a myth that these rates are uniformly high, or that there is a single or typical relationship between Hispanic immigration and crime. However, this cautious conclusion only suggests some of the most basic problems and possibilities involved in using prison statistics to inform our understanding of immigration and crime.

Clarifying Prison Statistics

Rates of immigrant imprisonment are further complicated by the fact that immigrants are younger and more often male than are citizens, and by the fact that immigrants are treated more restrictively than citizens by the criminal justice system. We know from studies of criminal behavior that involvement in crime is concentrated most heavily among younger males (Hirschi and Gottfredson 1983). We know further that restrictive decisions at entry points into the criminal justice system, especially at the point of bail decisions about pre-trial detention, have cumulative effects in increasing the likelihood of conviction and imprisonment (Hagan and Bumiller 1983; Hood 1992; Petersilia and Turner 1986). If our knowledge of these factors is used to inform our calculation of imprisonment rates, the meaning of these rates, and their relationship to immigration and criminal justice policies, can be clarified. That is, we must integrate and apply our knowledge of criminal behavior and criminal justice operations to make the calculation of imprisonment rates more meaningful.

Implicitly or explicitly, analysts of prison statistics on immigrants have wanted to estimate the relative probability or propensity of immigrants being involved in criminal behavior compared to citizens. Of course, this relative probability actually is unobserved and is therefore unknown. We already have indicated that the best known factor affecting the propensity to engage in crime is age, while a second factor that obviously further affects imprisonment rates

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is the probability of arrest once a crime has taken place, and then the conditional probability of incarceration given arrest. The latter is the product of the probabilities of conviction (given arrest) and of the probability of incarceration (given conviction). The challenge is to estimate the relative probabilities of involvement in criminal behavior of immigrants and citizens by combining the information we have on imprisonment with ancillary information we have on the age distribution of involvement in crime from studies of the causes of criminal behavior, as well as with the information we have from studies of criminal justice decision-making on causes of detention, conviction, and sentencing.

A beginning step in improving the use of the prison statistics on immigration and crime therefore involves adjusting by sex and age the base used in calculating imprisonment rates. To do this we recalculated imprisonment ratios using denominators for the compared rates that estimate the male populations of immigrants between 15 and 34 years of age from the various countries. To further assure representation of age differences we then adjusted these calculated rates for variation within the 15–34 age group. Next we must make some simplifying assumptions.

First, we assume that the age pattern of risks of being involved in crime is the same across the different immigrant and citizen groups (that is, that while there may be large potential differences in level, the shape of the age curve is the same for these different groups) and that the age pattern of the conditional risks of conviction and incarceration is also identical (albeit again different in level). We further make the more problematic assumption that the risk of arrest given an offense is identical across immigrant and citizen groups. This is likely a conservative assumption that will lead to an overestimation of the involvement of immigrants in crime from prison statistics, given notable evidence that the police are more likely to arrest immigrants than citizens for criminal behavior (see Zatz 1985). However, this source of police bias in outcomes is difficult to assess, and we therefore prefer to apply an assumption that understates rather than overstates the impact of police bias.

We begin, then, with the following expressions:

$$\begin{aligned} Z^{o} &= \Sigma_{x} c_{xz} z_{x} \\ I^{o} &= \Sigma_{x} c_{xi} i_{x} \end{aligned} \tag{1}$$

where Z^{o} and I^{o} are the observed incarceration rates (i.e., the measures of risk of incarceration) among citizens and immigrants respectively, c_{xz} and c_{xi} are the age distributions within the age groups 15 to 34 among citizens and immigrants respectively, and the quantities z_{x} and i_{x} represent the age specific incarceration rates (i.e., the age specific measures of risk incarceration) among citizens and immigrants.

Next we model z_{x} and i_{x} as follows (net of probabilities of arrest):

$$\begin{aligned} z_{x} &= (Z\lambda_{x})\delta_{z}\iota_{z} \\ i_{x} &= (I\lambda_{x})(\pi\delta_{z})(\mu\iota_{z}) \end{aligned} \tag{2}$$

where I and Z represent the unobserved levels of rates of criminal behavior among immigrants and citizens respectively, λ_{x} is the age pattern of crime rates (as noted above, assumed the same among immigrants and citizens), δ_{z} and ι_{z} are the conditional rates of being detained pre-trial and of being incarcerated (if detained), and π and μ are adjustment constants to transform the age patterns δ_{z} and ι_{z} into values appropriate for immigrants. The value of μ is the product of two components, μ_{1} and μ_{2}, the levels of conviction rates (given detention) and of incarceration (given conviction). Thus, the observed ratio of the immigrant incarceration rate to the citizen's incarceration rate is can be obtained by replacing (2) into (1) to obtain:

$$I^{o}/Z^{o} = \psi(I/Z)\pi\mu \tag{3}$$

where ψ is a quantity that expresses the ratio of the incarceration rate among immigrants to that of citizens that would be observed if both groups only differed in terms of their age distri-

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bution but not in terms of their age specific rates of crime (λ_x) or the (conditional) pre-trial detention rates (δ_z), or the (conditional) age-specific incarceration rate (ι_z), namely,

$$\psi = (\Sigma_x c_{xi}\lambda_x\delta_z\iota_z)/(\Sigma_x c_{xz}\lambda_x\delta_z\iota_z)$$

where, as always, the summation is over pertinent adult ages, x. Since δ_z and ι_z are assumed to be age invariant, they cancel out from the equation and we remain with the simpler expression

$$\psi = (\Sigma_x c_{xi}\lambda_x)/(\Sigma_x c_{xz}\lambda_x)$$

From (3) we can easily solve for (I/Z), the unobserved ratio of the immigrant to citizens' crime rates:

$$I/Z = (\Psi\pi\mu)^{-1}(Z^o/I^o) \quad (4)$$

Expression (4) suggests a base for estimating the target quantity, the ratio I/Z of immigrant to citizen rates of criminal *behavior*. Indeed, the expression suggest that we can adjust the ratio of observed *incarceration* (Z^o/I^o), using knowledge about the sequence that leads from criminal behavior to imprisonment. The adjustment that reflects this knowledge consists of multiplying the observed ratio of imprisonment rates by the reciprocal of the product of ψ and ($\pi\mu$).

Estimates of ψ can be obtained easily from reports of the age distribution of immigrants and citizens.[1] It is somewhat more difficult to obtain estimates of π and μ, since this requires us to know the magnitude of the effect of citizenship on conditional risk of detention, measured by π, and on the conditional risk of conviction and eventual incarceration, measured by μ.

There are no national data from which estimates of the risks of detention, conviction and incarceration can be obtained. However, very useful research has been done by Pennell, Curtis and Tayman (1989) on these decision points in two important jurisdictions where these decisions are made for substantial numbers of Hispanic immigrants: El Paso and San Diego. This study was undertaken to examine whether rising crime rates in El Paso, Texas and San Diego, California in 1986 could be attributed to, among other factors, the influx of undocumented aliens. A wide range of felony cases were included in both cities, using the population of cases in El Paso and a stratified random sample with equal probabilities in San Diego. We have reanalyzed the data from this research for use in the calculations presented below. A reanalysis of the data was necessary because Pennell, Curtis and Tayman (1989:45–92) present abridged results of their analyses in El Paso and San Diego. The reanalysis is based on logit models in which all of the independent and dependent variables are coded in an easily interpreted binary fashion.[2]

1. The quantity ψ can be estimated from known values of c_{xi} and c_{xz} and a standard age-specific schedule λ_x. Given the lack of information in this regard, we have used as a standard the national age-specific crime rates directly calculated from national statistics. This may not be an optimum choice but since age-specific crime rates seem to have a fairly universal and invariant shape (Hirschi and Gottfredson 1993), we are confident that our calculations will be robust to minor deviations from the standard pattern adopted here. The reader should recall that another assumption made along the way, namely that δ_z and ι_z are age-invariant, is probably more problematic, though we do not have sufficient evidence to suggest that it is less defensible.

2. The analysis is based on all 2,268 El Paso felony cases and 1,643 randomly selected San Diego felony cases processed from July 1, 1995, through June 30, 1986. A sampling fraction of .4 was used in San Diego to insure inclusion of a sufficient number of illegal aliens. The felony crime categories represented in both cities were: homicide (2%); rape (2%); robbery (11%); aggravated assault (19%); burglary (27%); larceny/theft (14%); motor vehicle theft (7%); and felony narcotics (18%). A methodologically conservative approach was used to identify legal and illegal immigrants. If the arrest report indicated that an arrestee was foreign-born and fit into additional categories, such as "interpreter needed" or "no permanent address," the person was considered a potential alien or non-U.S. citizen. The Immigration and Naturalization Service (INS) then examined its records to ascertain actual citizenship status. The INS review produced three groups: illegal aliens; legal immigrants; and those whose citizenship could not be confirmed. The illegal and legal immigrant categories are combined in the logit models estimated in this paper and the original categories are used in the cross-tabulations.

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Table 2 • *Logit Models for Being Detained, Convicted, and Imprisoned in El Paso and San Diego*

	El Paso		
Variables	*Pre-Trial Detention (n = 2253)*	*Conviction (n = 2253)*	*Incarceration (n = 885)*
Immigrant	1.35 (.13)*	−.08 (.19)	−.52 (.40)
Male	.63 (19)*	.48 (.19)*	.90 (.39)*
Less than 20	−.67 (.11)*	.34 (.11)*	−1.13 (.19)*
Violence	−.49 (.13)*	−.68 (.13)*	.43 (.25)**
Drugs	−.70 (.14)*	.06 (.14)	.08 (.24)
Violence*Immigrant	−1.10 (.26)*	−.09 (.29)	.36 (.53)
Drugs*Immigrant	−.82 (.39)*	−.20 (.30)	1.17 (.51)*
Detained		.44 (.11)*	1.74 (.19)*
Detained*Immigrant		.33 (.21)	−.61 (.42)
Log likelihood	−1420	−1449	−506
	San Diego		
	Pre-Trial Detention (n = 1286)	*Conviction (n = 1286)*	*Incarceration (n = 815)*
Immigrant	1.26 (.17)*	−.59 (.20)*	−.16 (.40)
Male	1.42 (.37)*	−.12 (.21)	−.16 (.40)
Less than 20	−.05 (.17)	.15 (.18)	−.32 (.25)
Violence	−.17 (.22)	−.33 (.21)	.18 (.70)
Drugs	−1.46 (.37)*	−.37 (.22)**	−2.96 (1.02)*
Violence*Immigrant	.05 (.30)	−.29 (.38)	.71 (.41)**
Drugs*Immigrant	−.07 (.44)	.57 (.33)**	2.16 (1.13)**
Detained		2.29 (.33)*	2.29 (.29)*
Detained*Immigrant		.79 (.42)**	−1.15 (.43)*
Log likelihood	−730	−683	−372

* $p < .05$, two-tailed ** $p < .10$, two-tailed

The results of our reanalyses of the El Paso and San Diego data sets are presented in Table 2. There is good reason to believe that given involvement in and arrest for a crime, Mexican and other immigrants are at greater risk of detention, conviction, and imprisonment than other groups. As noted above, pre-trial detention is known more generally to increase vulnerability to conviction and imprisonment, and in both El Paso and San Diego illegal aliens were much less likely than others to be released from jail prior to trial. For example, in El Paso only 14 percent of illegal immigrants compared to over 50 percent of all others were able to "bail-out" prior to trial. This difference may be associated with the fact that the Immigration and Naturalization Service can place "holds" on illegal immigrants, that illegal aliens are financially less able to post bail, that illegal immigrants are less likely to have the community ties often required for release, and that illegal immigrants and immigrants more generally are therefore more likely to be judged potential "flight risks," from the time of arrest to final court disposition. The problem is that when accused persons are unable to obtain release they have greater difficulty generating resources to defend themselves in court, making them more vulnerable to conviction, and ultimately to imprisonment.

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The results in Table 2 confirm that immigrants in general, in both El Paso and San Diego, are more likely to be detained prior to trial, and that in turn detention before trial greatly increases risks of conviction and imprisonment. Note, for example, that the coefficient for the variable "immigrant" is associated with relative risks of being detained that are 3.86 (exp[1.35]) and 3.52 (exp[1.26]) times as high as for non-immigrants in the El Paso and San Diego data respectively. The results also show that there are large effects of being detained on conviction (column 2) and on incarceration (column 3). Thus, for example, in the El Paso data the relative risk of being incarcerated for those who were detained is about 5.70 (exp[1.74]) times as high as for those who were not detained. Even taking into account the negative (but insignificant) effects of being incarcerated among those convicted (−.52), the net effects of immigration status on final incarceration amounts to a relative risk that is 7.1 (exp[1.35 + 1.74 − .52 − .61]) times as large as for the non-immigrant. That is, immigrants are at greater risk of conviction and imprisonment because they are more vulnerable to pre-trial detention. In addition, although the effects are only borderline significant, immigrants in El Paso and San Diego who are charged with drug offenses are more likely than others to be sentenced to prison. Thus, the relative risks of an immigrant being incarcerated for committing a drug offense are twice as large (exp[1.17 − .52]) as for non-immigrants. These differences cannot be the result of immigrants having more extensive criminal histories, because noncitizens are much less likely to have known criminal records (Scalia 1996). Patterns of detention, conviction, and sentencing combine to increase the vulnerability of immigrants to incarceration, which we can now take into account in adjusting our calculations of imprisonment ratios. As discussed further below, to do this we have averaged the mean values and the effect coefficients for the predictor variables in the models estimated in El Paso and San Diego. We also did these calculations separately by city with results that do not change our conclusions (i.e., the lower bound estimate from either city led to identical substantive conclusions).

The results of the calculations outlined above are now brought together in the remaining columns of Table 1 that began in column one with the ratios of imprisonment rates which implied that immigrants are more involved in crime than citizens. In column two we observe that simply modifying the base for these calculations to consider 15 to 34 year old males reduces these ratios substantially. For example, the rate for Mexican immigrants (47.61) is now quite similar to the U.S. citizen rate (45.51). The resulting imprisonment ratio in the second column of Table 1 for Mexican to U.S. citizens therefore is approximately unity (1.046). The imprisonment rates for some of the remaining countries are still substantially higher than the citizen rate. The Jamaican, Guatemalan, and Colombian ratios cluster at an intermediate level, while the Dominican and Cuban rates cluster at a higher level. However, even the latter rates are now about three times the citizen rates, where they were previously more than four times the citizen rates.

We next calculate the terms included in equation (4), which first further adjusts the distribution between ages 15 and 34 of the various groups. From 1990 national census data we estimate that the first term in this expression, ψ, hovers around .98. In contrast with our earlier finding of strong age effects, this means that differences in age composition within this 15 to 34 age group exert only a trivial influence, in the direction of slightly inflating citizen's incarceration rates. Although changes within this specific age grouping in the future may introduce notable effects, for the moment our preliminary and more general adjustment for age is much more noteworthy.

We use the results from the logistic models above with El Paso and San Diego data to estimate effects of immigration relative to citizenship status on the probabilities of detention, conviction, and incarceration (if convicted). We first calculated average estimates of effects and mean values by pooling the San Diego and El Paso data.[3] We then used the mean values of the

3. There are a number of different ways to generate pooled estimates from the two data sets. We followed the most straightforward one, where no implicit weighting scheme is imposed.

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variables and the estimated effects from the pooled sample to calculate predicted values for the corresponding probabilities and relative risks. We thus obtained values for the remaining constants in equation (4): $\pi = 3.155$; $\mu_1 = 1.650$; and $\mu_2 = 1.708$. The first of these figures implies that the average risk of detection among immigrants is more than three times the average risk for citizens. The next figure indicates that the average risk of conviction given detention among immigrants is 1.65 times as high as that among citizens. The final figure implies that the average immigrant risk of incarceration (given conviction) is 1.71 times as high as among citizens. It is clear from the differences in these quantities and the expressions derived above that observed differences between immigrants and citizens' incarceration rates are strongly and disproportionately affected not only by age differentials as indicated earlier, but also by differences in risks of detention prior to trial and sentencing.

We can now go on to actually estimate the consequence of introducing the contribution of each of the components in our analysis to differences between the observed incarceration rates. To do this we first write equation (4) more generically as:

$$Q = Q_0 F_1 F_2 F_3 F_4 \tag{5}$$

where Q stands for the ratio of I/Z, Q_0 for the ratio I^o/Z^o and F_k's, k = 1, . . ., 4 correspond respectively to the reciprocals of ψ, π, μ_1 and μ_2. The addition of each new term to this equation alters the estimate of the adjusted incarceration rate, as revealed across successive columns two through six in Table 1.

Thus the first column displays the observed ratios of raw immigrant to citizen incarceration rates. The second column shows the new ratios of incarceration rates with 15 to 34 year old male population bases. Successive columns display the ratios of the incarceration rates among immigrants and citizens given the influence of the further adjustment for variation in the 15 to 34 year age grouping (column 3), the influence of age distributions and detention rates (column 4), the influence of age distributions, detention rates and conviction rates (column 5), and, finally, the influence of age distributions, detention rates, conviction rates, and jail sentencing (column 6). Column six can be interpreted as the estimated values that would be observed in the implied ratios of criminal behavior between immigrants and citizens if there were no differences between immigrants and citizens in age composition, detention rates, conviction rates, and rates of sentencing to prison. That is, with the assumptions spelled out earlier, these adjusted ratios are our estimates of the relative involvements of immigrants and citizens in crime.

Although we have not suggested that these estimates are in any sense precise, we have argued that they represent a more meaningful understanding of the relative involvements of immigrants and citizens in crime, based on an integration of our knowledge of the causes of criminal behavior and justice system reactions to it. The differences between these estimates and those based on prison statistics alone are striking. For example, recall that the ratios of the raw incarceration rates of immigrant groups and citizens in the first column of Table 1, excluding those from El Salvador and Guatemala, greatly exceeded unity, with Mexicans estimated more than twice as criminal as citizens, and Cubans and Dominicans more than four times more involved in crime. Adjusting the base age population to represent 15 to 34 year old males markedly reduces most of these ratios, although they still mostly exceed unity. A further adjustment for age within the 15 to 34 age category makes little difference in column three. However, when differences in detention were taken into account in column four, only the ratio for Cubans remains close to unity, while the remaining ratios move substantially below unity. Smaller reductions in these ratios occur with adjustments for risks in conviction and imprisonment in columns five and six.

The contrasts between successive columns in Table 1 indicate that the biggest sources of differences between estimated (unobserved) involvement in criminal behavior and observed incarceration rates derive from differences in the age composition of immigrant and citizen

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groups and their relative risks of being detained prior to trial and sentencing. For example, the difference in inferences that results from taking these two factors into account is the gap between a ratio of raw incarceration rates that suggests Mexicans in the U.S. are twice as involved in crime as citizens, and a ratio that suggests that with age and detention differences taken into account, Mexicans are half if not less as involved in crime as citizens. Indeed, the revised estimates from our analysis go so far as to suggest that there may be no group of Hispanic immigrants that, with age, detention and other factors taken into account, is more highly involved in crime than citizens.

We would be remiss if we did not note a final source of misperceptions that may result from uncritical reliance on prison statistics. This final consideration involves the ways in which prison statistics are often used to more specifically characterize immigrant criminality in relation to drugs. The 1991 survey of state prison inmates reported that nearly half of all alien inmates were incarcerated for drug offenses, that about 40 percent of these alien inmates used drugs during the month prior to arrest, and that about 20 percent were under the influence of drugs at the time of their current offense. The 1991 survey reports that very high proportions of alien inmates from Columbia (87%) and the Dominican Republic (67%) were incarcerated for drug offenses.

While it is the case that just over 20 percent of all inmates were incarcerated for drug offenses in 1991, about half of all inmates said they had been using drugs in the month before their current offense, and more than 30 percent of all inmates said that they had been under the influence of drugs at the time of their current offense. So while there well may be a concentration in drug offenses among some imprisoned Hispanic offenders, especially from Columbia and the Dominican Republic, levels of drug use among Hispanic offenders in general seem to be about the same or lower than in the broader population of inmates. We do not know the extent to which the imprisonment of Hispanic defendants from particular countries for drug offenses is a product of their concentration in this kind of crime as contrasted with the courts selecting this kind of crime among immigrants from these nations for prison sentences.

However, we do have persuasive evidence that drug crime is *not* characteristic of legal immigrants, and is actually *un*characteristic of *il*legal immigrants. This evidence comes from a closer inspection of arrestees in the El Paso and San Diego criminal justice systems considered earlier. When arrestees are categorized by charge and citizenship status in these cities in Table 3, we see that immigrants are quite similar to citizens in their tendency to be arrested for drug, property, and violent crimes. Many drug offenders are prosecuted in the federal courts,

Table 3 • *Category of Arrest and Citizenship Status of El Paso and San Diego Arrestees**

	Citizenship Status				
City/Arrest	*U.S. Citizens*	*Illegal Aliens*	*Immigrants*	*Unconfirmed Citizenship Status*	*Total*
El Paso					
Violent crimes	602 (36%)	86 (25%)	54 (35%)	33 (32%)	775
Property crimes	728 (44%)	226 (66%)	73 (47%)	50 (49%)	1,077
Narcotics violations	338 (20%)	32 (9%)	27 (18%)	19 (19%)	416
San Diego					
Violent crimes	1,283 (36%)	124 (23%)	36 (35%)	47 (26%)	1,490
Property crimes	1,555 (43%)	343 (63%)	37 (36%)	86 (47%)	2,021
Narcotics violations	760 (21%)	80 (15%)	29 (28%)	51 (28%)	920

* Reconfigured from Pennell, Curtis and Tayman (1989:51, 74).

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including many immigrant offenders, but the evidence in these settings is that noncitizen offenders tend more often than citizens to be involved in "minor" and "low level" drug offenses (Scalia 1996:7). Meanwhile, illegal immigrants are less likely to be arrested for drug and violent crimes, and more likely to be arrested for property crimes. About two thirds of the illegal immigrants in each of these cities are arrested for property crimes, with only nine to 15 percent arrested for drug crimes. The involvements of legal immigrants and citizens in drug crime cluster between 20 and 30 percent, and their involvements in property crime cluster between 40 and 50 percent.

Discussion and Conclusion

We know relatively little about law making, breaking and punishment in relation to Hispanic populations of the United States. Noting the rapid growth of this and other immigrant sectors of the American population, Sampson and Lauritsen (1997:364) recently have suggested that, "the future picture of criminal justice processing may be closely tied to the experiences of race or ethnic groups that have heretofore been neglected by mainstream criminological research." At the same time, there is increasing public concern about the presence in prisons of immigrants generally, and Hispanics and Mexicans more specifically. This concern is increasing as state and federal governments become more conscious of the high costs of keeping these individuals in prisons. Sociological criminology has not kept pace with these changes, and it has not integrated the knowledge that it has available in ways that could notably improve public understandings of how ostensibly neutral immigration and criminal justice policies act to inflate Hispanic rates of imprisonment.

Imprisonment is a costly sanction, financially as well as otherwise, and the public and its politicians are eager to find ways to shift and reduce this expense. One response involves efforts by states to shift expenses to the federal government by demanding compensation for the costs of incarcerating immigrants; second is to press for the deportation of immigrants convicted of crimes; and a third response is to lobby for limitations on immigration and for stricter control of illegal immigration (see Cornelius, Martin and Hollifield 1994). It is of particular concern, in the political and economic context of cost shifting, that the same correctional departments that collect crime statistics may have a direct financial interest in the size of their immigrant inmate populations and in seeing these numbers reported and well publicized.

The purpose of this paper is not to assess directly the wisdom of the above policies, but rather as Sutherland and Cressey long ago urged, to integrate information and sociological knowledge about the relationship between immigration and crime. Prison statistics often are used to make the point that the number of immigrants in prison is large, and that a large proportion of immigrants who are in prison for drug charges are from Mexico. However, it is also plainly the case that increasing numbers of immigrants have been coming to the United States from Mexico to work for employers who are eager to have them as an inexpensive source of labor (Calavita 1994). These immigrants are disproportionately young and male, and therefore of an age and gender known to be disproportionately and transiently involved in crime, regardless of citizenship. So a first concern is to take into account not only the increased immigration that is occurring from Mexico, but also the age and sex distribution of these immigrants, in considering the numbers of Hispanic inmates and the connection of these numbers to immigration policies designed to provide inexpensive labor. When we take age and gender into account, we find Mexican immigrants, the most numerous Hispanic immigrants to the United States, are in state prisons at an adjusted rate that is not strikingly different from U.S. citizens.

Furthermore, it is important to take into account that, as a result of ostensibly neutral pre-trial detention policies, immigrants from Mexico and elsewhere are also subject to differential treatment in the criminal justice system. We have demonstrated that immigrants in El

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Paso and San Diego are at greater risk of being detained prior to trial, and that this results in their increased likelihoods of being convicted and imprisoned. When our knowledge of this differential treatment is integrated into estimates of ratios of immigrant to citizen offenders, using equations that begin with observed numbers of immigrants and citizens in state prisons, we find that these ratios are reduced below unity, the level that would indicate that immigrants are as involved in crime as citizens. The results reported in the preceding two paragraphs cast doubt on the hypothesis that immigration causes crime and help to make more apparent the ways in which immigration and criminal justice policies inflate the representation of Hispanics in prisons. As Tonry (1997) suggests, this kind of analysis increases the prospect that unforeseen consequences of ostensibly neutral policies will become foreseeable, and as a result more prominent in the policy making process.

Meanwhile, it also often is argued that immigrants, and especially illegal immigrants, are a source of drug problems in the United States. Yet we have seen that there is little evidence that this is the case when arrest records in cities such as El Paso and San Diego are examined. In these cities we found that illegal immigrants are less likely than citizens to be involved in drug crime, and instead they are most distinctively involved in property crime. This kind of petty property offense activity is consistent with the picture of offending provided in Richard Freeman's (1996) foraging model of crime. Thus young male illegal immigrants may be most likely to become involved in petty property crime as they attempt to satisfy basic subsistence needs while moving through the early stages of seeking, finding, losing, and regaining employment. To the further extent that Hispanic Americans are overrepresented in prison statistics because of differential treatment by the criminal justice system, it may be necessary to extend Freeman's theory into a "forage and storage" model of crime that incorporates the effective "warehousing" of this group in penal institutions. Legal immigrants in El Paso and San Diego are involved in drug crimes at about the same rate as citizens, a finding that casts additional doubt on the hypothesis that immigration in and of itself causes crime.

Notwithstanding our conclusion that the link drawn between immigration and crime is misleading, to the extent of constituting a mythology—further work should nonetheless be attentive to at least two poorly understood issues. The first involves the important task of making projections into the future that take into account interrelations between immigration, fertility and social behavior. Here it is important to acknowledge and plan for the possibility that even if immigrant group propensities to crime remain unchanged from those we have estimated in this paper, higher rates of fertility among immigrant groups, either alone or in combination with other factors, such as criminal justice system bias, could result in immigrants forming steadily larger proportions of prison populations in the future. These processes are in need of further study.

Second, it is also likely that specific groups of immigrants, much like specific groups of citizens, do have a heightened propensity that leads them to be disproportionately involved in crime. This typically involves countries with relatively few immigrants to the United States. In these instances in which immigration is rather limited, there may be unique social networks and selection processes that explain the higher rates of crime involvement. If legal immigration from these countries were allowed to increase, it is plausible that rates of crime associated with these immigrant groups in the United States would substantially decline, if only because the effects of these selection processes and social networks would be diluted. We know too little about these special cases to say much more; certainly there are too few Cubans and Dominicans in our El Paso or San Diego data sources to allow confident conclusions. In any event, it would be quite dangerous to extrapolate from such cases to the experience of immigrants more generally.

Finally, our more general finding that Hispanic immigrants are not more involved in criminal behavior than citizens helps to resolve a paradox in the picture of Mexican immigration to the United States, since by other measures of well-being—including smoking, alcohol consumption, drug use, and pregnancy outcomes—Mexican immigrants are generally found

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to do as well and sometimes better than citizens (Scribner 1996). This is often thought to be the result of the strength of extended and nuclear families and religion in Mexican communities. Insofar as this is the case, we may wish to place the priority in policy formation on ways to preserve, protect, and promote the social and cultural capital that Mexican immigrants bring to their experience in the United States. An increasing reliance on imprisonment likely detracts from this goal by banishing immigrant males from their families and communities.

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On Immigration and Crime

by Ramiro Martinez, Jr., and Matthew T. Lee

ABSTRACT

The connection between immigration and crime is one of the most contentious topics in contemporary society. These discussions are not new, as debates on the issue date back more than 100 years. A general point on which both pro- and anti-immigration writers agree is that, as we enter the new millennium, the latest wave of immigration is likely to have a more important impact on society than any other social issue. In this essay, we survey the vast body of theoretical and empirical works on the relationship between immigration and crime in 20th-century America. Throughout, we include new writings as well as older, sometimes neglected works. We discuss three major theoretical perspectives that have guided explanations of the immigration/crime link: opportunity structure, cultural approaches, and social disorganization. We also examine empirical studies of immigrant involvement in crime. We conclude with a review of public opinion about immigrants, especially as it relates to immigrants and crime, and then provide original data on the connection between public opinion and immigrant crime.

There are important reasons to believe that immigrants should be involved in crime to a greater degree than native-born Americans. For example, immigrants face acculturation and assimilation problems that most natives do not, and immigrants tend to settle in disorganized

Ramiro Martinez, Jr., is Associate Professor of Criminology and Criminal Justice at Florida International University in Miami and a member of the National Consortium on Violence Research at Carnegie Mellon University. Matthew T. Lee is Assistant Professor of Sociology at the University of Akron in Ohio.

neighborhoods characterized by structural characteristics often associated with crime, such as widespread poverty, ethnic heterogeneity, and a preponderance of young males. However, despite claims by pundits and writers that high levels of "immigrant crime" are an unavoidable product of immigration, scholars rarely produce any systematic evidence of this recently reemerging social problem.

Although a host of reasons exists to expect that immigrants are high-crime prone, the bulk of empirical studies conducted over the past century have found that immigrants are typically underrepresented in criminal statistics. There are some partial exceptions to this finding, but these appear to be linked more to differences in structural conditions across urban areas where immigrants settle rather than to the cultural traditions of the immigrant groups. Local context is a central influence shaping the criminal involvement of both immigrants and natives, but in many cases, compared with native groups, immigrants seem better able to withstand crime-facilitating conditions than native groups. In conclusion, this review suggests that native groups would profit from a better understanding of how immigrant groups faced with adverse social conditions maintain low rates of crime.

The linkage between immigrants and crime is one of the most controversial contemporary social issues. Discussions of this relationship are notably contentious; they are also not new, as debates on the matter date back more than 100 years. The one point on which writers both pro- and anti-immigration agree is that as we enter the new millennium the latest wave of immigration is likely to have a more significant impact on society than any other social issue (Suarez-Orozco 1998; Brimelow 1996). If the past is any guide, the immigrant-crime relationship will be at the center of attempts to make sense of this impact.

Historically, public opinion about immigration, and the immigrant-crime link especially, has been formed by stereotypes more often than reliable empirical data (Espenshade and Belanger 1998; Simon 1985). Similarly, early 20th-century immigration policy was guided more by questionable research and prejudicial beliefs than a solid foundation of knowledge based on the existing scientific literature (as cited in Sellin 1938). It is therefore essential to systematically review the scholarly literature on immigration and crime so that current public and policy debates may be better informed.

Historically, public opinion about immigration, and the immigrant-crime link especially, has been formed by stereotypes more often than reliable empirical data. Similarly, early 20th-century immigration policy was guided more by questionable research and prejudicial beliefs than a solid foundation of knowledge based on the existing scientific literature.

In this essay, we survey the vast body of theoretical and empirical works on the relationship between immigration and crime in 20th-century America. Throughout, we include both contemporary writings as well as older, sometimes neglected works. It is important to note that this literature is problematic in at least three ways (see Short 1997; Marshall 1997): (1) discussions of a particular immigrant group usually mask the wide variations in living conditions (e.g., socioeconomic status) of persons within that group; (2) the use of broad racial and ethnic categories conceals important subgroup differences and makes comparisons across studies difficult (e.g., Asian immigrants may be grouped together in one study but treated separately in another study as Filipinos, Chinese, Vietnamese, etc.); and (3) patterns of criminal involvement vary widely across generations of immigrant groups, and there is no agreed-upon rule for determining how many generations should pass before "immigrants" can be considered "natives."

In the first section of this essay, we discuss the three major theoretical perspectives that have guided explanations of the immigration-crime link: opportunity

structure, cultural approaches, and social disorganization (see Bankston 1998). Next, we examine empirical studies of immigrant involvement in crime. We have organized this body of research into subsections on group-specific comparisons of early and recent immigrants in the United States. Finally, we review public opinion about immigrants, especially as it relates to immigrants and crime, and then provide original data on the relationship between public opinions and immigrant crime.

Theoretical Perspectives

A seemingly infinite variety of explanations have been proposed to make sense of the voluminous and often contradictory data produced by research on the relationship between immigration and crime. Some early 20th-century American writers alleged that immigrant groups were biologically deficient compared with nonimmigrants and that crime was one of a host of deleterious outcomes that could be expected as long as "inferior" immigrants were allowed to enter the country. For example, one writer feared that immigration was likely to "destroy the inherent racial and family-stock qualities—physical, mental, and spiritual—of the people of the immigrant-receiving country" (Laughlin 1939, 5). Much of this biologically oriented work was associated with the long-discredited eugenics movement, and although it failed to provide solid empirical evidence to back such claims, these ideas did guide U.S. immigration legislation in the 1920s (Sellin 1938; Hagan and Palloni 1998).

Current popular theories tend to emphasize social-psychological (e.g., stress from acculturation pressures) or, more often, sociological variables (e.g., community disorganization). There is much disagreement between and within schools of thought as to the importance of different factors, but most theoretical work can be classified into one of the major perspectives mentioned in the next section. Explanations usually draw on several theoretical elements, and it is surely an analytical contrivance to think that "cultural" factors can be separated from their "structural" contexts.

Opportunity structure

Opportunity structure theories stress the material and social structures that shape the values and activities of groups in American society (Bankston 1998). Because legitimate opportunities for wealth and social status are not equally available to all groups, some will "innovate" by taking advantage of available illegitimate opportunities. This type of explanation was popularized by Merton (1938) and draws attention to the ways in which disadvantaged groups (which often includes immigrants) may be denied the legitimate means (e.g., jobs) to

attain culturally prescribed goals (e.g., a middle-class lifestyle). Cloward and Ohlin (1960) added the notion that some groups, particularly those living in "high crime" urban areas, have more illegitimate opportunities than others.

Scholars have long noted the tendency for new immigrants to settle in urban neighborhoods characterized by poverty, substandard housing, poor schools, and high crime rates (Thomas and Znaniecki 1920; Taylor 1931; Shaw and McKay [1942] 1969; Hagan and Palloni 1998). Segregated into such neighborhoods, immigrants may turn to crime as a means to overcome blocked economic opportunities or to organized crime to gain a foothold in politics (Whyte 1943). Other writers have suggested that previously noncriminal immigrant groups may simply be "contaminated" by the criminal opportunities that abound in their neighborhoods (Lambert 1970, 284; compare Sampson and Lauritsen 1997 on the "proximity" hypothesis). According to this view, immigrant criminality is more a function of preexisting structural factors like poverty (Yeager 1997); a preponderance of young, unattached males (Taft 1936; Gurr 1989); or the availability of alcohol (see Alaniz, Cartmill, and Parker 1998) than either the biological makeup or cultural traditions of immigrant groups.

Contemporary immigrant gangs can be viewed as an alternative means of securing wealth and status in urban areas in which immigrants are concentrated and a loss of blue-collar jobs has eroded the principal basis of upward mobility used by earlier generations of immigrants (Gans 1992). Patterns of factory life associated with the industrial revolution socialized earlier waves of immigrants (e.g., Irish, Italians) and facilitated their economic stability and assimilation into American culture, thus suppressing their crime rates (Lane 1997). But the contemporary loss of industrial jobs, traceable to the recent transition of the economy from a manufacturing to a service orientation, has had drastic consequences on the legitimate (and illegitimate) opportunities in American cities (cf. Wilson 1987; Anderson 1990). Although deindustrialization impacts both immigrants and nonimmigrants, newcomers (who are both economically and culturally marginal in American society) may find the potential benefits of the "informal economy" particularly attractive (Bankston 1998; see also Vigil and Long 1990). Since the possibility of gang membership is not open to everyone and is, in fact, usually based on ethnic identity, among other things (see Sanchez-Jankowski 1991), the availability of illegitimate opportunities in gangs is one structural variable that differentially influences immigrant crime across social units such as cities or neighborhoods.

Cultural approaches

In addition to this list of structural issues, scholars have viewed cultural forces as influencing criminal involvement, and immigrant crime in particular. The

"culture of poverty" thesis—where low-income people adapt to their structural conditions in ways that perpetuate their disadvantaged condition—is one example of a cultural explanation (Lewis 1965). Thus, engaging in crime as a means of acquiring social status draws children away from schoolwork, which reduces the probability of future economic advancement. A variant of this explanation for crime, the "subculture of violence" thesis, suggests that violence can become a "normal" and expected means of dispute resolution in economically disadvantaged areas (Wolfgang and Ferracuti 1967).

Because immigrants and ethnic minorities are more likely than native whites to reside in such areas, these cultural theories would seem particularly useful. Of course, as Sampson and Lauritsen (1997) point out, the subculture of violence theory cannot explain the wide variations in violent crime rates across structurally diverse minority neighborhoods, and they conclude that structural conditions are ultimately more important than cultural traditions. Bankston (1998) also notes that the existence of gangs across a variety of disadvantaged immigrant groups implies that they are the product of similar structural conditions rather than an outcome of the cultural traditions of any specific group. Finally, it would be a mistake to attribute comparatively high rates of minor crimes, such as illegal gambling among New York City's Puerto Rican newcomers in the 1950s, to their specific cultural traditions, since similarly disadvantaged native blacks also had high rates. As Handlin (1959, 101) points out, neither ethnic group saw gambling as a crime. Instead, the numbers game represented the American ideal of economic advancement.

Nevertheless, some writers suggest that certain types of crime are more prevalent among specific immigrant groups because of cultural traditions that are brought from the home country. Both Sutherland (1947) and Sellin (1938) argued that immigrant groups had "cultural predispositions" toward certain crimes. For example, in the 1920s, Italians had high rates of conviction for homicide but low rates of arrest for drunkenness (Sutherland and Cressey 1960). A study of Hawaii during the same period revealed that Chinese immigrants brought with them traditions of certain types of graft and gambling (Lind 1930b).

One prominent strain of cultural theory that is especially well suited to the immigration-crime relationship can be found in Sellin's (1938) writings on "culture conflict." Sellin (p. 21) recognized that the criminal law reflects the values of the "dominant interest groups" in society, and that the values of other social groups, particularly immigrants, were quite different. In cases in which the cultural codes of subordinate and dominant groups conflict, legal agents label as deviant the behavior of members of the subordinate classes. Nevertheless,

the criminal may be acting according to subculturally accepted norms and feel no "mental conflict" when violating the law. Sellin (p. 68) provides a classic example:

> A few years ago a Sicilian father in New Jersey killed the sixteen-year-old seducer of his daughter, expressing surprise at his arrest since he had merely defended his family honor in a traditional way. In this case a mental conflict in the sociological sense did not exist.

Thus, Sellin (p. 69) distinguishes "internal" (mental) conflict that may involve guilt or shame from "external" (i.e., at the level of cultural codes) conflict that does not. As society becomes more complex through processes of differentiation (of which immigration is just one), the potential for conflict among operative "cultural codes" also increases.

Four years earlier, Sutherland (1934, 51–52) offered a similar explanation, stating that "the conflict of cultures is therefore the fundamental principle in the explanation of crime [and] . . . the more the cultural patterns conflict, the more unpredictable is the behavior of the individual." This thesis has broad applicability to all social groups, but it clearly provides a key reason why immigrants are likely to engage in crime, perhaps suggesting that immigrants should have higher crime rates than the native population, other factors being equal.

Another body of literature in the cultural tradition has focused on the processes by which immigrants adapt to the traditions of the host country (Padilla 1980). It has long been asserted that acculturation to a new environment involves "adjustment to heterogeneous conduct norms" and this, in turn, can lead to higher crime rates (Sellin 1938, 85). Immigrant gangs, for example, usually owe as much to American cultural traditions as to those of the immigrant culture; adult Vietnamese refer to Vietnamese delinquents as "Americanized" (Bankston 1998). Immigrant gangs also form as a reaction to ethnic tensions in diverse neighborhoods; they provide physical protection from other ethnic-based gangs and maintain ethnic identities in the face of pressures for assimilation (Chin 1990; Du Phuoc Long 1996).

Other theoretical and empirical work on acculturation and crime found that delinquency among Korean-American youths varied by mode of acculturation: separation, assimilation, and marginalization. Interestingly, assimilated youths were more likely to be delinquent than those who were separated or marginalized (Lee 1998). Similarly, Stepick (1998) reports that Haitian children in South Florida adjust to extreme anti-Haitian sentiment by trying to "pass" as being African-American. Increasingly, these children are becoming part of the

criminal justice system as a result of alienation from both their Haitian background and American society (p. 120).

Social disorganization

The social disorganization perspective, although not denying the importance of cultural or structural forces, adds to the other perspectives a concern with the breakdown of community social institutions that results from social change. Bursik (1988, 521) concisely describes disorganized areas as possessing an "inability to realize the common values of their residents or solve commonly expressed problems." In organized neighborhoods, local community institutions work together to realize community goals, protect values, and generally control the behavior of community members in ways that conform to these goals and values. Bankston (1998) notes that immigration may undermine established institutions via a process of population turnover, while it also makes agreement about common values more difficult. The implication is that when social control is weakened in this manner crime will flourish.

One early influential statement of this perspective was set forth by Thomas and Znaniecki in their five-volume work published between 1918 and 1920 titled *The Polish Peasant in Europe and America.* Thomas and Znaniecki wrote about the many social changes affecting Polish peasants in this time period, including the disorganizing influences inherent in moving from simple, homogeneous, rural areas of Poland to the complex, heterogeneous, urban areas of the United States. They defined social disorganization as "a decrease of the influence of existing social rules of behavior upon individual members of the group" (Thomas and Znaniecki 1920, 2). The effectiveness of social rules (i.e., laws) derived from the individual's investment in them (i.e., attitudes favorable to laws). In the organized society, there was congruence between group rules and individual attitudes. Disorganization implied a gap between rules and attitudes, such that an individual did not feel bound by the rules and was free to disobey them (i.e., engage in crime). Viewed in this light, disorganization was a neutral term that suggested the possibility of social change, both positive and negative, and individual liberation from oppressive community standards, although it has generally been applied to studies of crime. One contribution to this literature is the recognition that crime not only is a function of economic (e.g., poverty) or cultural (e.g., subculture of violence) forces, but is intimately tied to the fundamental processes of social change.

Thomas and Znaniecki (1920, 82–83) describe two general types of disorganization that affect communities (and other organizations such as the family). The first, especially relevant to immigrant communities, causes a decay in solidarity

when the young generation adopts the values of the new community rather than the traditional "old world" values of the parent's generation (see also Schneider 1995 on "cultural anomie"). One example is the influence of American schools on immigrant children—learning a new language and values generates new desires in children for experiences that the older generation cannot provide, simultaneously reducing parental and communal ability to control their behavior (see also Young 1936). The second type of disorganization occurs among members of the old generation as values are subjected to differential interpretations over time or as rules are simply not enforced, leading to "social disharmony." For Thomas and Znaniecki, this second type of disorganization is rooted in the individual's unique temperament and may be expressed through a lack of compliance with social rules, even though there is no explicit rejection of the rules themselves. Control is thus weakened gradually through the cumulative actions of individuals who contribute to social change, even if this is not their intention.

Although not chiefly concerned with crime, Thomas and Znaniecki ([1920] 1984) did apply the idea of disorganization to the crimes of Polish immigrants. Murder among the Polish, for example, had a very different etiology in Poland as compared with the United States. Murder among the Polish in Poland was predominantly confined to one's own small communal group—murders of strangers were rare. The motive (e.g., revenge) in these cases was most often strongly felt and was preceded by long periods of brooding and a sense of violation of deeply held values. In contrast, murder in America by Polish immigrants was more likely to involve a victim who was a stranger or not well known to the offender and less likely to involve a concern with the seriousness of violating standards of the community. In the disorganized areas of America into which Polish immigrants settled, the social ties of the old country were weakened and community controls were loosened. Freed from traditional social controls, and subjected to social forces that influenced their lives in ways they could not control or sometimes understand, some Polish immigrants exhibited a "general nervousness" and "vague expectation of hostility" (Thomas and Znaniecki 1984, 286) that was virtually unknown in the old country. In such circumstances, murder could be provoked by seemingly trivial offenses partly because the immigrant feels the need to take individual action against affronts since:

> the murderer does not feel himself backed in his dealings with the outside world by any strong social group of his own and is not conscious of being the member of a steadily organized society. His family is too weak and scattered to give him a safe social refuge . . . where he could ignore outside provocations in the feeling of his social importance and security. (Thomas and Znaniecki 1984, 286)

Thus, the weakened Polish community in America was not able to solve disputes and organize behavior as it had in the old country, and this had implications for a host of social problems, including increased crime.

Despite the voluminous work of Thomas and Znaniecki, scholars most often associate the social disorganization perspective with the writings of Shaw and McKay (1931, [1942] 1969; see also Ross 1937; Kobrin 1959) on the ecological distribution of delinquency. Shaw and McKay utilized the concept of social disorganization to great effect by using quantitative data from Chicago neighborhoods to specify the role of community disorganization in producing high crime rates, other conditions (e.g., residential poverty, ethnicity) being equal. Their most important finding was that "within the same type of social area, the foreign born and the natives, recent immigrant nationalities, and older immigrants produce very similar rates of delinquents" (Shaw and McKay 1969, 158). Or, in the words of one of their critics, "nativity and nationality have no vital relationship to juvenile delinquency" (Jonassen 1949, 613). The Chicago data suggested to Shaw and McKay (though not to Jonassen) that certain areas had consistently high rates of delinquency, regardless of which immigrant groups lived there, and that as immigrant groups moved out of these areas and into better neighborhoods, their juvenile delinquency rates fell as well.

Inspired by Thomas and Znaniecki, the theoretical explanation suggested by these patterns was that neighborhoods characterized by "disorganizing" factors such as high population turnover and ethnic heterogeneity will be less likely than other neighborhoods to control the behavior of their inhabitants. According to this view, immigrants will have high crime rates only when they settle in disorganized neighborhoods, not because of biological factors or criminal cultural traditions. Thus, Lind (1930a, 1930b) stresses the value of distinguishing homogeneous but poor "ghettos" from heterogeneous, disorganized, and poor "slums." According to this view, crime might flourish in slums but not in ghettos, even though both are characterized by economic deprivation and other deleterious social conditions, because ghetto residents exert a degree of control over neighbors that is missing in disorganized slums (Lind 1930a). However, recent work on nonimmigrant black ghettos has found that homogeneous areas are not better organized for social control (Anderson 1990) and suffer from the concentration of multiple social problems (Sampson and Wilson 1995). Contemporary studies on immigrants in this tradition are lacking.

Recent theoretical work by Sampson and Lauritsen (1997) has attempted to demonstrate how macrosocial and local community-level structural forces can be combined to improve on the classic disorganization framework. Although this work developed from an attempt to explain black-white crime differences as well as the wide variation in black crime rates across structurally different

community areas, the theory can also advance our knowledge of the link between immigration and crime. Sampson and Lauritsen suggest that powerful factors (e.g., segregation, housing discrimination, structural transformation of the economy) coincide with local community-level factors (e.g., residential turnover, concentrated poverty, family disruption) to impede the social organization of inner cities (p. 340; see also Sampson and Wilson 1995). A deeper understanding of the interaction effects of these variables would enhance existing theories by better incorporating the role of the "massive social change" (Sampson and Lauritsen 1997, 340) experienced by the mostly black residents in U.S. inner cities in the 1970s and 1980s. Since the post-1965 immigration wave brought newcomers into these same structural conditions, we might expect high rates of immigrant crime during this time period. This question is the subject of the next section on empirical studies of immigration and crime.

Empirical Studies

There have been two major waves of immigration in the 20th century. The first consisted mainly of Europeans and Canadians and lasted until the mid-1920s, when congressional acts ended the flow of large numbers of immigrants into this country. The second, referred to as the "new immigration," began after 1965, peaked in the early 1990s, and was dominated by Afro-Caribbeans, Asians, and Latin Americans (Suarez-Orozco 1998; Hagan and Palloni 1998). Mexico is currently the largest single source of immigrants, both legal and illegal (Baker et al. 1998). In the following section, we consider some of the major studies of immigration and crime undertaken during both waves.

Early studies

In a recent historical study, Roger Lane (1997, 298) concluded that "homicide in the modern world has been mostly an irrationally impulsive crime, committed by young men, especially poor and aimless young men energized by frustration and anger." The same could be said about other crimes. Because immigration brings a disproportionate number of young males into the host country, we might expect immigrants to exhibit higher crime rates than the native population. In some cases, this has been the case (Gurr 1989; Monkkonen 1989). Beyond this population issue, each of the theoretical perspectives discussed previously offers compelling reasons why immigrants should have high crime rates: They usually settle in high-poverty, high-crime, disorganized communities and encounter problems of adjustment that native groups do not experience. Also, many immigrants move from rural areas into urban areas, and this also exerts a disorganizing influence on communal and familial structures (Young 1936). In fact, urban growth and immigration usually occur simultaneously and increases

in crime are often more a function of urbanization than any other factor (Handlin 1959). Yet the major finding of a century of research on immigration and crime is that immigrants display tremendous variations over time and space in their criminal involvement and, contrary to popular opinion, nearly always exhibit lower crime rates than native groups (Hagan and Palloni 1998; Tonry 1997; Ferracuti 1968; Sellin 1938).

The earliest studies on crime among the first wave of immigrants, which ended in 1924, were based on evidence that was uneven at best (Tonry 1997, 21). These studies suggested that newcomers were less likely to be involved in crime than the native born. A special report issued by the Industrial Commission of 1901 found that "foreign-born whites were less criminal than native whites," while the 1911 Immigration Commission concluded that "immigration has not increased the volume of crime" and that the presence of immigrants may have even suppressed criminal activity (Tonry 1997, 21). A review of other early immigrant-crime studies discovered that, contrary to stereotypes, most researchers did not find first-wave immigrants to be crime prone (McCord 1995), although results did vary by city (e.g., immigrants in Boston, but not in Chicago, were disproportionately involved in crime). These studies generally found that children of immigrants had crime rates higher than their immigrant parents but not higher than native-born children. This suggests that the acculturation to American life of the second generation of immigrants, and not the assumed "criminal traditions" of immigrant groups, was related to immigrant crime rates (Hagan and Palloni 1998; Sellin 1938; Sutherland 1934). Acculturation was apparently weakening the impact of traditional social controls in immigrant communities.

Even so, studies of the first wave of immigration support the conclusion that immigrants were not as highly involved in crime as natives (Abbott 1915; Lind 1930b; Taft 1936; Ferracuti 1968). A prominent report published in 1931 by the National Commission on Law Observance and Enforcement, popularly known as the Wickersham Report, noted that immigrants had lower overall crime rates than nonimmigrants, although some groups appeared disproportionately involved in specific types of crime. Mexicans in one study, for example, displayed higher rates of arrest for some violent crimes than native whites (though lower than native blacks; see Bowler 1931, 119). However, as Taylor (1931) and later Sellin (1938, 76) pointed out, these rates are "untrustworthy" because reliable Mexican population estimates were not available because of the migratory nature of that group, a situation made worse by the fact that the 1930 census figures were not yet available (Taylor 1931).

Immigrant rates were untrustworthy for other reasons as well. Another author of the 1931 report discussed ways in which prejudice against Mexicans by

criminal justice personnel worked to inflate their arrest rates (Warnshuis 1931). For example, police in a community with a large Mexican presence expressed the belief that most crimes were committed by Mexicans, while an examination of arrests revealed that only 4 Mexicans out of 252 total persons arrested were booked in 1 month; 3 of these were for "disorderly conduct," and none were convicted. In terms of official statistics, police prejudice had practical consequences when serious crimes occurred: Police would sometimes arrest "all the Mexicans they could find" (p. 283), artificially inflating Mexican crime rates.

Scholarly research and public concern about immigration and crime practically disappeared in the middle third of the 20th century. This is not surprising because immigration was low during this time period and large segments (e.g., Italians, Irish) of the first wave had been assimilated into American society.

Despite the limitations of the Mexican crime data, Taylor (1931) was able to draw several conclusions. First, the number of Mexican crimes committed was about what could be expected by the number of Mexicans in the population and the crime rates were similar to those of other groups. Second, patterns of criminal arrest varied considerably across cities and were shaped by a host of social factors, including poverty and the age and sex distributions of the immigrant population. Taylor (p. 235) also discovered that Mexican criminal involvement displayed "interesting diversity within the same locality," suggesting the need to examine structural factors differentially affecting immigrants in socially meaningful areas such as neighborhoods, rather than in larger political divisions such as cities and States. In short, there was no single conclusion that could be drawn about "Mexican crime rates" independent of the local community context.

This last point was investigated by Lind (1930a, 1930b) in work on immigration and crime in the ethnically diverse city of Honolulu, Hawaii. Following proponents of the Chicago school of sociology like Robert Park and Louis Wirth, Lind (1930a, 206–207) stressed the distinction between "slums"—ethnically diverse, unstable, and poor neighborhoods—and "ghettos," which were also economically depressed but characterized by a stable, organized, and racially homogenous group of residents (see also Whyte 1943 on "rooming house" verses "settlement" districts). Lind found that due to neighborhood heterogeneity and instability, groups in slums had multiple moral codes and were less effective in organizing families and community organizations to control the behavior of residents who were members of that group. Thus, delinquency was rare in poor but stable Japanese neighborhoods and high among native

Another study found that immigration was not related to youth violence in California, while alcohol availability was an important influence on serious crime among young males in three cities with a heavy Latino population. Finally, a study of Puerto Rican newcomers found that those living in New York City had high rates of homicide, while Puerto Ricans living elsewhere had rates comparable to native whites.

Hawaiians residing in economically better off, but disorganized, areas (Lind 1930b). In addition, Lind (1930a, 209) documented that Japanese delinquency rates also varied by the degree of neighborhood segregation and population concentration. Such findings provided empirical support for the argument that immigrant crime is a function of generic processes (i.e., disorganization) associated with urban living rather than the cultural traditions or predispositions of immigrants (see also Shaw and McKay 1969).

Other research found that compared with native groups, some immigrant groups were able to resist the disorganizing effects of slum areas to a higher degree than native groups. One study showed that although Seattle's Japanese immigrants lived in an economically deprived, high-crime, disorganized area, their "strong family and community organization" was able to keep their children's delinquency rates low (Hayner 1933, 319). This is consistent with results reported by Taft (1936, 736) showing that immigrants usually displayed lower crime rates than nonimmigrants and that variations could be explained by some combination of "adverse conditions" found in high-crime areas and the insulating effect of the "cultural values which different national groups bring." Von Hentig (1945, 793; contra Glueck and Glueck 1930) reports similar findings: Descendants of native-born groups were much more involved in serious crime than descendants of immigrants or of mixed parentage, though there were "vast diversities by States." Again, some combination of ecological conditions and cultural factors seemed important for explaining the involvement, or lack thereof, of immigrants in crime.

Contemporary studies

Scholarly research and public concern about immigration and crime practically disappeared in the middle third of the 20th century (Hagan and Palloni 1998). This is not surprising because immigration was low during this time period and large segments (e.g., Italians, Irish) of the first wave had been assimilated into American society (Lane 1997). But the post-1965 wave of immigration (largely Latino, Asian, and Afro-Caribbean) has generated renewed interest in the topic, in part because the arrival of these immigrants coincided with the rise in crime rates in this country during the late 1960s and 1970s.

Some writers have suggested that immigrant criminality was becoming an increasingly serious problem in the 1990s, as evidenced by the increase in prosecutions in U.S. district courts of noncitizens from 1984 to 1994 (3,462 cases to 10,352 cases, respectively; see Marshall 1997). Yet this finding does not take into account the tremendous increase in immigration over this time period and is largely the result of increased prosecution for drug offenses as well as for immigration offenses to which natives are not subject. As we discuss in the following section, public opinion continues to associate immigrants with crime, so it is important to review recent studies that have investigated this relationship.

The 1994 U.S. Commission on Immigration Reform compared crime in cities along the U.S.-Mexico border with nonborder cities in order to assess the impact of Mexican immigrants on crime rates. The Commission concluded that crime rates in border cities such as El Paso, Texas, were generally lower (in some cases much lower) than rates in nonborder cities. In addition to city rate comparisons, regression analysis revealed that "crime is lower on average in border areas than in other U.S. cities when the characteristics of the urban population are held constant" (1994, 20). A followup study conducted a more direct statistical test of the effect of immigration on levels of crime and found "no consistent or compelling evidence at the SMSA [Standard Metropolitan Statistical Area] level that immigration causes crime" (Hagan and Palloni 1998, 380).

Much of the recent work on immigration and crime has focused on gangs (see Bankston 1998; Marshall 1997). This is not surprising, since new immigrants often settle in urban areas characterized by a high level of gang activity, and many immigrant gangs form as a means of protection from these existing threats (Chin 1990). Although there have been numerous studies of specific immigrant gangs (cf. Du Phuoc Long 1996), there has been no research that systematically compares the level of immigrant involvement versus native involvement in gangs or suggests reasons to suspect that immigration increases gang activity (Bankston 1998).

Post-1965 studies of nongang criminal involvement of specific immigrant or ethnic groups have been rare (Martinez and Lee 1999). The studies that have been conducted tend to agree with research on the first wave: Immigrants are not disproportionately involved in crime and often are much less involved than native groups. For example, research on homicide in San Antonio found that homicide rates among Mexican males fell between those of native whites and native-born blacks and that homicide remained concentrated in poor areas of the city, regardless of whether the residents were black or Mexican over the 1940 through 1980 period (Bradshaw et al. 1998). Another study found that immigration was not related to youth violence in California, while alcohol availability was an important influence on serious crime among young males in

In the small number of studies providing empirical evidence, immigrants are generally less involved in crime than similarly situated groups, despite the wealth of prominent criminological theories that provide good reasons why this should not be the case.

three cities with a heavy Latino population (Alaniz, Cartmill, and Parker 1998). Finally, a study of Puerto Rican newcomers found that those living in New York City had high rates of homicide, while Puerto Ricans living elsewhere had rates comparable to native whites (Rosenwaike and Hempstead 1990).

A number of studies have examined homicide among several prominent ethnic groups in Miami. Although Mariel refugees were often portrayed by the media as high-rate killers, the empirical evidence demonstrated that they were rarely overrepresented as either victims or offenders, and in fact, after a short time, were much less likely to offend than Miami's established Cubans (Martinez 1997a). In addition, despite a constant influx of Latino immigrants in the 1980s, Miami's homicide rates continued to decline (Martinez 1997b). Finally, Martinez and Lee (1998) found that Miami's Haitians and Latinos were underrepresented in homicide relative to group size, while African-Americans were overrepresented, and in some cases the rate of homicide among the two immigrant groups was *lower* than that of non-Latino whites (Anglos).

Consistent with earlier studies, the criminal involvement of immigrant groups varies considerably in different cities. A good example of this variation is provided in a study of Latino homicide among El Paso's Mexicans and Miami's Cubans (Lee, Martinez, and Rodriguez 2000). Despite the two cities' similar structural characteristics (e.g., employment, poverty), Miami's Latino homicide rate was almost three times that of El Paso. In addition to city-specific characteristics such as Miami's older population, greater income inequality (see Martinez 1996 for a discussion of absolute deprivation versus relative deprivation among Latinos), and possibly greater availability of guns, other local conditions shaped the comparatively high Cuban homicide rate. For example, Cubans settled in a more violent area of the country (south Florida) than did Latinos in El Paso, and this regional context may shape each group's involvement in homicide. Wilbanks (1984; see also Epstein and Greene 1993), demonstrated that Miami's homicide trends mirror those for south Florida generally, and that this area experienced a sharp rise in homicides *prior to* the arrival of thousands of Cuban refugees in the Mariel boatlift of 1980. Thus, Miami's Latinos lived in a location experiencing higher levels of violence than El Paso's Latinos.

Just as important differences were revealed by the experience of two groups of Latinos described in the preceding research, other studies have also examined within-group differences among ethnic groups (see Hawkins 1999 for a similar strategy). Martinez and Lee (2000) investigated Afro-Caribbean homicides in Miami and found that Mariel Cuban, Haitian, and Jamaican immigrants were generally less involved in homicide than natives. Comparing the early 1980s, when these groups first began arriving in Miami in large numbers, to the late 1990s, the authors noted a strong pattern of declining violence, especially for Jamaicans and Mariels, while Haitians continuously maintained a low overall rate. As these immigrant groups grew in size and had a lower proportion of young males, homicide rates rapidly declined. This finding suggests that, contrary to key propositions of social disorganization theory, rapid immigration may not create disorganized communities but may instead stabilize neighborhoods through the creation of new social and economic institutions (see also Portes and Stepick 1993).

The general conclusions of recent research on immigration and crime echoes themes found in earlier studies. In the small number of studies providing empirical evidence, immigrants are generally less involved in crime than similarly situated groups, despite the wealth of prominent criminological theories that provide good reasons why this should not be the case (e.g., residence in disorganized neighborhoods, acculturation difficulties, conflicts between cultural codes). Furthermore, immigrant experiences vary greatly with local conditions, as illustrated by the study of Cubans in Miami and Mexicans in El Paso, and it is likely that these conditions shape criminal involvement to a larger degree than the cultural traditions of the groups themselves. We now turn to a review of persistent public opinions about immigrants and crime.

Public Opinion on Immigration and Crime

Rita Simon (1985, 1987, 1993) points out that, regardless of the time period, public opinion has almost always been negative on the issue of whether additional immigrants should be permitted to enter this country (see also Espenshade and Belanger 1998). In addition, the most recent immigrants are almost always viewed unfavorably, although public opinion eventually improves with the passage of time (Roper Reports 1995). According to Simon (1987, 47), older ethnic communities are viewed more positively than recent arrivals, even if the earlier communities were initially disliked.

For example, the Chinese and Japanese are viewed more favorably today in public opinion polls than they were in the 1920s, although they were once

Articles in other popular magazines also expressed the low public opinion in which immigrants were held. One 1923 issue of the Saturday Evening Post viewed immigrants from southeastern Europe as the "dullest and dumbest people in Europe," while another from the same year quoted eugenicist Harry Laughlin as saying "If America doesn't keep out the queer alien mongrelized people of Southern and Eastern Europe, her crop of citizens will eventually be dwarfed and mongrelized in turn."

widely subjected to discriminatory legislation and prejudicial public attitudes (Abbott 1931). Similarly, opinion was decidedly against Irish, Italians, and Poles in the 1920s, but white ethnic groups are usually better regarded today, at least in some recent surveys. A 1985 survey showed that 82 eminent social scientists believe, like most Americans for the past century and a half, that the current group of immigrants (mostly Latinos) is somehow different than previous groups: They are thought less likely to assimilate and especially dangerous to cultural values and institutions (Simon 1993).

In addition to this temporal issue, opinions about immigrants are not uniformly distributed across social groups—persons with advanced education, prestigious jobs, and higher incomes hold more favorable opinions of all immigrants regardless of country of origin (Simon 1987), although some surveys have shown that compared with other groups, blacks and Latinos hold immigrants in high esteem (Espenshade and Belanger 1998). However, small wording changes in these surveys make a big difference in the attitudes of respondents. For example, immigrants who have lived in the United States for at least 5 years are viewed much more favorably than recent arrivals. It is also telling that in surveys that place concerns about immigration in a wider context of social issues, immigration ranks much lower than other issues such as crime and jobs as well as "don't know" responses (Espenshade and Belanger 1998).

To place the current anti-immigrant sentiment in context, it is informative to examine trends in opinions about immigration and crime throughout the 20th century.[1] As Ferracuti (1968, 190; see also Short 1997; Abbott 1931) points out, in the first half of the 20th century, "popular opinion often expressed the view that migrants were responsible for a large fraction of the crime rate," despite considerable empirical evidence that they were not. Statements like the following from a turn-of-the-century New York City police commissioner were typical: "[S]omething like 85 out of one hundred of our criminals should be found to be of exotic origin" (Simon 1985, 70–71). This commissioner viewed Italians as particularly dangerous "malefactors"

(Simon 1985, 71) and promoted the view that the French and Belgians organized white slavery rackets.

The idea of criminal immigrants was not a product of the 20th century; a series of articles in the *New York Times* published in 1880 was also concerned with criminal Italians, a particularly "clannish race," and the prototypical "bad Irish boy [who was] about as unwholesome a product as was ever reared in any body politic" (Simon 1985, 186). Note also that the 1892 Democratic Party platform nodded approvingly at legislation designed to keep the United States from "being used as a dumping ground for known criminals" from Europe and China (Simon 1985, 18). Such views had long been reflected in scholarly writings as well. For example, an article by William Jeffrey in an 1893 issue of the *Journal of Political Economy* also blamed immigrants for increasing crime rates (Simon 1985).

This image appeared in prominent periodicals throughout the first wave of immigration. A 1915 editorial in *North American Review* called for a restriction of immigration on the grounds of the "criminal and economic worthlessness" of southern European immigrants (Simon 1985, 71). Articles in other popular magazines also expressed the low public opinion in which immigrants were held. One 1923 issue of the *Saturday Evening Post* viewed immigrants from southeastern Europe (e.g., Italy, Greece) as the "dullest and dumbest people in Europe," while another from the same year quoted eugenicist Harry Laughlin as saying "If America doesn't keep out the queer alien mongrelized people of Southern and Eastern Europe, her crop of citizens will eventually be dwarfed and mongrelized in turn" (Simon 1985, 85). The assumed criminal nature of immigrants is a common theme in these writings, with authors claiming that immigrants were filling penitentiaries and insane asylums faster than the native born. During the 1920s, Italians, Jews, Poles, Russians, Greeks, and others were commonly portrayed as criminals.

Contemporary anti-immigrant sentiment is often promoted by organized interest groups that accuse Third World nations of exporting their excessive numbers to the United States (Simon 1993, 69). And as in the past (see Sellin 1938), current legislation—such as California's Proposition 187 and the U.S. Congress' 1996 Immigration Reform bill—has not relied on scholarly research, even though it has been justified on the grounds that it will stem the tide of "criminal aliens," who are a growing presence in the criminal justice system (U.S. Commission on Immigration Reform 1994; Scalia 1996). This imagery has profound consequences for recent immigrants who must confront these enduring negative stereotypes, are singled out for discrimination, and are consistently viewed in public opinion polls as burdens to society (Simon 1987; Roper Reports 1995).

One consequence of prejudicial attitudes about immigrants can be seen in the rise in reported hate crimes against Asian-Americans that occurred in some communities in the 1980s (Zinsmeister 1987). Citizens in areas in which Asian immigrants were settling believed that these immigrants "come from jungle communities," eat dogs, and represent unfair labor competition because they work "day and night" (Zinsmeister 1987, 8). Gore Vidal echoed this last concern in a 1986 article in the *Nation,* claiming that unless the United States and Canada cooperated with each other and limited the number of Asian newcomers, Americans would end up as "mere entertainment" for Asians (p. 350). These ideas, when added to the traditional allegation that immigrants are a highly criminal population, facilitate a resentment against immigrants that may increase the probability of hate crimes.

Public opinion and immigrant homicide in three cities

As a point of departure, we focus on homicide victim rates, not offending rates, in the following section. Although much of our discussion has centered on offender motivation, the immigration-crime link should be evident in the following analyses of homicide victim rates given the closeness of the victim and offender relationship in most cases. The Federal Bureau of Investigation's 1995 Uniform Crime Report (UCR) notes that in 75 percent of homicide cases cleared with an arrest, the victim and offender had *some* type of prior relationship (spouse, family member, lover, friend, neighbor) or previous contact such as an acquaintance or coworker (U.S. Department of Justice, Federal Bureau of Investigation 1997, table 2.12). Thus, in most cases, the "stranger element" is absent even after considering the proportion of unknown offenders, which could fall in any category of familiarity or individual characteristic (age, race, gender). Furthermore, the vast majority of all homicides were intraracial (white on white, black on black) and speaks to neighborhood residential patterns and segregation that have long been part of American society. Most persons tend to live in areas in which other residents have similar attributes, including race and ethnicity.

In addition, historical and contemporary studies also report that many killings were victim precipitated and that the event initially arose out of an argument (as one example) that turned lethal (Wolfgang 1958; Martinez and Lee 1998). The closeness of this relationship is further illustrated in a recent city-level homicide study of black and white victim rates. Ousey (1999, 410) reports that the effects of structural conditions on homicide victim and arrest rates are substantially similar and provide the same conclusions. This is not surprising

given the spatial proximity between victims and offenders, most of whom reside in the same neighborhood and share points of economic comparison (Anderson 1990). In fact, both are also products of communities weakened by a host of social problems that impede the ability of institutions to maintain order and provide opportunities to those most in need and typically more active in crime in general and violence specifically.

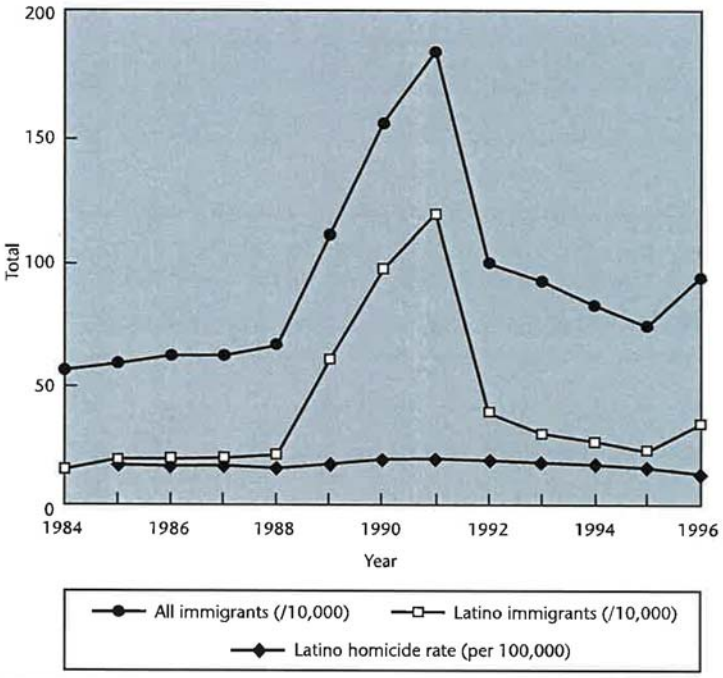
Finally, data issues also encourage us to focus on victim data. Homicides are rare events relative to any other type of violent crime (e.g., armed robbery, aggravated assault). Disaggregating these already scarce cases by ethnicity thins an already shallow pool even more, encouraging researchers to use all available homicide victim incidents. Furthermore, to the best of our knowledge, no large-scale datasets are available or, if available, are not fully complete that break down offending behavior for Latinos. Critics might suggest that our findings in the following section are city specific. We admit there is a chance that our findings might differ from those of others, but again, most of our cases involve members of the same group, and in all instances, the contours of victim and offender data followed each other closely. Therefore, victimization rates represent a valuable, albeit not entirely satisfactory, proxy for group-specific offending behavior.

Because immigration adds to the country's total population—and especially the population of young, unattached males—it will also likely increase the absolute volume of crime. The key question is whether immigrants contribute a disproportionate amount of crime beyond what we would expect from native populations with similar demographic characteristics.

Despite claims by pundits and writers that high levels of crime are an unavoidable product of immigration, scholars rarely produce any systematic evidence of this recently reemerging social problem (Beck 1996; Hagan and Palloni 1998; Sampson and Lauritsen 1997). As Hagan and Palloni note, because immigration adds to the country's total population—and especially the population of young, unattached males—it will also likely increase the absolute volume of crime. The key question is whether immigrants contribute a disproportionate amount of crime beyond what we would expect from native populations with similar demographic characteristics. As exhibit 1 shows, the recent wave of immigrants (predominantly Latino) appears not to have affected rates of Latino homicide.[2]

Although theory and popular wisdom suggest that immigrants should be disproportionately involved in crime, the trends reported in exhibit 1 show that Latino homicide rates remained remarkably stable despite a massive increase in Latino immigrants in the early 1990s. Similar findings showing no systematic relationship between immigration and crime rates have been reported for the early 20th-century wave of immigration as well (Hagan and Palloni 1998, 369). In the final section of this essay, we explore the relationship between public opinion, local context, and immigrant crime by reporting previously unpublished group-specific homicide victim data.

Exhibit 1. Total and Latino immigration counts with the national Latino homicide victim rate



Sources: Immigration figures are reported by the U.S. Department of Justice, Immigration and Naturalization Service (1997, 1987); homicide rates are based on data from the U.S. Department of Health and Human Services, National Center for Health Statistics (1998).

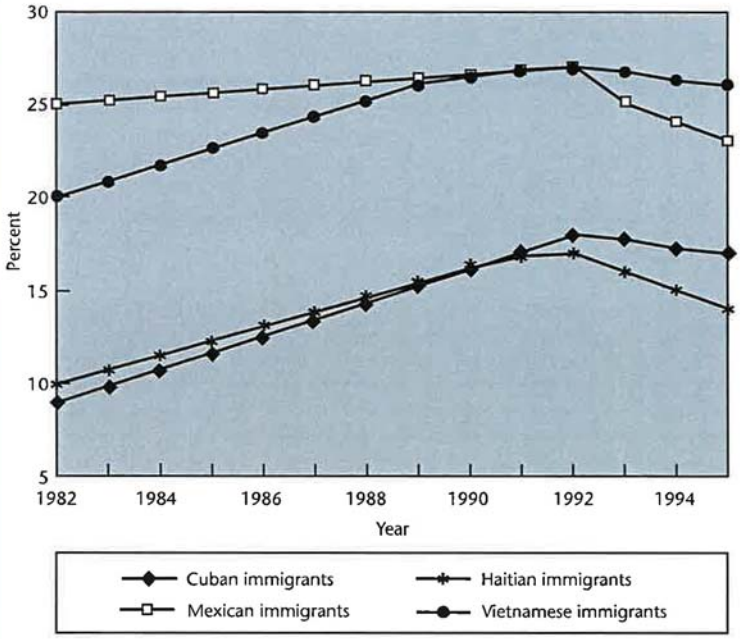
In this section, we present empirical findings for the latest available time period from an ongoing study of immigrant homicide victimization in three cities, El Paso, Miami, and San Diego. Little evidence exists that violence among Latinos in these cities, almost one-third foreign born, is persistently and systematically influenced by high levels of immigration. Despite this finding, public opinion has remained negative on the issue of whether specific immigrant groups have "been a good thing for the country," which suggests that these immigrant groups face widespread prejudice and, presumably, difficulties adjusting to American society (Roper Reports 1995). As exhibit 2 shows, only a small percentage of Americans (in no case more than 27 percent) hold favorable opinions of the immigrants listed. Although positive attitudes toward Mexicans and Vietnamese are twice as high as those about Cubans and Haitians, they nevertheless still are expressed by a relatively small proportion of the U.S. population. By comparison, European immigrants (i.e., English, Irish, and others from the first wave of immigration) are generally viewed at least twice as favorably as the recent immigrant groups. For example, 45 percent of respondents in 1995 rated the Irish favorably (see Roper Reports 1995).

Before turning to an examination of group-specific homicide rates, as these might be influenced by unfavorable public attitudes, we first present total homicide rates for the three cities involved. We have commented throughout this essay on the importance of local conditions in shaping immigrant crime, and it is important to place the homicide involvement of immigrant groups in their local context. Since victim data are more fully complete than offender information, and given that roughly 20 to 25 percent of homicide cases are neither cleared with an arrest nor have an identified assailant, victim rates are used as proxies for homicides. Offender rates, at least in these cities, follow the same racial and ethnic contours of victimization data, allowing us to remain reasonably confident that our argument applies to all homicide cases.

The Mariel contribution to local homicide was concentrated for a few years and then greatly declined, dropping below Miami's total and Latino rates and even below national rates for cities of Miami's size.

As exhibit 3 shows, homicide rates per 100,000 persons in Miami are much higher than in San Diego and El Paso, so we would expect that this finding would shape immigrant homicide. Therefore, in the exhibits that follow, the homicide rates of immigrant groups will be compared with relevant local and national rates. In many of the exhibits, immigrant rates mirror both city-specific and national trends.[3]

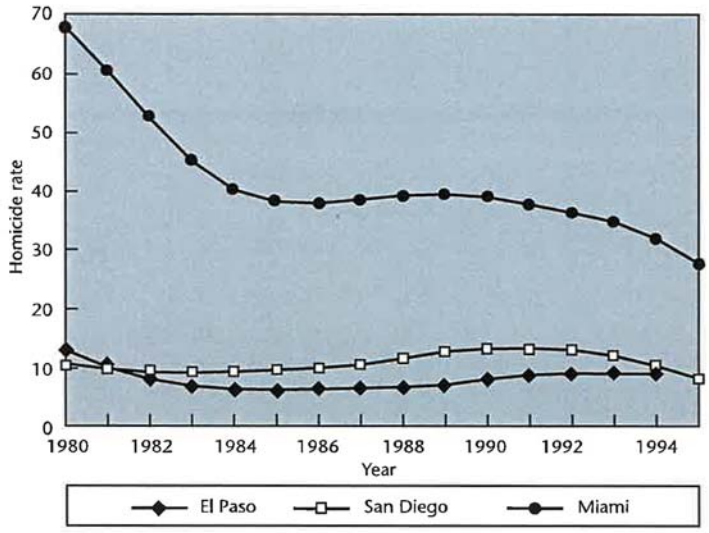
Exhibit 2. Percent of U.S. population sample who believe selected immigrant groups have been a "good thing for the country"



Source: Roper Reports 1995, 113–114.

There are probably some unique instances for Latinos and others in which changes in public attitudes and victimization rates shift over time. Take, for example, the highly publicized 1980 Mariel boatlift in which almost 125,000 people fled Cuba, with most ending up in south Florida (see Portes and Stepick 1993). The popular media linked these refugees to already record-high crime rates in Miami, labeled them as escorias, or scum, released from Cuban prisons, and generated a great deal of negative publicity about the group as a whole (see Aguirre, Saenz, and James 1997). As we see in exhibit 4, the Mariel homicide victim rate did exceed the Miami city total from 1980 to 1986. However, the Mariel Cubans never surpassed the African-American level of victimization

Exhibit 3. Total homicide victim rates for three cities

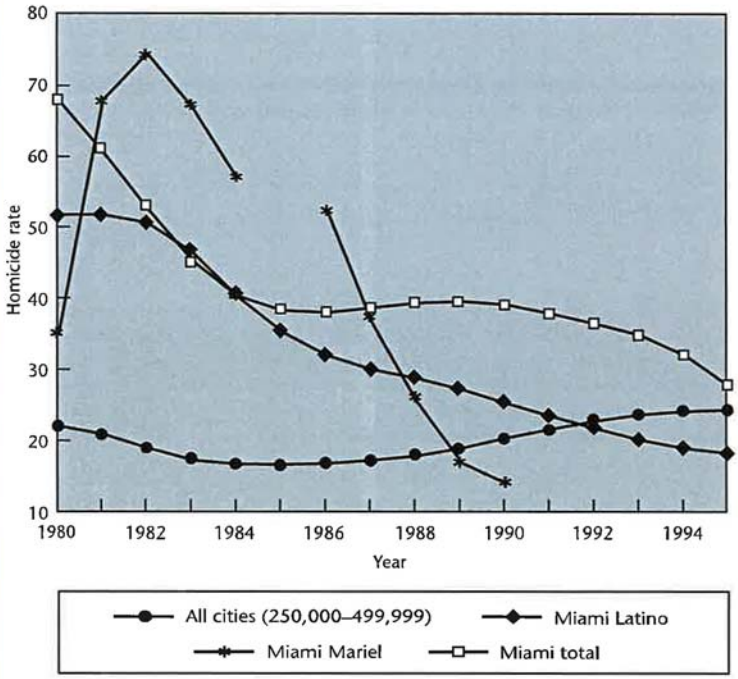


Sources: City of El Paso Police Department homicide investigations records 1980–95; City of Miami Police Department homicide investigations records 1980–95; City of San Diego Police Department homicide investigations records 1980–95. Population estimates are based on data from the U.S. Bureau of the Census (1990).

and apparently landed in Miami during an overall period of urban strife (see Martinez 1997 for data collection material). This is not to suggest that the Mariels were *not* overinvolved in crime. In fact, they did contribute, like others, to the high crime levels in Miami, but at the same time they were not the persistent and high-crime-prone immigrants portrayed in the media. The Mariel contribution to local homicide was concentrated for a few years and then greatly declined, dropping below Miami's total and Latino rates and even below national rates for cities of Miami's size.

The negative attitudes toward Cubans persisted much longer than the initial negative publicity. Using the work of Portes and Stepick (1993) as a starting point, we note that the percentage of the national population agreeing that "Cubans were a

Exhibit 4. Miami's Mariel homicide victim rates in local and national context

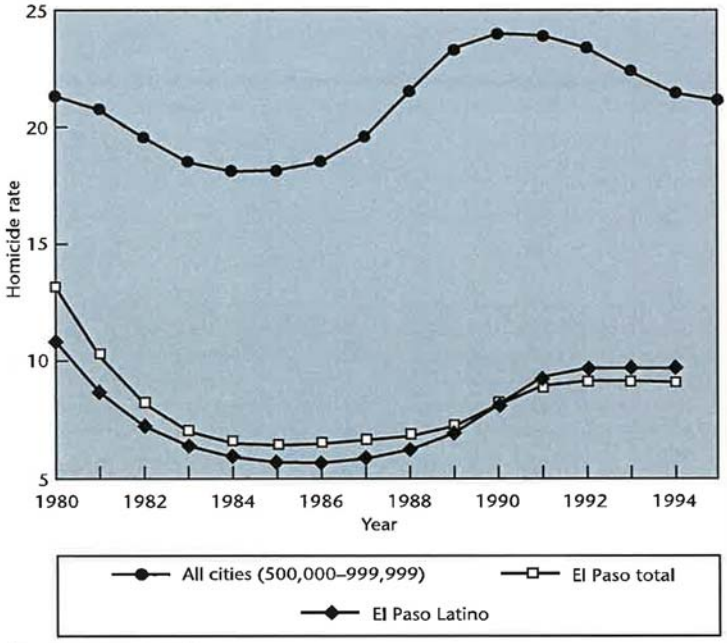


Sources: City of Miami Police Department homicide investigations records 1980–95. Population estimates are based on data from the U.S. Bureau of the Census (1990); population estimates for Mariels are based on data discussed in Portes, Clark, and Manning (1985); homicide rates for all cities are based on data from the U.S. Department of Justice, Federal Bureau of Investigation (1980–95).

good thing for the United States" was below 10 percent and rose only to about 17 percent for Cubans in 1995, according to the latest available Roper Reports (see exhibit 2). This was despite the substantial decline of Mariel violence in Miami, where most of the refugees settled.

The same negative attitudes hold true in cities on the U.S.-Mexico border. Public opinion toward Mexicans never reflects the persistently low Latino (Mexican) victim rates in the two largest border cities—El Paso and San Diego.

Exhibit 5. El Paso's Latino homicide victim rates in local and national context

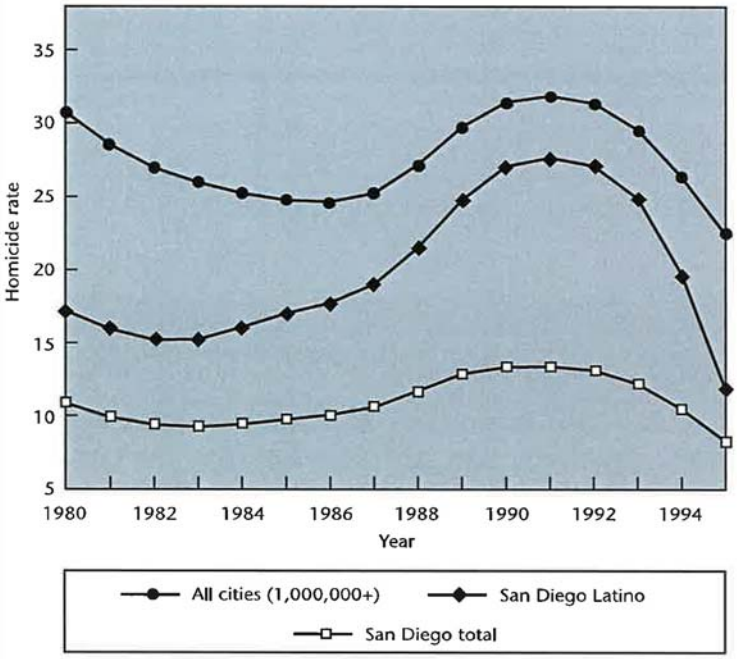


Sources: City of El Paso Police Department homicide investigations records 1980-95. Population estimates are based on data from the U.S. Bureau of the Census (1990); homicide rates for all cities are based on data from the U.S. Department of Justice, Federal Bureau of Investigation (1980–95).

The homicide rate in these two cities never approached that of other cities of similar size (see exhibits 5 and 6). Exhibit 2 shows how national attitudes toward Mexicans remained flat, then actually declined, notwithstanding the low Mexican-origin homicide rates.

The connection between low victim rates and public perception becomes most apparent in the case of Miami's Haitians, as shown in exhibit 7. Miami is not only a major destination for Afro-Caribbeans, it is home to the second largest Haitian community in the United States. Yet, again, public attitudes toward

Exhibit 6. San Diego's Latino homicide victim rates in local and national context

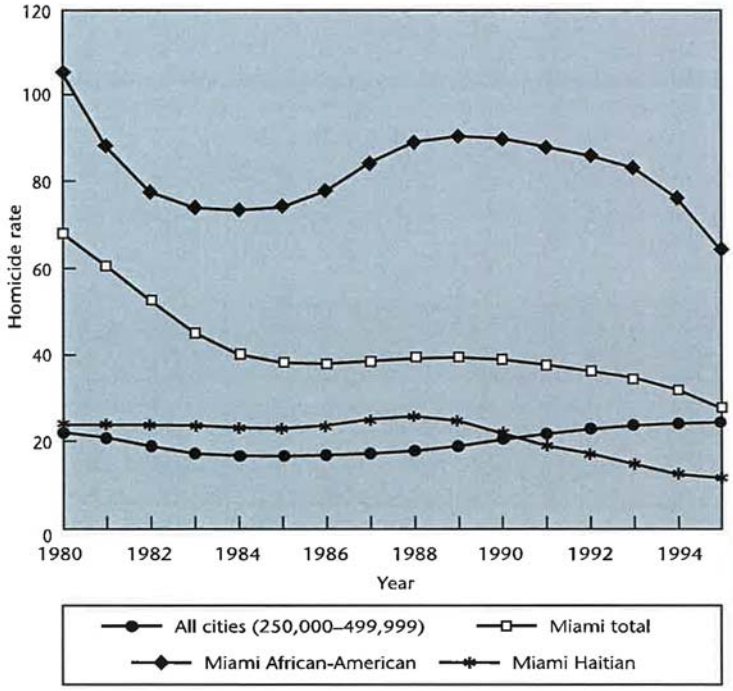


Sources: City of San Diego Police Department homicide investigations records 1980–95. Population estimates are based on data from the U.S. Bureau of the Census (1990); homicide rates for all cities are based on data from the U.S. Department of Justice, Federal Bureau of Investigation (1980–95).

Haitians have rarely been favorable (see exhibit 2) and are actually worsening, despite Haitians having homicide rates lower than those of *any* ethnic group in Miami and lower than the average in all small cities in the 1990s.

Finally, we examine homicide rates among San Diego's Asian immigrants (predominantly Vietnamese) in exhibit 8 and compare these rates with public attitudes about Vietnamese. At first glance, the level of homicide among Asians appears to ebb and flow; however, the rate never rises above single digits (7.5 to 8.0 per 100,000 Asians in San Diego). In fact, Asian rates fall below San Diego's total rate and well below the rates of cities with more than

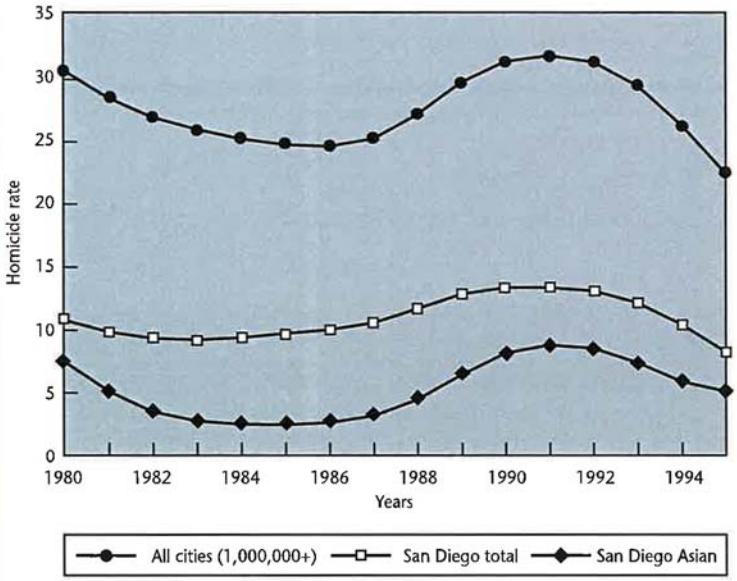
Exhibit 7. Miami's Haitian homicide victim rates in local and national context



Sources: City of Miami Police Department homicide investigations records 1980–95. Population estimates are based on data from the U.S. Bureau of the Census (1990); homicide rates for all cities are based on data from the U.S. Department of Justice, Federal Bureau of Investigation (1980–95).

1 million residents. Despite this, positive attitudes toward the Vietnamese have not materialized to any appreciable degree over the period of this study (see exhibit 2). Like the other immigrant groups, Asians continue to be viewed unfavorably despite their comparatively low involvement in crime, at least as measured by homicide rates. Apparently, national attitudes toward ethnic groups are not in line with crime rates (if we assume a favorable attitude to low crime groups) but are instead probably shaped by ethnic or racial stereotypes, not empirical data.

Exhibit 8. San Diego's Asian homicide victim rates in local and national context



Sources: City of San Diego Police Department Homicide Investigations records 1980–95. Population estimates are based on data from the U.S. Bureau of the Census (1990); homicide rates for all cities are based on data from the U.S. Department of Justice, Federal Bureau of Investigation (1980–95).

Conclusion

We can draw a number of broad conclusions from this review of immigration and crime in 20th-century America. First, there are good theoretical reasons to believe that immigrants should be involved in crime to a greater degree than natives. For example, immigrants face acculturation problems that natives do not, and immigrants tend to settle in disorganized neighborhoods characterized by deleterious structural conditions such as poverty, ethnic heterogeneity, a preponderance of young males, and possibly more criminal opportunities in the form of gangs. Also, the cultural codes of immigrants may conflict with the legal codes constructed by native groups. Despite these and other reasons to expect high levels of immigrant crime, the bulk of empirical studies conducted

over the past century have found that immigrants are usually underrepresented in criminal statistics. There are variations to this general finding, but these appear to be linked more to differences in structural conditions across areas where immigrants settle than to the cultural traditions of the immigrant groups. Local context appears to be the central influence shaping the criminal involvement of both immigrants and natives, although in many cases, immigrants seem more able to withstand crime-facilitating conditions than native groups.

We offer some plausible explanations for why immigrant crime rates are lower than expected. Contrary to a tradition of social science research on urban violence dating back to the pioneering work of Thomas and Znaniecki (1920), contemporary immigration might not create disorganized communities but instead stabilize neighborhoods through the creation of new social and economic institutions. Immigrants reside in highly impoverished communities but provide a buffer to further neighborhood decline through higher levels of intact and extended families and regular contact with the world of work. Moore and Pinderhughes (1993) note that immigrant Latinos, as a whole, have relatively high rates of employment, but in lower blue-collar occupations (e.g., operators, fabricators, and laborers) and in the informal economy (e.g., street vendors, domestics). Thus, immigrants are characterized more as the working poor than as chronically unemployed, and they work in areas dominated by small business owners and the self-employed.

To illustrate, in some areas, immigration has influenced the economy by increasing marginal jobs for newcomers in the secondary sector and contributed to labor market competition with other urban minority groups (see Aponte 1996). Thus, although immigration is linked to heightened poverty, many scholars describe a pool of foreign-born residents who are typically working, often at subsistence levels, and who are routinely attached to the labor market, although at times through the informal economy (see Introduction in Moore and Pinderhughes 1993).

Furthermore, Wilson (1987, 1996) and Sampson (1987) also describe how economic attachment is a key factor in maintaining family structures, stabilizing community institutions, and decreasing social problems in urban neighborhoods. The consequence for crime and violence is that although immigrant poverty is just as widespread as in African-American communities, its impact on violence might be lower than predicted because of greater attachment to the economy through low-paying but relatively steady jobs in ethnic enclaves, simultaneous with lower rates of family disruption (see also Portes 1996). Although speculative, this is at least one plausible account for our finding that immigrant crime is lower than expected. We invite future elaborations on this topic.

Much more research is needed on immigration and crime, particularly on the latest wave of immigration, to advance our knowledge beyond the general conclusions provided by this essay. Like others, we suggest that future research should be more open to the idea that immigration can be a positive influence on communities to suppress crime. We agree with Hagan and Palloni (1998, 382) that researchers should:

> place the priority on finding ways to preserve, protect, and promote the social capital that . . . immigrants bring to their experience in the United States, rather than overemphasize issues of crime and punishment.

Academic study of immigrants that is limited to gangs and crime can serve only to promote the impression that immigrants are a crime-prone group—an image that the empirical research of the past 100 years does not support. In sum, this review suggests that native groups would profit from a better understanding of how immigrant groups faced with adverse social conditions maintain relatively low levels of crime.

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Notes

1. The fear that too many crime-prone immigrants are entering the United States is a contemporary concern, in addition to other reasons for anti-immigrant sentiment. In a national bestselling book promising "common sense about America's immigration disaster," a writer at *Forbes* and *National Review* magazines states: "[I]mmigration is not the *only* cause of crime. It may not even be the major cause of crime. But it is a *factor*" (Brimelow 1996, 182; emphasis in original).

Hailed as a "non-fiction horror story of a nation that is willfully but blindly pursuing a course of national suicide" (Jesse E. Todd, Jr., cited in the front matter of Brimelow 1996), such alarmist writings assume a strong link between immigrants and crime while providing little empirical data. In fact, Brimelow (p. 182) claims that "there has been no serious academic study of the impact on crime" of the post-1965 wave of immigration.

2. Although not evident in exhibit 1, the Latino homicide rates fluctuated from slightly more than 16 per 100,000 in 1985 to a high of 18.6 in 1991, and declined to a low of 12.4 in 1996 (U.S. Department of Health and Human Services 1998). The massive increase in legal immigration did not increase Latino homicide rates, which actually declined somewhat throughout the 1990s. Homicide rates for blacks and whites also declined after the peak years of immigration. Black rates fell from a high of 41.6 per 100,000 in 1991 to 29.9 in 1996, while white rates over a similar period dropped from 4.4 to 3.5. Thus, homicide rates declined for all groups after the arrival of large numbers of immigrants.

3. All rates are per 100,000 group-specific persons as measured by the 1990 census.

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Immigration and violence: The offsetting effects of immigrant concentration on Latino violence ☆

Ben Feldmeyer *

Department of Sociology, University of Tennessee, 901 McClung Tower, Knoxville, TN 37996, USA

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ABSTRACT

Despite longstanding interest in the effects of immigration on American society, there are few studies that examine the relationship between immigration and crime. Drawing from social disorganization theory and community resource/social capital perspectives, this study examines the effects of Latino immigration on Latino violence. Data on violence (i.e., homicide, robbery, and Violent Index) and the structural conditions of Latino populations are drawn from the California Arrest Data (CAL), New York State Arrest Data (NYSAD), and U.S. Census data for approximately 400 census places during the 1999–2001 period. Findings suggest that immigrant concentration has no direct effect on Latino homicide or Violent Index rates but may reduce Latino robbery. Immigration also appears to have multiple, offsetting indirect effects on Latino violence that work through social disorganization and community resource measures. These results suggest that (1) immigrant concentration does not contribute to Latino violence and may even reduce some forms of violence, (2) immigration simultaneously stabilizes and destabilizes structural conditions in Latino populations, and (3) it is useful to examine both the direct and indirect effects of immigration on crime.

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1. Introduction

The social consequences of population movement and immigration have interested social science researchers for nearly a century. However, there are few studies on immigration and crime and particularly few studies that examine contemporary patterns of Latino immigration and violence. Following periods of high European immigration to the U.S. in the early 20th century, researchers gave considerable attention to the effects of immigration on crime and other social problems (Shaw and McKay, 1942; Taft, 1933). This interest waned in later decades but resurfaced near the end of the 20th century, largely in response to (1) new waves of Latino immigration from Central and South America (Healey, 2006) and (2) conflicting views among the American public and in sociological research and theory about the effects of immigration on violence.

The current study contributes to this resurgent body of research on immigration and crime by examining how immigrant concentration influences Latino violence – an issue which has important political and social implications for immigrant populations as well as critical substantive implications for social science theory and research. First, examining the immigration–crime relationship holds key political and social implications for the more than 27 million Latino immigrants and 45 million Latinos residing in the U.S. (U.S. Census Bureau, 2008). Despite a scarcity of immigration–crime research, public opinion has consistently linked contemporary trends in immigration to social problems and particularly to increased crime and violence. U.S. citizens have expressed increasingly salient fears that immigrants are harming the American way of life, taking job opportunities from citizens, and contributing to violence and crime – with more than 70 percent of Americans believing that

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* Fax: +1 865 974 7013.
E-mail address: bfeldmey@utk.edu

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immigration has substantial crime-generating effects (Horowitz, 2001; NYT, 2006; Muller, 1993; see reviews in Lee et al., 2001; Martinez and Lee, 2000). These fears of immigration-generated "zones of lawlessness" (Horowitz, 2001: 8) have contributed to structural barriers that inhibit immigrant-assimilation and upward mobility (particularly for Latino populations) through increased social conflict and barriers with native-born residents, limited education and employment opportunities, and increased use of legislation aimed at reducing crime by restricting immigration and immigrant rights (Clark, 1998; DeJong and Madamba, 2001; Martinez and Lee, 2000).

Second, addressing the relationship between immigration and violence has important substantive implications for a broad set of disciplines within the social sciences interested in the consequences of population movement and migration. As immigration trends continue and the foreign-born (especially Latino foreign-born) account for an increasingly larger share of the U.S. population, it is particularly important that social scientists identify the social consequences of immigration and its effects on crime and violence. By 2050, population projections indicate that nearly 20 percent of the U.S. population will be foreign-born (compared to 12 percent in 2005) and 30 percent of the U.S. will be Latino (Passel and Cohn, 2008). Thus, identifying the impacts of these dramatic population shifts is a pressing question for sociologists, criminologists, and other social science researchers and one that holds key implications for influential social–ecological theories of crime – particularly in light of the competing positions and debate on the effects of immigration on crime found in sociological and criminological research and theory. On one hand, traditional versions of social disorganization theory suggest that immigration may increase violence by destabilizing communities and contributing to structural sources of violence – e.g., community levels of poverty, mobility, and heterogeneity (see reviews in Lee et al., 2001; Martinez, 2000, 2002; Martinez and Lee, 2000; Shaw and McKay, 1942). On the other hand, research on immigrant-assimilation, ethnic entrepreneurship, and cultural transmission suggests that immigration may stabilize communities and reduce violence by providing protective community resources and strengthening social institutions and social capital networks (Lee et al., 2001; Light and Gold, 2000; Martinez, 2002; Moore and Pinderhughes, 1993; Portes and Stepick, 1993; Velez, 2006).

In addition to this disagreement, prior research on immigration and crime has faced several other limitations. First, while there is a shortage of empirical studies that examine the effects of immigration on crime in general, there is an absence of research in particular that examines how contemporary patterns of Latino immigration influence Latino rates of violence across localities – which is particularly concerning considering the rapid growth in the Latino and Latino immigrant populations and the increased application of political and criminal justice policies toward Latino groups based on assumed immigration–crime links. Unfortunately, such analyses have rarely been possible because most official sources of crime data provide offense counts for only a small set of racial categories (e.g., white, black) and do not provide Latino offense counts across cities or other macrosocial units. Second, prior aggregate-level studies of immigration and crime have typically employed small neighborhoods and census tracts (Lee et al., 2001; Nielsen et al., 2005) or large cities/MSAs (e.g., Martinez, 2000; Reid et al., 2005) as study units. While these studies have greatly advanced research on immigration and crime, it is also important to utilize data at other levels of analysis (particularly those that incorporate both large and small locales) to extend understandings of immigration–crime relationships. Third, prior research has generally been limited to a specific offense, like homicide (Lee et al., 2001; Martinez, 2000, 2002) or drug violence (Martinez et al., 2004). Finally, past research has typically assumed direct causal effects of immigrant concentration on rates of crime when *indirect* causal links may be more theoretically plausible (see reviews in Reid et al., 2005; Stowell, 2007). That is, rather than influencing violence directly, explanations for immigration–crime relationships generally suggest that immigration influences violence indirectly, working through other structural conditions (e.g., poverty, population heterogeneity, employment rates, family structure).

The current study addresses the competing sociological perspectives on immigration and crime and contributes to the small but growing body of immigration–crime research by examining *the relationship between Latino immigration and rates of Latino violence* across 396 California and New York census places. Using structural equation modeling techniques, I examine the direct, indirect, and total effects of immigrant concentration on levels of Latino violence, paying particular attention to whether immigration effects are mediated by other structural factors – i.e., poverty, heterogeneity, family structure, and employment rates. By examining these indirect effects, this analysis addresses the competing perspectives on immigration and violence, referred to here as the *social disorganization* and *community resource* perspectives, and can identify whether (1) immigrant concentration increases Latino violence as argued by traditional versions of the social disorganization perspective, (2) immigrant concentration decreases Latino violence as argued by the community resource perspective, or (3) immigration has competing and perhaps offsetting effects on Latino violence, which supports both perspectives.

2. Competing views of the immigration–violence relationship

2.1. Violence-generating effects of immigration

In the same way public perception has often linked immigration and crime (Horowitz, 2001; Muller, 1993; see review in Martinez and Lee, 2000), some sociological and criminological theories argue that immigration is linked to interpersonal violence. For example, demographic explanations of crime suggest that immigration may lead to higher rates of violence because Latino immigrants tend to be part of a crime-prone demographic group – young, single males with relatively few local family attachments (Hagan and Palloni, 1999; Martinez and Lee, 2000; Pew Hispanic Center, 2006). Criminological theories also suggest that the immigration process may contribute to violence by creating strain and limited opportunities among immigrant

populations as these groups face language and assimilation barriers and as they move into highly disadvantaged communities with few resources, below-average schools, and low-quality housing (Lee et al., 2001; Martinez, 2002; Velez, 2006). Additionally, structural–cultural explanations of crime suggest that these harsh conditions may further contribute to crime to the degree that they foster subcultural adaptations that provide further opportunities for violence (Anderson, 1999; Sampson and Wilson, 1995). Thus, even if they are not particularly "crime-prone," immigrants and immigrants' children may be pulled into violence as they are exposed to multiple disadvantages and criminal opportunities in their communities.

Perhaps the most common theoretical link between immigration and violence, however (and thus the focus of this paper), is drawn from social disorganization theory and the Chicago School of sociology (e.g., Lee et al., 2001; Martinez and Lee, 2000; Nielsen et al., 2005; Reid et al., 2005; Stowell, 2007). Often credited with the theory, Shaw and McKay (1942: 18) examined crime and other social problems in the early 20th century Chicago communities and found that crime was concentrated in the "Zone of Transition" – a mixed-use area in Chicago that experienced high levels of immigration, poverty, and population heterogeneity and turnover. Social disorganization theory explains that the structural conditions in high-immigrant areas contribute to crime by disorganizing and destabilizing neighborhoods – i.e., by limiting residents' abilities to communicate and to recognize and achieve common goals, like crime control (Bursik, 1988; Sampson and Groves, 1989; Shaw and McKay, 1942). Social disorganization theory suggests there is nothing inherently criminal about immigrants, but rather argues that immigrant concentration indirectly increases violence by contributing to the criminogenic structural conditions (i.e., poverty, population heterogeneity, and residential mobility) that destabilize communities and inhibit community cohesion, communication, and social control.

Applying this perspective to contemporary Latino immigration patterns, social disorganization theory suggests that the immigration process could have destabilizing and possibly crime-generating effects on Latino communities. Latino immigrants often have few resources, little wealth, and low incomes when they enter the U.S., which may increase disadvantage and poverty levels in immigrant communities (Healey, 2006; Light and Gold, 2000; Portes and Rumbaut, 2006). Latino immigration contributes to population turnover and residential mobility as new residents enter a community. Arrival of new immigrants from around the world with diverse backgrounds may also contribute to greater racial/ethnic heterogeneity and greater mixing of different languages and norms (Thomas and Znaniecki, 1927). Thus, even if Latino immigrants themselves are not particularly crime-prone, the traditional arguments of social disorganization theory suggest that Latino immigration may structurally disorganize Latino communities, inhibit cohesion and communication among residents, and lead to greater violence.

2.2. Violence-reducing effects of immigration

In contrast to the arguments discussed above, a small but growing body of sociological research suggests that immigrant concentration and especially Latino immigration does not lead to increased violence because (1) Latino immigrants do not commit many crimes and (2) immigration may have stabilizing, violence-reducing effects on Latino populations.

Research shows that Latinos and Latino immigrants have surprisingly low rates of violence (and other social problems) considering their disadvantaged economic and social positions (Martinez, 2002). Several studies by Martinez and colleagues show that violence levels are consistently low among highly-immigrant Latino and Haitian populations and are often only slightly higher than non-Latino white levels of violence (Martinez and Lee, 1998, 2000; Martinez, 2002; Martinez and Nielsen, 2006). Research by Butcher and Piehl (1998) indicates that immigrants and especially recent immigrants are much less likely to be incarcerated and institutionalized than native-born U.S. residents with similar demographic characteristics. Additionally, Hagan and Palloni (1999: 630) find that foreign-born Latinos are less likely to be incarcerated than whites, leading them to conclude that the immigration–crime link is "misleading, to the extent of constituting mythology."

Several studies also suggest that immigration does not have destabilizing, violence-generating effects on Latino communities as argued by the social disorganization perspective, but may instead reduce violence among Latino populations (Lee et al., 2001; Martinez, 2000; Martinez et al., 2004; Nielsen et al., 2005; Reid et al., 2005; Sampson et al., 2005). For example, Lee et al. (2001) show that having high proportions of new immigrants had no significant effect on Latino homicides in San Diego and Miami neighborhoods and may have actually lowered Latino homicide rates in El Paso neighborhoods. Martinez (2000) reports that the effect of immigration on homicide varies across types of homicide but concludes that immigration has no significant effect on either Latino or total homicide rates. A key study by Sampson and colleagues (Sampson et al., 2005) indicates that immigration reduced violence in Chicago neighborhoods, noting that the odds of violence were four-fifths lower for individuals in neighborhoods in which 40 percent of the residents were immigrants compared to individuals in communities with no immigrants. Also, Reid et al. (2005) show that the percentage of foreign-born residents from Latin America does not contribute to city-level rates of violence or property crime.

Researchers offer several reasons why immigration may not contribute to greater Latino violence. First, they note that Latino immigrants are not a particularly crime-prone group and generally do not have criminal motives when they enter the country. Gaining entry to the U.S. can often be a difficult and strenuous process that requires determination, work, risk, and sacrifice, such as leaving behind family members and friends. Thus, Latino immigrants who choose to come to the U.S. are generally a self-selected set of highly-motivated, dedicated individuals who tend to have higher education levels, greater financial resources, and more human capital than the average individual from their homeland (González, 2000; Hagan and Palloni, 1999; Tonry, 1997). Additionally, Latino immigrants are typically accepting of conventional institutions (e.g., work, family, church) and mainstream American values and often enter the U.S. specifically in search of employment and legitimate success opportunities (Tonry, 1997).

Second, referred to here as the *community resource perspective* on immigration and crime, research on immigrant-assimilation, ethnic entrepreneurship, and cultural transmission argues that immigration does not destabilize communities. Instead, this perspective suggests that immigration increases community stability and resources and strengthens social cohesion among Latino populations (Lee et al., 2001; Martinez and Lee, 1998; Moore and Pinderhughes, 1993; Portes and Stepick, 1993; Velez, 2006). Drawing from the social capital perspective (Coleman, 1988), researchers explain that Latino enclaves often have strong kinship bonds and social ties that are based largely on the shared heritage, language, and cultural background of residents. These networks act as support systems for new immigrants and provide resources and social capital to help protect community residents against crime, violence, and effects of disadvantage and poverty that may be present in Latino communities (Light and Gold, 2000; Healey, 2006; Martinez and Lee, 2000).

Researchers argue that immigration helps strengthen these social ties and support systems by reinforcing the common heritage and traditions that bond members of Latino enclaves. Each new wave of immigrants provides Latino communities with a fresh infusion of shared heritage and culture that reinforces traditional values and norms of the Latino community and strengthens cohesion among residents (Martinez, 2002). Additionally, kinship ties and support-networks are further strengthened due to frequent "chain migration," in which new immigrants move into communities near family members and friends who can provide social support and assistance with childcare, employment, finances, and housing (Healey, 2006; Martinez et al., 2004; Portes and Rumbaut, 2006).

Research also suggests that Latino immigration strengthens conventional institutions, like work and family, and may attract new resources into Latino communities (Martinez and Lee, 1998, 2000; Portes and Stepick, 1993). For example, because they are an attractive source of low-wage labor to employers, immigrants are likely to attract jobs to Latino communities and increase the employment potential for community residents. Also, traditional family structures and Latino attachment to the church and other conventional social institutions may be strengthened as immigrants enter the community and reinforce these values (Martinez and Lee, 1998). Thus, immigration may not destabilize communities, but may instead reduce Latino violence by strengthening social support-networks and social institutions.

2.3. The current study

In sum, there is a scarcity of research addressing the immigration–violence relationship and a particular shortage of research on Latino immigration and crime. Additionally, prior research and theory offer conflicting views about this relationship. A thorough review of prior research reveals an absence of empirical studies that have simultaneously examined these differing views (i.e., social disorganization and community resource perspectives) by examining the *direct and indirect* effects of immigration on violence (for a noteworthy exception, see Stowell, 2007). Thus, it is unclear whether immigration creates violence by destabilizing Latino communities or whether immigration has stabilizing, violence-reducing effects on Latino populations.

The current study makes two major contributions to existing research on immigration and crime. First, I examine the direct effects as well as the indirect and total effects of immigration on Latino violence. Second, in doing so this analysis assess two or possibly three competing views on immigration and violence – (a) immigration increases Latino violence by destabilizing structural characteristics of Latino communities, as argued by the social disorganization perspective; (b) immigration reduces Latino violence by stabilizing Latino populations, increasing community resources, and strengthening conventional institutions, as argued by the community resource perspective; or (c) immigration has multiple offsetting effects on Latino violence, providing support for both social disorganization and community resource arguments (see Fig. 1).

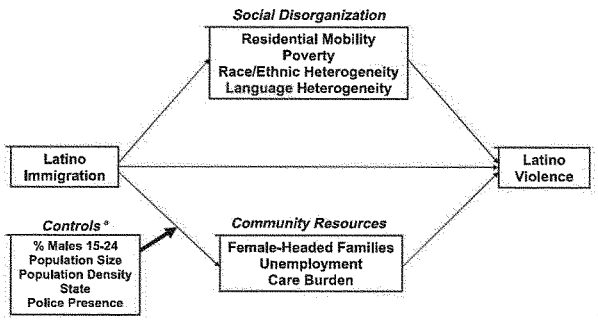

Fig. 1. Structural equation model for the direct and indirect effects of immigration on Latino violence. *Note:* Social disorganization, community resource, and control variables are all measured, entered, and predicted separately. Latent factors are not used to predict Latino violence. *The full set of control variables is used to predict all Latino violence and mediating variables (e.g., poverty, residential mobility, unemployment).

3. Data and methods

To address these issues, information was drawn from two sources. First, data on Latino violence were drawn from California Arrest Data (CAL) and New York State Arrest Data (NYSAD) for the 1999–2001 period. Second, information on the immigration patterns and structural conditions of Latino populations was obtained from 2000 U.S. Census data.

The unit of analysis is the incorporated *census place*, which includes a variety of non-overlapping geographic areas that are tracked by the U.S. Census Bureau – ranging from small villages and boroughs with as few as several hundred people to large cities with several million residents. The use of census places is well-suited for this analysis because – first, they are large enough to provide reliable measures of Latino violence and structural characteristics (using some selection criteria) but small enough to preserve a large sample size for the analysis. Second, census places included in the analysis vary widely in size and structural characteristics (e.g., rates of immigration, violence, poverty) and are less likely to face restricted distributions in measures of interest compared to the small neighborhood-level data (e.g., census tracts) (Lee et al., 2001; Nielsen et al., 2005) or large census places (e.g., MSAs, large cities) (Martinez, 2000; Reid et al., 2005) examined in prior research. In particular, large study units (MSAs, large cities/census places) tend to have similar structural characteristics and may provide less variation in measures needed to examine the relationships among immigration, structural characteristics, and violence compared to census place-level data that include widely varying locales, ranging from small villages and towns to the large places and cities examined in prior studies (Hipp, 2007; Shihadeh and Shrum, 2004).[1] Third, census places provide an untapped unit of analysis for addressing immigration–violence relationships and for addressing whether prior findings on immigration and violence are generalizable to other geographic spatial units.

Several selection criteria were used to provide greater reliability in the measures of Latino violence and social structure. The sample includes only the census places with at least 10,000 total residents and 1000 Latino residents in the year 2000, resulting in a final sample of 396 California and New York census places.[2]

It is worth noting that although this sample does not provide complete coverage of foreign-born Latinos across the U.S., these census places offer some of the best available data and geographic coverage of contemporary patterns of Latino violence and immigration. Specifically, this sample of census places accounts for more than five million foreign-born Latino residents, which represents nearly 84 percent of all foreign-born Latinos in California and New York and more than 35 percent of all foreign-born Latinos in the U.S. in 2000 (U.S. Census Bureau, 2008). I acknowledge that these data may not reflect patterns of Latino immigration and violence in states with different immigrant profiles (e.g., Texas, Florida) or in states that are not traditional immigrant gateways. However, at worst, this sample describes the relationships between Latino immigration and violence across California and New York places, which account for more than one-third of all year 2000 foreign-born Latinos in the U.S., and uses a relatively untapped aggregate unit of study for examining immigration–violence relationships.

3.1. Dependent variables

Data on Latino violent crime were drawn from the California Arrest Data (CAL) and the New York State Arrest Data (NYSAD), which provide individual-level information on demographic characteristics (age, race/ethnicity, gender), arresting agency, and offense type and seriousness for all recorded arrests and citations in California and New York.[3] These data are particularly well-suited for this project because they identify arrests of "Hispanic" offenders, unlike the Uniform Crime Reports

[1] A concern with combining small and large census places in one sample is that the largest census places, which account for larger shares of the Latino population and violence, are given equal weight as small towns and villages. To address this concern, I replicated the analyses using three approximately equal subsamples of small (<30,000), medium (30,000–59,999), and large places (>60,000) based on natural breaking points in population size that preserved adequate sample sizes for statistical analysis (results not shown). Due to reduced statistical power, the direct and indirect effects of immigration on violence were not significant for all subsamples but were in the same directions as those reported here (excluding the effect of immigration on poverty, which was negative for the subsample of large places).

[2] Arrest data were available only for incorporated census places (excludes census designated places) in California and New York. In California, cities and towns may be considered for incorporation if they meet the state requirement of 500 registered voters. To be incorporated in New York, villages must have at least 500 people and at least 100 people per square mile, and city incorporation requires a special act of the state legislature (U.S. Census Bureau, 1994). New York City is treated as five separate boroughs due to the large size and distinct boundaries of each borough (i.e., county divisions across boroughs). Several smaller census places meeting the selection criteria were excluded from the analysis because arrest data were not available at the agency-level (14 in California; 13 in New York). All of these missing census places had fewer than 150,000 residents, only 2 had more than 100,000 residents, and 22 of the 27 places had approximately 50,000 or fewer residents.

[3] Individual arrest counts provided in the CAL and NYSAD data are collected by nearly 1500 law enforcement agencies throughout New York and California and reported on a monthly basis in the *California Monthly Arrest Citation Register* (MACR) and the New York *eJusticeNY* system as part of the National Uniform Crime Reporting program. Thus, law enforcement agencies follow UCR guidelines when creating and reporting these counts (e.g., only report the most serious offense for each arrest) but include more detailed information on offense type and offender race/ethnicity (e.g., "Hispanic" identity) than is available in the FBI's UCR data. Arrest counts are compiled and monitored by the California Bureau of Criminal Information and Analysis and the New York State Division of Criminal Justice Services. Final tabulations of arrest records, disaggregated by offense and a limited set of demographic characteristics are used to create state reports on crime and are reported to the Federal Bureau of Investigation for use in tabulating annual Uniform Crime Report figures for California and New York (California Department of Justice, 2006; New York State Division of Criminal Justice Services, 2008).

(UCR) and many other sources of official crime data.[4] Latino arrest counts were drawn from the data, aggregated to the census place-level, and combined with year 2000 Latino population figures to create the dependent measures: Latino arrest rates/100,000 for homicide, robbery, and the Violent Index.[5] I calculated the dependent measures using 3-year averaged arrest figures for the 1999–2001 period to add stability to violence measures and to ensure that the data include adequate arrest counts for statistically rare offenses (e.g., homicide). All dependent measures received square root transformations to reduce positive skewness and impose normal distributions.[6]

It is important to acknowledge that arrest data are not a perfect proxy for Latino offending because (1) they are likely to underreport true levels of violence and (2) variations in Latino arrest rates across census places may in part reflect differential enforcement – i.e., a greater/lesser willingness to arrest Latino suspects in places with large immigrant populations. Due to data limitations, I was unable to establish the precise degree to which Latino arrest patterns in California and New York census places correspond to broader Latino offending patterns. However, I attempt to minimize problems with underreporting in arrest data by focusing on index-violent offenses – which are considered "serious" offenses and are more reliably reported across demographic groups and structural contexts (LaFreeet al., 1992; Steffensmeier and Haynie, 2000). Additionally, as discussed below, all models control for the relative size of the police force to account for potential differential enforcement patterns across census places.

3.2. Independent variables

The independent variables include structural characteristics of Latino populations across California and New York census places drawn from 2000 U.S. Census data – Summary Tape Files 1 and 4. The key explanatory variable in this analysis is *Latino immigration*, which is measured as the percentage of the Latino population in a census place in 2000 that is foreign-born and entered the U.S. between 1990 and 2000. It is important to note that this measure includes both documented (authorized) and undocumented (unauthorized) Latino immigrants. Although, it is possible that undocumented and documented immigration have differing effects on violence, such information is not readily available at the census place-level. Thus, addressing this question is beyond the scope of the current project but is an important avenue for future research on immigration and violence.[7]

Drawing from social disorganization theory and prior research (Kubrin and Weitzer, 2003; Shaw and McKay, 1942; Silver, 2000), the following variables are used as proxies of social disorganization: *poverty*, *residential mobility*, *racial/ethnic heterogeneity*, and *language heterogeneity*.[8] *Poverty* is measured as the percentage of Latino residents in a census place with income levels below the poverty line. *Residential mobility* reflects the percentage of Latino households that experienced a change in residents during the 1995–2000 period. To measure *racial/ethnic heterogeneity*, a 3-group (black, white, and Latino) entropy index score was calculated for each census place (Reardon and Firebaugh, 2002). The entropy index (E) is a multi-group measure of the diversity of a geographic area, calculated as:

$$E = \sum_{m=1}^{M} \pi_m \ln(1/\pi_m)$$

where π_m is the proportion of people in race/ethnicity m (e.g., proportion black) and M is the total number of racial/ethnic groups. E has a minimum value of 0 when a census place has no diversity and is composed entirely of one racial/ethnic group

[4] The CAL and NYSAD data use the term "Hispanic," whereas U.S. Census data and research on race/ethnicity and crime commonly use the terms "Hispanic" and "Latino" interchangeably to refer to individuals with origins from Spanish-speaking countries of Central and South America and the Caribbean (e.g., Dominican Republic, Cuba) or those individuals who self-identify as "Latino," "Hispanic," "Hispanic-American," etc. (U.S. Census Bureau, 2008). The term "Latino" is used here to identify men and women with origins from these countries rather than the term "Hispanic," which is a more contested term among Latinos and has been linked by some to a "history of U.S. colonialism" (see Feagin, 2000; Golash-Boza, 2006; Rodriguez et al., 2000: 512).

[5] Violent Index figures include Latino arrest counts for homicide, rape, robbery, and aggravated assault. I acknowledge that Latino immigration may also influence violent offending for the total population and for other race/ethnic groups than Latinos. Identifying such effects is an important avenue for future research. However, due to the complexity of the analysis and the limited attention given to Latino violence in prior literature, the current analysis focuses specifically on the effects of immigration on Latino violence.

[6] The California arrest data were obtained from the California Bureau of Criminal Information and Analysis (1999–2001). The New York State Arrest Data were obtained from the New York State Division of Criminal Justice Services (1999–2001). In both states, offender's race is determined primarily based on self-identification (California Department of Justice, 2006; New York State Division of Criminal Justice Services, 2008). However, identification practices may vary across jurisdictions when offenders do not self-identify their race. The NYSAD and CAL data treat "Hispanic" ethnicity as a separate racial/ethnic category from "white," "black," and other race/ethnic groups but do not separate Latinos into more refined racial/ethnic categories, such as "white-Hispanics," "black-Hispanics," or the various subgroups of "Hispanics" (e.g., Mexican, Puerto Rican, Cuban). Thus, both "black" and "white" Latinos are included among the "Hispanic" arrest group. Additionally, although log transformations are frequently used to transform positively skewed measures of violence in aggregate-level studies of crime, square root transformations provided more normal distributions in the dependent measures (for similar treatments, see Phillips, 2002).

[7] It is important to acknowledge that this measure of immigration is likely to underestimate undocumented or unauthorized (illegal) Latino immigration. As illustrated in prior research, adjusting for potential undercounts in Latino immigration (particularly for unauthorized immigrants) is both a complex and highly debated issue (for further details, see Bean et al., 2001; Passel et al., 2004). Thus, there is a need for future research that provides straightforward adjustments or corrections for immigration undercounts in U.S. Census data that can be applied to aggregate analyses of immigration and crime.

[8] Although they do not directly measure organizational processes in Latino communities (e.g., organizational participation, collective efficacy, and social networks and ties), the measures of poverty, residential mobility, and population heterogeneity (racial/ethnic and language heterogeneity) are well established in sociological literature and theory as structural sources of social disorganization and community instability. Thus, the present analysis is limited to examining how immigration affects the macro-structural sources of social disorganization, and in turn, how these factors influence Latino violence.

and a maximum value of 1 when blacks, whites, and Latinos are equally represented in a census place. Similarly, to address the diversity of languages within census places and potential communication barriers among residents, I calculated a 2-group (English-only and Spanish-only speakers) entropy index using the above formula to measure census place *language heterogeneity* – which has a minimum value of 0 when all residents speak either English or Spanish and a maximum value of 1 when a census place has an equal share of English-only and Spanish-only speakers.[9]

The decision to use race/ethnicity-specific measures of social disorganization merits some discussion. Social disorganization theory describes a general process in which the characteristics of place (not necessarily the characteristics of one race/ethnic group) influence social control and crime (Shaw and McKay, 1942; Kubrin and Weitzer, 2003). However, prior ecological studies of crime suggest using race/ethnicity-specific predictors at high levels of aggregation (e.g., MSAs, census places) and for highly segregated racial/ethnic populations, as is the case here (Harer and Steffensmeier, 1992; Shihadeh and Steffensmeier, 1994; see McNulty, 2001).[10] Thus, the selection of predictors follows the practices of prior studies of race and crime that use race/ethnicity-specific measures of disadvantage and disorganization (e.g., Lee et al., 2001; Martinez, 2000; Phillips, 2002).

Drawing from research that suggests that immigration strengthens Latino communities and institutions (e.g., Latino employment, family structure), community resource explanations of immigration and crime are addressed using measures of *male unemployment, female-headed families with children*, and the *care burden* in Latino populations. *Unemployment* is measured using the percentage of Latino males in each census place who were at least 16 years old and were either unemployed or not in the labor force in the year 2000. *Family structure* is measured using the percentage of Latino families that had female heads and children under 18 years of age. The Latino *care burden* (also known as the *dependency ratio* in demographic research – e.g., see Rosenberg, 1985; Schwartz, 2006) is included as an alternative measure of community social support and strain and is measured as the sum of children (0–12) and elderly (65+) per 100 Latino adults (21–64), where higher values reflect a greater potential burden or strain of caring for children and elders among the adult Latino population.[11]

Several other structural measures that may affect the immigration–violence relationship are also included in the analysis. To control for the relative size of the *young-male population*, models include a measure of the percentage of males ages 15–24 in the Latino population. *Population size* is measured using the logged total population for each census place. *Population density* reflects the number of persons per square mile of land. A dummy variable is included to indicate the *state* in which a census place is located, where 1 refers to New York and 0 refers to California. Additionally, I control for the *police presence* based on the number of full-time law enforcement officers/1000 residents in the census place.[12]

The predictors were selected primarily based on theoretical considerations and practices of prior literature. However, potential collinearity problems were also considered, and correlations among measures were closely examined when selecting independent variables. The final set of independent variables are correlated at or below $r = .50$, with only one exception ($r = .63$ between female-headed families and percent in poverty) (see Table 1). Preliminary analyses revealed that VIF values were below 4.0 for multivariate models of Latino violence using the full set of predictors. Additionally, estimated effects did not substantially change when predictors were added to models one at a time. Based on these observations, multicollinearity does not appear to be a serious concern for the analysis.

3.3. Analytic procedures

Structural equation models (SEMs) are used to examine the impact of immigration on Latino violence. SEMs are particularly well-suited for this analysis because, unlike ordinary least squares regression, they allow easy estimation of direct, indirect, and total effects of immigration on violence. Thus, structural equation models provide the most straightforward and efficient methods for empirically examining mediating effects in the immigration–violence relationship and for addressing the social disorganization and community resource arguments described in the previous sections.

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[9] Both the racial/ethnic heterogeneity measure and the language heterogeneity measure were divided by their maximum values of approximately 1.099 and 0.693, respectively, to make E values range from 0 to 1 and to make results more easily interpretable. An alternative measure of racial/ethnic heterogeneity which accounts for the total population (including white, black, Latino, and "other race" categories) was used in supplemental analyses. Additionally, an alternative language heterogeneity measure that includes a more diverse set of languages (including individuals who spoke only English, Spanish, Indo-European languages, Asian and Pacific Island languages, and "other" languages) was also examined. Results obtained with these alternative measures were nearly identical to those presented in the current analysis.

[10] Examination of Latino residential isolation indices (P^*) for census places in preliminary analyses indicates that Latinos are highly segregated from whites and that a randomly chosen Latino resident has a .53 probability of being isolated from whites. To more fully exhaust the data, measures of Latino residential mobility were replaced with measures of total residential mobility in supplemental analyses, which showed slightly stronger violence-reducing effects of immigration on Latino violence (results not shown). Thus, the models presented here reflect the more conservative estimates of the immigration-violence relationship.

[11] Although there are many potential measures of "community resources," the community resource measures used here are drawn from prior research and theory which focuses in particular on the potential stabilizing effects of immigration on employment and family structure (e.g., Light and Gold, 2000; Martinez, 2002). Several alternative measures of community resources (i.e., % Latinos employed in professional or managerial occupations, % Latino homeowners, and Latino median income) were also considered in preliminary analyses but were excluded from the final models because they provided few significant effects and were highly correlated with other independent and mediating variables.

[12] Counts of law enforcement officers were drawn from year 2000 agency-level Uniform Crime Report data (U.S. Department of Justice, Federal Bureau of Investigation, 2004), which were aggregated to the census-place level using the 2000 Uniform Crime Report Crosswalk file (U.S. Department of Justice, Bureau of Justice Statistics, 2002) and then combined with total population figures from the 2000 U.S. Census to create the police presence measure.

Table 1
Correlations of dependent and independent variables for the Latino population.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Latino homicide rate	1.000															
2. Latino robbery rate	0.396	1.000														
3. Latino Violent Index rate	0.447	0.693	1.000													
4. % Foreign-born 1990–2000	−0.051	−0.161	−0.115	1.000												
5. % Males age 15–24	−0.053	−0.197	−0.086	0.305	1.000											
6. Total population	0.215	0.257	0.076	0.030	−0.079	1.000										
7. Population density	0.166	0.291	0.060	0.160	−0.080	0.389	1.000									
8. State (New York = 1)	−0.207	−0.376	−0.674	0.184	0.110	0.006	0.085	1.000								
9. Police presence	0.025	0.043	−0.113	0.164	0.148	0.181	0.286	0.473	1.000							
10. % in poverty	0.233	0.094	0.240	0.138	0.288	0.047	0.110	0.202	0.305	1.000						
11. Residential mobility	−0.105	−0.121	−0.020	0.181	0.338	−0.042	−0.244	0.145	0.149	0.361	1.000					
12. Racial/ethnic heterogeneity	0.185	0.137	0.136	0.132	0.091	0.389	0.106	−0.004	0.104	0.106	0.046	1.000				
13. Language heterogeneity	0.293	0.279	0.400	0.358	0.060	0.128	0.325	−0.311	−0.102	0.215	−0.297	0.260	1.000			
14. % Female-headed families	0.131	0.051	−0.015	−0.146	0.162	0.073	0.110	0.402	0.456	0.634	0.386	0.099	−0.177	1.000		
15. % Unemployed males	0.181	0.059	0.040	−0.172	0.159	0.047	0.077	0.259	0.185	0.502	0.001	0.118	0.067	0.426	1.000	
16. Care burden	0.235	0.198	0.441	−0.240	−0.192	0.026	−0.119	−0.234	−0.164	0.408	0.070	0.130	0.233	0.206	0.235	1.000

Fig. 1 displays the key elements of the structural equation models. The straight arrow going from left to right between immigration and violence reflects a direct effect of immigration on Latino violence. The model also includes seven mediating or indirect effects between immigration and Latino violence. The short arrows branching out from the immigration variable show that immigration is used to predict the four social disorganization measures and the three community resource measures. In turn, each of these mediators is simultaneously included as a predictor of Latino violence, controlling for the effects of other mediators and predictors. Thus, the structural equation models test whether immigration influences violence indirectly through its effect on each of these seven structural factors. Additionally, the short, thick arrow extending from the control variable box (Fig. 1, bottom left) indicates that the control measures are included as predictors for each mediator and the Latino violence measures. The structural equation models outlined in Fig. 1 are estimated using AMOS, and the results are reported below.

Based on the theoretical arguments described earlier, I interpret observed relationships as effects of immigration on Latino resources, structural stability, and violence. However, because the analysis relies on cross-sectional data, these relationships may also reflect a tendency for Latino immigrants to move into communities with higher or lower levels of disorganization, resources, and violence. Longitudinal data on Latino immigration, social structure, and violence are not currently available for all measures used here. Thus, I am unable to fully tease out these relationships. I address this issue to some degree by using immigration measures based on the 1990–2000 period to predict year 2000 measures of community disorganization, resources, and violence. However, detailed longitudinal data and analyses are required to more thoroughly address this noteworthy limitation.

4. Results

4.1. Bivariate results

Table 1 displays the bivariate correlation matrix for measures of Latino violence and proxies of structural position. These bivariate results are used as an initial method to identify links between Latino immigration, mediating variables, and Latino violence. Turning first to the immigration–violence relationship, Table 1 shows that immigration is negatively correlated with all three forms of violence (homicide, $r = -.05$; robbery, $r = -.16$; Violent Index, $r = -.12$). Although these relationships are fairly modest and are not as strong as the more sizable correlations between violence and other structural measures, they

indicate that Latino rates of violence and especially robbery tend to be lower in census places with higher levels of Latino immigration. Thus, to the extent that there is an effect, the bivariate results indicate that immigration may reduce Latino violence.

The bivariate results in Table 1 also provide support for the social disorganization perspective on immigration and violence. Specifically, Latino immigration is positively correlated with Latino poverty ($r = .14$), residential mobility ($r = .18$), racial/ethnic heterogeneity ($r = .13$), and language heterogeneity (.36), which suggests that areas with more Latino immigration tend to be more structurally disorganized. In turn, all of the social disorganization measures (except for residential mobility) are positively correlated with the three Latino violence measures, which indicates that Latino violence is higher in census places with greater Latino poverty and greater racial/ethnic and language diversity. Excluding the effect of residential mobility, these findings are consistent with social disorganization arguments about the immigration–violence relationship and suggest that immigration indirectly increases violence by structurally destabilizing Latino communities.

The bivariate results also provide support for community resource arguments. Table 1 shows that census places with high levels of Latino immigration have relatively fewer Latino families with single mothers ($r = -.15$), smaller percentages of unemployed Latino males ($r = -.17$), and lower care burden scores ($r = -.24$). These structural factors are all positively related to Latino violence (excluding the family structure–Violent Index relationship, $r = -.015$), indicating that Latino violence is higher in places with relatively more Latino unemployment, female-headed families with children, and a greater care burden. Thus, the bivariate findings are also consistent with community resource arguments and suggest that immigration indirectly reduces violence by decreasing strains and strengthening social institutions in Latino communities.

4.2. Multivariate results: structural equation models

I turn next to the question of whether the bivariate findings on immigration and violence persist net of controls and when the social disorganization and community resource arguments are tested simultaneously – using SEMs to estimate the direct, indirect, and total effects of immigration on Latino violence. To simulate the OLS models of immigration and violence used in prior research, I first present the direct effects of immigration (and controls) on Latino violence – in which immigration is modeled simply as one of many structural predictors of violence. I then extend these findings by describing the indirect effects of immigration on violence that work through social disorganization and community resource factors as well as the total effects (combined direct and indirect effects) of immigration on violence.

4.2.1. Direct effects

The last three columns of Table 2 show the direct effects of the independent, control, and mediating variables on Latino violence. First, examining the control variables, the results show that the percentage of young males in the Latino population has no significant effect on Latino homicide. In contrast, the results indicate that Latino robbery and Violent Index rates are lower in places with relatively more Latino males in the population. Population size has positive effects on Latino homicide and robbery, indicating that Latino violence is higher in places with larger populations. Latino robbery rates are higher in census places with greater population density. Latino robbery and Violent Index arrest rates are higher in places with a relatively larger police presence. Additionally, the control for state shows that Latino violence rates are lower in New York than in California census places.[13]

Turning next to immigration, Table 2 shows that immigration has a negative direct effect on Latino robbery ($B = -0.14$, $p < .01$), indicating that Latino robbery rates are lower in census places with higher concentrations of Latino immigrants, net of the other predictors. Immigration also has negative direct effects on Latino homicide ($B = -0.01$) and Violent Index ($B = -0.03$) rates. However, Table 2 shows that these coefficients are not statistically significant using standard significance levels. Thus, findings suggest that immigration has no direct effect on Latino homicide or Violent Index rates but reduces Latino rates of robbery.

4.2.2. Indirect effects

The social disorganization and community resource positions suggest that immigration may influence violence through other structural factors, which cannot be seen when one tests only for direct effects. Thus, the following paragraphs describe the indirect and total effects of immigration on Latino violence to address these theoretical perspectives and to provide a richer picture of the immigration–violence relationship.

Table 2 (first 7 columns) shows that several of the control variables are related to the social disorganization and community resource measures. The presence of young males in the population contributes to Latino social disorganization measures (residential mobility and poverty). In addition, the percentage of young males contributes to higher rates of Latino female-headed families and male unemployment but lowers Latino care burdens. Population size contributes to greater racial/ethnic

[13] Pooling census places from California and New York into one sample may mask important differences in the Latino populations and the immigration-violence relationship across states. To address this issue, I allowed the "state" measure to interact with "immigration" effects on violence and on the mediating variables (results not shown). None of the interaction terms were significant. I also replicated the current analysis separately for California and New York census places. The results from the analysis of California places were similar to those shown here. Due to the smaller number of New York census places (and reduced statistical power), the New York analysis produced few statistically significant effects. However, the effects of immigration on violence were in the same direction as those shown for the combined data.

Table 2
Standardized coefficients for the structural equation models predicting Latino violence.[a,b]

	Social disorganization				Community resource			Homicide	Robbery	Violent index
	Residential mobility	% Poverty	Racial/ethnic heterogeneity	Language heterogeneity	Female-headed families	Unemployment	Care burden			
Variables										
% Foreign-born 1990–2000	0.12*	0.01	0.11*	0.38***	−0.32***	−0.32***	−0.15**	−0.01	−0.14**	−0.03
Controls										
% Male 15–24	0.26***	0.25***	0.08	0.02	0.19***	0.23***	−0.13*	−0.07	−0.18***	−0.10**
Total population	0.07	0.01	0.41***	0.01	0.02	0.03	0.07	0.14**	0.11*	−0.02
Population density	−0.32***	0.06	−0.07	0.32***	0.05	0.10*	−0.11*	0.01	0.18***	0.03
State (New York = 1)	0.05*	0.07	−0.05	−0.37***	0.28***	0.26***	−0.18***	−0.27***	−0.45***	−0.78***
Police presence	0.14**	0.22***	0.05	−0.09	0.33***	0.05	−0.02	0.03	0.17**	0.20***
Social disorganization										
Residential mobility	–	–	–	–	–	–	–	−0.12*	0.05	0.03
% In poverty	–	–	–	–	–	–	–	0.16**	0.05	0.25***
Racial/ethnic heterogeneity	–	–	–	–	–	–	–	0.06	0.03	0.05
Language heterogeneity	–	–	–	–	–	–	–	0.14*	0.16**	0.11*
Community resource										
% Female-headed families	–	–	–	–	–	–	–	0.15*	0.12*	0.08*
% Unemployed males	–	–	–	–	–	–	–	0.09	0.06	0.04
Care burden	–	–	–	–	–	–	–	0.02	−0.03	0.13***
Fit index										
CFI	–	–	–	–	–	–	–	0.90	0.90	0.90
R^2	0.21	0.16	0.18	0.37	0.35	0.18	0.12	0.20	0.36	0.65

$N = 396$.

[a] Homicide, robbery, and Violent Index are predicted in three separate models. The effects of immigration and the controls on the mediators are reported only once based on the homicide model because they did not change across models.

[b] Correlations are estimated for all exogenous predictors to account for unobserved shared association among predictors and to improve model fit.

* $p < .05$.
** $p < .01$.
*** $p < .001$.

heterogeneity but has no effects on other social disorganization variables or community resource measures. Population density has mixed effects on both the social disorganization and community resource measures – specifically, density contributes to Latino unemployment and language heterogeneity but appears to reduce residential mobility and the Latino care burden. Census places with a greater police presence tend to have higher levels of Latino poverty, residential mobility, and female-headed families. The results also indicate that levels of unemployment and female-headed families are greater for Latino populations in New York than in California, while language heterogeneity and the care burden have significantly lower values in New York census places.

Turning next to the social disorganization perspective, the results indicate that immigration contributes to Latino social disorganization measures, net of other controls (Table 2, columns 1–4). Specifically, the percentage of new immigrants in the Latino population increases Latino residential mobility ($B = 0.12$, $p < .05$), racial/ethnic heterogeneity ($B = 0.11$, $p < .05$), and language heterogeneity ($B = 0.38$, $p < .001$). The standardized coefficients in Table 2 reveal that immigration contributes particularly strongly to the language heterogeneity of census places but has no noticeable effect on poverty rates among Latino populations. These results support traditional social disorganization perspectives on immigration and suggest that immigration destabilizes Latino communities by increasing the population turnover and heterogeneity (both racial/ethnic and language heterogeneity) of census places.

In turn, several of the social disorganization measures influence Latino violence. The last three columns of Table 2 show that poverty has significant, positive effects on Latino homicide ($B = 0.16$, $p < .01$) and Violent Index rates ($B = 0.25$, $p < .001$), which suggests that Latino poverty contributes to Latino violence, net of other controls and mediating effects. Language heterogeneity contributes significantly to all three forms of Latino violence (homicide $B = 0.14$, $p < .05$; robbery $B = 0.16$, $p < .01$; Violent Index $B = 0.11$, $p < .05$). In contrast, residential mobility appears to reduce Latino homicide ($B = -0.12$, $p < .05$) and has no significant effect on Latino robbery or Violent Index rates. Racial/ethnic heterogeneity has no significant effect on Latino violence. These findings provide some support for traditional social disorganization arguments and show that poverty and language heterogeneity both contribute to Latino violence. Furthermore, because immigration contributes to several of the social disorganization measures, the results also suggest that immigration has indirect effects on violence – i.e., that immigration indirectly contributes to violence by destabilizing structural conditions of Latino communities and particularly by increasing the language heterogeneity of census places.

To better illustrate the indirect effects of immigration on violence, Table 3 lists the effects of Latino immigration on each mediating variable and then shows the effects of the mediators on Latino rates of violence. In addition, Table 3 (top 3 rows) sums the indirect effects to show the combined mediating effects of the four social disorganization measures in the immigration–violence relationship, net of other factors. These summed effects indicate that immigration indirectly increases Latino homicide ($B = 0.044$), robbery ($B = 0.070$), and Violent Index rates ($B = 0.052$) through the combined set of social disorganization measures. Thus, the results from Table 3 provide some additional support for social disorganization arguments and suggest that immigration indirectly increases violence by destabilizing Latino communities.

In contrast to the social disorganization position, the results also support community resource arguments and show that immigrant concentration strengthens social resources and institutions in Latino populations. Table 2 (columns 5–7) shows that the percentage of new Latino immigrants has significant negative effects on the percentage of female-headed families with children ($B = -0.32$, $p < .001$), the percentage of unemployed males ($B = -0.32$, $p < .001$), and the burden of care ($B = -0.15$, $p < .01$) in Latino populations. This indicates that places with higher immigration tend to have relatively fewer Latina-headed families with children, fewer unemployed males, and a lower Latino burden of care, net of controls.

In support of community resource arguments, the results also suggest that several of these structural measures are related to Latino violence. The last three columns of Table 2 show that the percentage of Latina-headed families with children has significant, positive effects on Latino homicide ($B = 0.15$, $p < .05$), robbery ($B = 0.12$, $p < .05$), and Violent Index rates ($B = .08$, $p < .05$), which suggests that the percentage of female-headed families contributes to Latino violence, net of other factors (e.g., poverty, unemployment). The Latino care burden is also positively related to Latino Violent Index offending ($B = 0.13$, $p < .001$), indicating that Latino violence is higher in places with a greater burden of care placed on Latino residents. These findings provide some support for community resource arguments because immigration *reduces* the Latino care burden and presence of female-headed families, which both contribute to Latino violence. Thus, the findings suggest that immigration indirectly reduces violence by strengthening social institutions and support systems of Latino populations.

To further illustrate these indirect effects, Table 3 (top 3 rows) sums the indirect effects of immigration on violence that operate through the combined set of community resource measures. Through these three measures, immigration has negative indirect effects on Latino homicide ($B = -0.079$), robbery ($B = -0.053$), and Violent Index rates ($B = -0.059$). Thus, the results in Table 3 provide further support for community resource arguments and suggest that immigration indirectly reduces violence by stabilizing the structural conditions (especially family structure) of Latino populations, controlling for other social–ecological factors.

Taken together, these results lead to several conclusions. The findings support social disorganization arguments but also support the community resource perspective on immigration and violence, suggesting that immigration has multiple, countervailing indirect effects on violence. Table 3 shows that the destabilizing effects of immigration that lead to violence are offset by the violence-reducing effects of immigration that operate through community resource measures. The results also show that immigration has little direct effect on Latino homicide or Violent Index measures but appears to reduce Latino robbery. Finally, combining these indirect and direct effects reveals the overall or *total* effects of immigration on Latino violence (Table 3). The total effects show that immigration has small, but slightly negative effects on Latino homicide

Table 3
Direct, indirect, and total effects of immigration on Latino arrest rates for homicide, robbery, and Violent Index.

	Direct effect	Indirect effects through social disorganization	Indirect effects through community resources	Total immigration effect
% Foreign-born 1990–2000 → homicide	−0.009	0.044	−0.079	−0.043
% Foreign-born 1990–2000 → robbery	−0.144	0.070	−0.053	−0.126
% Foreign-born 1990–2000 → Violent Index	−0.025	0.052	−0.059	−0.032

Standardized effects of Latino immigration on mediating variables and effects of mediating variables on Latino violence

% Foreign-born 1990–2000	Mediator	Robbery
0.12*	Residential mobility	−0.12*
0.01	Poverty	0.16**
0.11*	Racial/ethnic heterogeneity	0.06
0.38***	Language heterogeneity	0.14*
−0.32***	Female-headed families	0.15*
−0.32***	Unemployment	0.09
−0.15**	Care burden	0.02
% Foreign-born 1990–2000	**Mediator**	**Violent**
0.12*	Residential mobility	0.05
0.01	Poverty	0.05
0.11*	Racial/ethnic heterogeneity	0.03
0.38***	Language heterogeneity	0.16**
−0.32***	Female-headed families	0.12*
−0.32***	Unemployment	0.06
−0.15**	Care burden	−0.03
% Foreign-born 1990–2000	**Mediator**	**Violent Index**
0.12*	Residential mobility	0.03
0.01	Poverty	0.25***
0.11*	Racial/ethnic heterogeneity	0.05
0.38***	Language heterogeneity	0.11*
−0.32***	Female-headed families	0.08*
−0.32***	Unemployment	0.04
−0.15**	Care burden	0.13***

$N = 396$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

($B = -0.043$) and Violent Index rates ($B = -0.032$) but much more sizable negative effects on Latino robbery rates ($B = -0.126$). Thus, to the degree that it has an overall effect, immigration appears to slightly reduce Latino violence (i.e., robbery).

Last, although they are modeled separately and independently, it is possible that measures of social disorganization and community resource factors may affect one another. To account for such effects and more fully exhaust the data, I estimated several supplemental models in which social disorganization measures and community resource measures were permitted to influence one another (results not shown). These effects did not substantially improve the fit of the models and did not change the substantive findings on immigration and violence reported above. However, exploring the connections between mediating processes in the immigration–crime relationship may be a fruitful avenue for future research.[14]

5. Discussion and conclusion

Despite longstanding interest in the social consequences of immigration, there are few studies that examine the effects of immigration on crime and particularly few studies that address the effects of contemporary Latino immigration on crime. Additionally, prior research and theory offer conflicting views on the immigration–violence relationship. This study addresses the immigration–violence debate by examining the direct, indirect, and total effects of immigration on Latino vio-

[14] Although SEM techniques used in AMOS are well-suited for identifying indirect effects, they do not allow estimation of Poisson-based regression models used in prior research (e.g., Lee et al., 2001; Stowell, 2007). To address this issue, I estimated several preliminary negative binomial models of Latino homicide in which the sets of social disorganization and community resource were entered sequentially. The supplemental results generally paralleled the findings reported here, indicating that Latino immigration does not contribute to violence, has no noticeable direct effects, and may have indirect effects on violence working through social disorganization and community resource measures (based on upward/downward shifts in the immigration coefficient as social disorganization and community resource measures were added to the model).

lence and by simultaneously addressing competing perspectives on immigration and violence – i.e., the *social disorganization* and *community resource* perspectives.

The results indicate that recent patterns of Latino immigration have several noteworthy effects on Latino violence. First, immigrant concentration appears to have no direct effect on Latino homicide or Violent Index rates but some violence-reducing effects on Latino robbery. Second, immigration has several opposing *indirect* effects on Latino violence that work through both social disorganization and community resource measures and largely offset one another. Specifically, immigrant concentration contributes to Latino violence by increasing population heterogeneity (i.e., language heterogeneity) but simultaneously reduces violence by strengthening Latino social institutions and support/capital – i.e., by reducing the presence of female-headed families and the burden of care among Latino populations, controlling for other factors. Third, the *total* effects of immigrant concentration reveal that immigration ultimately reduces Latino robbery but has smaller effects on Latino homicide and Violent Index rates. In sum, the results suggest that immigration is not part of the violence-generating process for Latinos and may actually have some violence-reducing effects on Latino populations.

Important implications of these findings include, first – immigration does not appear to contribute to Latino violence, at least in the short run. Public perceptions of immigration-generated waves of violence and commonly held fears among U.S. residents that Latino immigration is contributing to violence and creating "zones of lawlessness" are at odds with the current results (as for example, Horowitz, 2001: 8). Additionally, the increasing use of restrictive legislation aimed at reducing crime by targeting immigration is not consistent with findings on immigration and crime described above. Instead, these findings indicate that immigration has little overall impact on Latino violence with some violence-reducing effects. In addition, the findings suggest that popular perceptions linking immigration and crime and legislation aimed at curbing crime by restricting immigration and the resources and services available to immigrant populations may be inconsistent with social realities. Further research is needed to more closely evaluate the political and social impact of such policies and perceptions on crime control and on the social well-being of immigrant populations. However, in light of the current findings, future studies should more fully explore the potential protective, stabilizing benefits of immigration illustrated here and consider ways to maximize any existing violence-reducing effects of immigration and minimize the violence-generating barriers and constraints (e.g., language barriers among community members) that may confront immigration populations.

Second, these findings provide support for elements of social disorganization and community resource arguments and indicate that immigration does not have one direct pathway for influencing violence. Immigration appears to have some destabilizing effects that contribute to violence, but it also reduces violence by strengthening Latino institutions and social support/capital frameworks. Although these effects neutralize one another, they suggest that research on immigration and crime should consider both social disorganization and community resource arguments and should continue to explore both the potential stabilizing and destabilizing effects of immigration on Latino communities and violence.

Third, the current findings illustrate the need to move beyond analyses of direct effects to also identify indirect (and total) effects in research on immigration and crime. As shown here, the direct effects are only one part of the immigration–violence relationship. By ignoring the indirect and total effects of immigration on violence, research is likely to overlook important nuances of this relationship.

Though this research offers several important findings and provides one of the first examinations of indirect effects of immigration on Latino violence, future research needs to move beyond this study in several ways. As in most macro-level studies of crime, independent and mediating variables were constrained by collinearity concerns and were also limited to measures available from U.S. Census data at the census place-level. As a result, some potentially important factors in the immigration–violence relationship could not be included (e.g., collective efficacy, extended family ties, religious participation, percent employed in professional occupations, authorized/unauthorized immigration). As characterizes most prior research, this study relies on cross-sectional data to examine the immigration–violence relationship. Thus, to better establish the effects of immigration on violence as well as potential reciprocal effects or selection effects, there is a strong need for future immigration–violence research that employs longitudinal data. Although census place-level data have several strengths and provide an untapped unit of analysis for addressing immigration–crime relationships, like other study units used in immigration–crime research (e.g., census tracts, MSAs) they are not a perfect proxy for "communities." Thus, future studies should expand immigration–crime research to other units of analysis and particularly to those that more closely approximate neighborhoods and communities. Last, although the CAL and NYSAD data provide some of the best available information on Latino offending for serious violent crimes across two of the largest and most highly-immigrant-populated states in the U.S. (Pew Hispanic Center, 2006), it is important for future research to build on these analyses to identify immigration effects on crime for other race/ethnic groups, states, and offense types. Taken together, more research is needed along the lines of this study for purposes of informing scholars, policy-makers, and the general public about the effects of immigration on crime and about the social consequences and political implications of contemporary trends in immigration on U.S. society.

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Exploring the Connection between Immigration and Violent Crime Rates in U.S. Cities, 1980–2000

Graham C. Ousey, *College of William & Mary*

Charis E. Kubrin, *George Washington University*

A popular perception is that immigration causes higher crime rates. Yet, historical and contemporary research finds that at the individual level, immigrants are not more inclined to commit crime than the native born. Knowledge of the macro-level relationship between immigration and crime, however, is characterized by important gaps. Most notably, despite the fact that immigration is a macro-level social process that unfolds over time, longitudinal macro-level research on the immigration-crime nexus is virtually nonexistent. Moreover, while several theoretical perspectives posit sound reasons why over-time changes in immigration could result in higher or lower crime rates, we currently know little about the veracity of these arguments. To address these issues, this study investigates the longitudinal relationship between immigration and violent crime across U.S. cities and provides the first empirical assessment of theoretical perspectives that offer explanations of that relationship. Findings support the argument that immigration lowers violent crime rates by bolstering intact (two-parent) family structures. Keywords: immigration, violent crime, demographic transitions, family structure, drug markets.

Nearly 80 years ago, criminologist Edwin Sutherland (1924, 1934) highlighted immigration and crime as an area of popular misconception and policy distortion. Today, not much has changed as both public opinion about immigration and immigration policy appear to be driven more by stereotype than by empirical fact (Martinez and Lee 2000). Ruben Rumbaut and Walter Ewing (2007) note: "The misperception that the foreign born, especially illegal immigrants, are responsible for higher crime rates is deeply rooted in American public opinion and is sustained by media anecdote and popular myth" (p. 3; see also Hagan, Levi, and Dinovitzer 2008:96).

In contrast to common perception, a rapidly expanding literature reports that immigrants are less criminally involved than their native-born counterparts (Hagan and Palloni 1999; Martinez 2002). Based on an extensive review of the literature, Ramiro Martinez and Matthew Lee (2000) conclude: "The major finding of a century of research on immigration and crime is that . . . immigrants nearly always exhibit lower crime rates than native groups" (p. 496). This finding of lower immigrant criminality is evident across studies that focus on several outcomes including crime and incarceration (e.g., Butcher and Piehl 1998b; Sampson, Morenoff, and Raudenbush 2005). Thus, the salient question of whether immigrants have a greater propensity to commit crime than the native born appears to be no, although by no means is this answer definitive.

But the immigration-crime research picture remains incomplete. First, unlike the abundance of research on the *individual-level* association between immigrant status and criminal offending, there exists a comparative shortage of research on the *macro-level* relationship

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Social Problems, Vol. 56, Issue 3, pp. 447–473, ISSN 0037-7791, electronic ISSN 1533-8533. © 2009 by Society for the Study of Social Problems, Inc. All rights reserved. Please direct all requests for permission to photocopy or reproduce article content through the University of California Press's Rights and Permissions website at www.ucpressjournals.com/reprintinfo/asp. DOI: 10.1525/sp.2009.56.3.447.

between immigration and crime rates (Mears 2001; Reid et al. 2005). This is problematic because immigration is an aggregate-level phenomenon whose effects may extend far beyond the simple thesis that immigrants are more crime prone than nonimmigrants. Indeed, there are good reasons to suspect that immigration affects demographic, economic, and social structures in ways that will impact overall crime rates, net of any differences in the individual-level offending of immigrants and natives (Reid et al. 2005).

Second, the relative scarcity of macro-level research is compounded by a near absence of *longitudinal* research on the immigration-crime nexus. In fact, although immigration is fundamentally a process of social change that unfolds over time, most prior aggregate-level studies on immigration and crime are cross-sectional. This is a serious limitation because cross-sectional analysis is best suited for analyzing whether stable features of aggregate social units are correlated with one another (i.e., stock effects) not how temporal change in one social process affects change in another (i.e., flow effects). Moreover, ample evidence suggests there are differences in the cross-sectional and longitudinal effects of macro-social predictors on crime (Cantor and Land 1985, 2001; Chiricos 1987; Marvell and Moody, Jr. 1991; Phillips 2006a), which makes it clear that we cannot make generalizations about the longitudinal immigration-crime relationship from extant cross-sectional research.

Third, prior macro-level research is limited by the fact that theories of the immigration-crime relationship have not been sufficiently evaluated. Indeed, although there are multiple explanatory frameworks that posit plausible mechanisms by which immigration may affect change in crime, indicators representing those frameworks often have not been collectively included within macro-level studies (Mears 2002:284). Perhaps more importantly, prior aggregate studies have not assessed whether salient social factors *mediate* the immigration-crime relationship in the manner predicted by prominent theoretical arguments.

In the present study, we seek to advance the literature on the connection between immigration and crime by attending to these limitations. Specifically, we investigate the impact of change in measures of immigration on change in serious crime for 159 U.S. cities from 1980 through 2000. We also address important theoretical limitations by empirically testing the efficacy of several alternative explanations of the temporal nexus between immigration and crime. In particular, we examine the extent to which within-city, over-time change in measures drawn from these explanatory models can account for (i.e., mediate) the longitudinal relationship between immigration and violent crime. Before describing our research design and discussing the results, we begin by outlining potential explanations for the immigration-crime relationship. We follow that with a summary of what has been learned from prior research on the immigration-crime nexus.

Conceptual Framework

Despite popular perception that immigration and crime go hand in hand, there are sound reasons to believe that immigration can impact social life in ways that either increase *or* decrease crime rates. We first review those perspectives that suggest immigration leads to more crime. Then we review those that suggest the opposite—that increased immigration results in less crime.

Perspectives Positing a Positive Immigration-Crime Relationship

Demographic Transition and Population Instability One framework theorizing a positive longitudinal relationship highlights the fact that immigration leads to demographic transitions that affect crime rates. There are two variants of the demographic transition framework, with each emphasizing somewhat different causal mechanisms. The first argument is compositional. It suggests that immigration increases crime rates by raising the share of the population with a

"crime-prone" demographic profile. A firmly established criminological finding is that crime follows a distinctive age pattern with offending rates being highest among teens and young adults (Hirschi and Gottfredson 1983). Another well-established "crime-fact" is that males are involved in crime, especially violent crime, at significantly higher rates than females (Pastore and Maguire 2008). Thus, to the extent that immigration increases the percentage of the population that is young and male, crime rates will increase. Consistent with that logic, evidence from the 2000 Census suggests that compared to the native-born population, a higher proportion of recent immigrants are male (51.4 percent versus 48.9 percent) and in the 15 to 34 year age range (53.7 percent versus 26.8 percent) (U.S. Census Bureau 2001).

The second demographic transition argument is contextual and it draws from the social disorganization framework, particularly early formulations of that theoretical perspective (e.g., Shaw and McKay 1969). As traditionally conceptualized, social disorganization theory contends that crime rates will rise when rapid social change breaks down the social networks and institutions necessary for effective socialization and behavioral regulation. One change believed to contribute to social disorganization is population instability. As a major driver of population change and residential instability, immigration may thus be regarded as a critical factor behind the breakdown of informal social control and concomitant increases in crime rates (Bankston 1998; Lee, Martinez and Rosenfed 2001; Lee and Martinez 2002; Mears 2002; Reid et al. 2005). In essence, this argument asserts that immigration creates population turnover and instability, which lead to more crime. It should be noted that a key distinction between the two demographic transition hypotheses is the latter suggests that increased immigration will lead to higher crime rates among all population groups, while the former suggests the increase will be confined to the recent foreign born.

Labor Market Structure and Economic Deprivation A second set of perspectives supporting a positive association between changes in immigration and crime highlight the role of economic opportunities and economic deprivation. One strand of this viewpoint posits that immigration elevates crime by increasing the share of the population with low educational attainment, marginal labor market skills, and poor employment prospects. Research documents that recent waves of immigrants are less skilled than both earlier immigrants and natives (Butcher and Piehl 1998a:461; see, for example, Borjas 1990). This lack of human capital dampens their job prospects and also may narrow their residential options. Consequently, many immigrants are channeled into neighborhoods located in or around urban ghettos (Hagan and Palloni 1999; Shaw and McKay 1969; Thomas and Znaniecki 1920) where they are more likely exposed to unemployment, poverty, and sundry social ills associated with contexts of severe economic deprivation. Immigrants may thus come to the realization that opportunities for attaining economic success via legitimate avenues are bleak. According to opportunity structure theory, that realization can lead to strain and frustration, which will heighten the probability of adaptive responses that involve alternative economic pursuits, such as crime (Lee et al. 2001:561; Mears 2002:284; Reid et al. 2005:759).

While an influx of low-skilled immigrants may, in general, contribute to rising unemployment and poverty rates, more recent waves of immigration may be particularly likely to produce such outcomes. This is because the urban labor market structure encountered by immigrants in recent decades is distinct from that faced by immigrants during other periods of mass immigration in American history:

> Whereas earlier European immigrants entered American cities at a time when manufacturing jobs were plentiful and provided a means of upward mobility, new immigrants must confront an "hourglass economy" that bifurcates opportunities for employment between menial low-wage jobs at the bottom and high-skill professional and technical jobs at the top and provides very limited opportunities for immigrants to advance beyond the bottom rung of the economic ladder without substantial investments in human capital and acquisition of requisite social networks (Morenoff and Astor 2006:38).

The implication of this newer economic order is that the "Americanization" experience of recent arrivals, in some cases, parallels the experience of similarly situated African Americans or Latinos (Martinez, Lee, and Nielsen 2004:135). That is, assimilation into American life may not involve the desired trajectory of upward mobility but instead may be "downward," involving sustained exposure to economic deprivation and a deviant lifestyle (Portes and Rumbaut 2001; Rumbaut et al. 2006:73). In short, while immigrants generally face tough economic hurdles in the assimilation process, immigrants entering during the most recent waves may be especially vulnerable to the types of economic deprivation associated with greater crime and violence.

The above argument focuses on how a lack of human capital among immigrants creates disadvantages in the labor market that ultimately lead to their own involvement in crime. A broader related thesis suggests that immigration may affect economic deprivation and crime among nonimmigrants as well. This viewpoint contends that immigration increases crime by changing the overall structure of local labor markets (Reid et al. 2005). For example, research suggests that increased immigration produces a new pool of low-skill, low-wage labor that competes with and may displace existing low-skill workers (Beck 1996; Waldinger 1997). As a result, displaced groups face greater deprivation, which may elevate their involvement in crime (Wilson 1996). Immigration also may increase overall levels of economic deprivation by driving up the supply of low-skill labor and driving down the base wage among all low-skill workers. Meanwhile, the surplus of low-skill workers rendered by immigration may simultaneously increase unemployment rates. These deleterious effects of immigration on wages and employment rates may increase the demand for public welfare services beyond the capacity of existing resources, which would only exacerbate further the experience of economic deprivation. In sum, this latter argument suggests that by increasing unemployment, depressing wages, and straining public welfare resources, temporal increases in immigration may contribute to higher crime rates among both low-skilled native-born workers as well as among immigrants themselves (Butcher and Piehl 1998a).[1]

Illegal Drug Markets Several studies document that recent trends in crime are linked to changes in illicit drug markets (Baumer et al. 1998; Fryer et al. 2005; Levitt 2004; Ousey and Lee 2002, 2004, 2007). Researchers have argued the proliferation of crack-cocaine markets during the 1980s produced a volatile marketplace that contributed to higher rates of violent crime. The volatility of these drug markets emanated from several sources. First, the rush to capitalize on this emergent economic opportunity was substantial and competition-related conflict was fairly prevalent. Second, due to the illegality of the enterprise, drug market conflicts were less likely to be resolved through legal dispute resolution mechanisms and more likely to be handled with personal aggression than conflicts that occurred within legitimate businesses. Third, drug market activities commonly took place in open-air street markets that exposed sellers and buyers to high risks of robbery and assault, which contributed to an "on-guard" posture and the frequent carrying of firearms (Blumstein 1995; Jacobs 2000). And finally, these markets developed in economically disadvantaged areas where attenuated informal social controls exacerbated their tendency toward violence (Ousey and Lee 2002, 2004).

There are numerous reasons to suspect the proliferation of drug markets may be a salient intervening mechanism linking immigration to changes in crime rates. Considering long-standing stereotypes, Martinez (2002) notes a popular perception is that Latinos are heavily involved in drug trafficking and the concomitant violence it generates. He claims this stereotype is both reflected in, and reinforced by, blockbuster Hollywood films such as *Scarface* (1983), *Carlito's Way*

1. We acknowledge there are other possible links between immigration and economic outcomes. In addition to research showing deleterious outcomes (Borjas 2003; Borjas, Freeman, and Katz 1997), there is also evidence that immigration may produce *beneficial* impacts for *some* labor market sectors and native-born groups (Pedace 2006). However, given these alternative arguments are still debated in the labor economics literature and given the nascent state of longitudinal immigration-crime research, we believe it prudent at this point to limit our focus to the most prominent immigration-labor market thesis discussed above. We leave it to future work to extend our efforts in this regard.

(1993), and *Traffic* (2001), which depict the drug trade and gang activity as pervasive in Latino communities. Despite common stereotypes, empirical research in this area is not well developed. In one of the few studies that does exist, Martinez (2002) reports evidence that Latinos are not over-involved in drug- and gang-related violence (see also Martinez, Nielsen, and Lee 2003).

While acknowledging that the disproportionate involvement of immigrants in the drug trade may be more myth than fact, we also argue there are structural reasons supporting the notion that immigration, drug markets, and violence may be connected. As explained earlier, many immigrants enter the United States with relatively low levels of human capital, which exposes them to tough sledding in the post-industrial labor market. Hence, it is plausible that illegitimate opportunity structures such as the illegal drug trade are particularly appealing avenues of economic success for immigrants who encounter difficulties locating work in legitimate industries. Moreover, given that immigrants disproportionately settle in economically disadvantaged neighborhoods, it is likely they face greater exposure to the promises (and pitfalls) of open-air drug markets. Finally, because new immigrants are disproportionately young and male, they fit the demographic profile of individuals recruited to participate in crack-cocaine markets (Blumstein 1995). Indeed, some scholars contend that Latino gangs in California heavily recruit drug market participants from recently arrived immigrant pools, particularly illegal immigrants from Mexico and Central America (Mac Donald 2004). It has also been reported that service in the drug trade is one way that illegal immigrants pay off debts to the Latino gangs that helped arrange their transit from Mexico to the United States (Mac Donald 2004).

In sum, there are several conceptual arguments that proffer reasons why immigration will produce increases in rates of crime and violence over time. Some of these perspectives focus on the behavior of immigrants themselves, while others suggest that rising crime rates reflect the behavior of both immigrants and the native born. Although each explanation provides a rationale consistent with the popular perception of a positive relationship between immigration and crime, much empirical evidence—particularly at the individual level—contradicts that perception. This conflicting evidence calls for conceptual arguments that explain the possibility of a negative association between immigration and crime. In the following section, we discuss such arguments.

Perspectives Positing a Negative Immigration-Crime Relationship

Immigrant Selection Effects Several explanatory frameworks posit that over-time increases in immigration will contribute to less crime and violence. The first argument suggests that immigrants are not necessarily a random cross-section of the sending population but are a self-selected group with relatively high levels of achievement ambition and low criminal propensity (Butcher and Piehl 2005). As Michael Tonry (1997:21) argues, many immigrants are highly motivated to come to the United States to pursue economic and educational opportunities that are not available in their home countries. They seek to build better lives, are willing to work hard, defer short-term gratification in the interest of longer-term advancement, and are likely to avoid actions that put them in opposition to mainstream norms and values of American society. Moreover, some immigrant groups (e.g., Koreans) arrive in the United States better educated than the average native-born American and are therefore more qualified to find jobs in the primary labor market (Alba and Nee 1997). In essence, because immigration is often an arduous process that takes considerable planning and resources, those who immigrate are more likely to be selected from the low end of the criminal propensity distribution and therefore, rising immigration levels should bolster the low-criminality segment of the U.S. population, leading to less crime over time.

Formal Social Control A second argument that increased immigration will result in lower crime rates focuses on the formal social control response to immigration flows. This viewpoint posits that because of stereotypes regarding immigrant criminality, an increase in immigration

will propagate fear and concern about a worsening "crime problem." Fear and concern among the general public will, in turn, put pressure on elected officials, local governments, and law enforcement leaders to respond to this perceived problem. One common response is to bolster the local formal social control apparatus. The most straightforward and visible way to do this may be to hire additional police officers to patrol the streets and deter the crimes that immigration is believed to engender. Some longitudinal research evidence suggests that increasing the police force size contributes to lower crime rates (Levitt 2004; Marvell and Moody, Jr. 1996). Thus, a plausible hypothesis is that temporal increases in immigration contribute to decreases in crime and violence by expanding the formal social control capacity (i.e., police officers per capita).

Social Capital and Family Structure A final perspective that posits a negative relationship between immigration and crime focuses on the levels of social capital and informal social control that tend to characterize communities populated by immigrants. Decades ago, Donald Taft (1933:72) argued that immigrant "ghettos" can serve a protective function by dampening culture conflict and preserving "old world" mechanisms of informal social control. More recently, this argument has been made with respect to ethnic enclaves. Ethnic enclaves may encourage cultural preservation, promote or maintain family ties and social networks, provide employment and entrepreneurial opportunities, and bolster informal social control, all of which help curb crime (see Desmond and Kubrin forthcoming for a detailed discussion). In their study of a New Orleans Vietnamese enclave, Min Zhou and Carl Banskton (2006) provide empirical evidence in support of this argument: "We found that although Vietnamese young people lived in a socially marginal local environment they were shielded from the negative influences of that environment by being tightly bound up in a system of ethnic social relations providing both control and direction" (pp.119–20). Along these lines, it has been argued that illegal immigrants' relatively limited involvement in crime can be explained, in part, by social support in ethnic communities (Engbersen and van der Leun 2001:51).

An extension of this argument, reflecting more recent formulations of social disorganization theory, claims the positive benefits of immigration are not confined to the immigrants residing in enclaves. Recall that early versions of social disorganization theory suggested an influx of immigrants into an area weakens informal social control and increases crime. In line with more contemporary versions of the theory, Lee and Martinez (2002) advance an immigrant revitalization thesis suggesting that an increase in immigration instead fosters social control, thereby reducing crime. Martinez (2006) notes:

> Contemporary scholars are now more open to the possibility that an influx of immigrants into disadvantaged and high-crime communities may encourage new forms of social organization and adaptive social structures. Such adaptations may mediate the negative effects of economic deprivation and various forms of demographic heterogeneity (ethnic, cultural, social) on formal and informal social control, thereby decreasing crime (p. 10; see also Lee and Martinez 2002:366; Lee et al. 2001:564).

Empirical support for this argument is documented by Alejandro Portes and Alex Stepick (1993), who find that rather than causing community disorganization, immigrants stabilized and revitalized Miami's economic and cultural institutions.

An unresolved issue for the revitalization thesis focuses on identifying which salient changes brought on by immigration ultimately contribute to declining rates of crime and violence. One possibility is that immigration itself contributes to a revitalization of the economy in places where immigrants settle. In contrast to the view that immigrants increase the size of the economically marginal population and add strain to existing public services, this argument suggests that immigrants bring new energy, skills, and entrepreneurial spirit into their communities. As a result, they may work to lower unemployment and poverty rates and improve the vitality of economic institutions.

Another strand of this argument suggests that immigration alters aggregate family and household structures in ways that strengthen informal social control and impede crime. The

segmented assimilation model, for example, suggests that contemporary immigrant communities erect important social networks that fortify traditional intact (two-parent) family structures and support the legitimacy of parental authority norms (Martínez et al. 2004). Related to this, scholars argue that many immigrant groups have a more familistic and pro-nuptial cultural orientation than the native born (e.g., Fukuyama 1993; Oropesa 1996; Oropesa, Lichter and Anderson 1994; Vega 1990, 1995; Wildsmith 2004). According to David Brooks (2006), "[I]mmigrants themselves are like a booster shot of traditional morality injected into the body politic . . . They have traditional ideas about family structure, and they work heroically to make them a reality." In accordance with this logic, research finds that despite experiencing higher rates of the types of economic deprivation that impede marriage, immigrant groups such as Mexican Americans have comparable marriage rates to non-Hispanic whites (Oropesa and Landale 2004; Oropesa et al. 1994; Sampson et al. 2005) and place greater value on marriage than do non-Hispanic whites (Oropesa and Gorman 2000). To the extent that immigrants have greater intact family structures and pro-family cultural orientations, it is likely that increasing immigration will lead to less crime. Indeed, criminologists have long documented that areas with higher rates of single-parent families and higher divorce rates experience more crime, presumably because the breakdown in traditional family structures deplete social capital and attenuate socialization and informal social control processes (Land, McCall and Cohen 1990; Ousey 2000; Sampson 1987; Sampson and Groves 1989; Shihadeh and Steffensmeier 1994). To summarize, the above discussion leads to a hypothesis that temporal increases in immigration result in lower crime rates, in part by reducing family disruption.

Prior Research

The Immigration-Crime Relationship

Documented scholarly interest in the connection between immigration and crime goes back more than a century but attention devoted to the issue has been intermittent. Indeed, investigations of immigration and crime have varied along with immigration itself. When immigration flows have been high, scholarship has flourished; when immigration flows have been low, scholarship has waned (Stowell 2007). Reviewing research from the past century, several observations are particularly noteworthy.

First, the vast majority of research focuses on the question of whether immigrants have higher crime, arrest, and incarceration rates than native-born individuals. In general, the answer to this individual-level question is no. Going back over seven decades, the National Commission on Law Observance and Enforcement—commonly known as the Wickersham Commission—reported that in proportion to their numbers, the foreign born commit considerably fewer crimes than the native born. Contemporary empirical studies as well as comprehensive literature reviews continue to find that crime, arrest, and incarceration levels are lower among immigrants (Butcher and Piehl 1998b:654; Hagan and Palloni 1999:629; Martinez and Lee 2000; McCord 1995; Tonry 1997).

A related observation from prior research is that the individual level link between immigrants and crime appears to wane across generations. That is, the children of immigrants who are born in the United States exhibit much higher crime rates than their parents (Morenoff and Astor 2006:36; Rumbaut et al. 2006:72; Sampson et al. 2005; Taft 1933; Zhou and Bankston 1998), suggesting one part of the "Americanization" process involves increased crime and incarceration levels.

Second, and shifting focus from the individual-level immigrant-crime question to the central concern of this research—the macro-level immigration-crime relationship—it is apparent that findings from extant studies are more inconsistent (see Table 1 for a summary of aggregate-level studies on the immigration-crime relationship). For example, in their analysis of metropolitan areas, Lesley Williams Reid and colleagues (2005) report that the bivariate

Table 1 • ***Summary of Aggregate-Level Studies on the Immigration-Crime Relationship***

Study	*Unit of Analysis*[a]	*Sample Size*	*Immigration Independent Variable(s)*	*Dependent Variable(s)*	*Relationship*	*Controls*
Butcher and Piehl (1998)[b]	Metropolitan areas	43	% recent immigrants	Total and violent crime rates	No sig. effects	Mean age, % female, % black, % Hispanic, mean education, % central city, total population size, mean wage, wage dispersion, % employed
Desmond and Kubrin (forthcoming)[b]	Individuals nested within block groups	9,500 youth in block groups	Immigrant concentration index: (1) % foreign born (2) % persons who speak English not well or not at all	Self-reported juvenile violence	Sig. (–) effect	Individual-level controls include gender, age, race/ethnicity, parental SES, immigration status, residential mobility, family structure, attachment to child, parental attachment, parental supervision, grades in school, school attachment, delinquent peers, prior delinquency, urban location, region. Neighborhood-level controls include disadvantage, residential mobility, racial heterogeneity.
Lee and Martinez (2002)	Census tracts	12	% recent immigrants	Black homicide rates	(–) effect[c]	N/A
Lee et al. (2001)	Census tracts	352	% new immigrants	Latino and black homicide rates	Sig. (–) effect for El Paso tracts in Latino models; sig. (–) effects for Miami and San Diego tracts in black models	Race-specific measures of population size: % poverty, instability % female-headed families % male joblessness % young males, spatial lag

Martinez (2000)	Cities	111	Latino immigration index: (1) Foreign-born Latinos (2) Latinos residing in foreign-country 5 yrs. before 1980 Census	Total and motive-disaggregated Latino homicide rates	No sig. effect for total homicide rates; sig. (+) effect for Latino felony rates; sig. (–) effect for Latino acquaintance rates	% Latino poverty, Latino income inequality, % Latino high school grads, total population size, % Latino males 15 to 24, % Latino divorced, % Latino population, region
Martinez et al. (2008)	Census tracts	532	% recent immigrants	Homicide victims	Sig. (–) effect[d]	Disadvantage index, stability index, % professional, adult to child ratio, spatial lag
Martinez et al. (2004)	Census tracts	266	(1) % immigrants who arrived in U.S. in 1980s (2) % immigrants who arrived in U.S. in 1970s (3) % immigrants who arrived in U.S. in 1960s[e]	Drug-related homicides	Sig. (–) effect of % immigrated 1960s; sig. (+) effect of % immigrated 1980s in San Diego tracts only	% young males, % low-skill workers, % vacant, economic deprivation
Nielsen et al. (2005)	Census tracts	266	% recent immigrants	Race- and motive-disaggregated homicide rates	Too many findings to report here but generally null or (–) effects	Disadvantage index, residential instability, % young males, population size, spatial lag
Reid et al. (2005)	Metropolitan statistical areas	150	(1) % recent foreign-born (2) % born in Asian country (3) % born in Latin American country (4) % foreign-born who speak English not well or not at all[f]	Murder, robbery, burglary, and theft rates	Sig. (–) effect for recent foreign born on homicide rate; sig. (–) effect for Asian foreign born on theft rate; other measures non-significant for all crime rates	Total population size, population density, median family income, % families living below poverty, % black, % children < 18 yrs. not living with both parents, income inequality, unemployment rate, % employed in low-skill service sector industries, % employed manufacturing industries, divorce rate, % pop. aged 15 to 29, region

(*continued*)

Table 1 • *(continued)*

Study	*Unit of Analysis*[a]	*Sample Size*	*Immigration Independent Variable(s)*	*Dependent Variable(s)*	*Relationship*	*Controls*
Sampson et al. (2005)[g]	Individuals nested within census tracts	2,925 persons in 180 tracts	% 1st generation immigrant	Self-reported violence	Sig. (–) effect	Individual-level controls include race/ethnicity, gender, immigrant status, family structure, SES, individual differences (e.g., verbal/reading ability) measures Neighborhood-level controls include % black, % managerial occupation, concentrated disadvantage, residential stability, moral/legal cynicism, collective efficacy, friend/kin ties, organizations/youth services, prior violent crime rate
Stowell and Martinez (2007)	Census tracts	592	% recent immigrants	Total and motive-disaggregated violent crime rates	Sig. (–) effect only for tracts located in Miami[h]	% poverty, racial diversity, % residential instability, % unemployment, % males aged 18 to 24, spatial lag

[a]A description of data years included in each study would be beneficial here. Unfortunately, for the studies listed, the use of diverse data sets covering different time periods in conjunction with running multiple subanalyses over several periods of time preclude effective representation of data years for each study.
[b]This study measures the contextual effect of neighborhood immigrant concentration on self-reported juvenile violence.
[c]Findings are not based on regression results. Lee and Martinez (2002) conduct a version of the single case study method known as the critical case as well as conduct spatial analysis.
[d]Effect is negative and significant only in San Antonio neighborhoods in subanalysis of Latino neighborhoods.
[e]Immigration measures are individually included in analyses.
[f]Immigration measures are individually included in analyses.
[g]This study measures the contextual effects of neighborhood immigrant concentration on residents' self-reported violence.
[h]Significant negative effects were found for all outcomes excluding Haitian violent crime rates in Miami. No significant effects were reported for Houston tracts.

association between immigration and crime varies from negative to positive to nil depending on which measures of immigration and crime are used. And in their multivariate analyses, 14 of the 16 immigration-crime regression coefficients are not statistically significant. Of the two that are, one suggests that metropolitan areas with a higher percentage of recent immigrants have lower homicide rates while the other indicates that a greater percentage of Asian foreign born is linked to lower larceny rates. Likewise, in their analysis of metropolitan areas, Kristin Butcher and Anne Morrison Piehl (1998a) find that the flow of recent immigrants is positively associated with the level of crime but that it has no effect on changes in the crime rate. Finally, in a study of 111 cities, Martinez (2000) reports that Latino immigration has no relationship with overall Latino homicide rates, a positive association with Latino felony-murder rates, and a negative relationship with Latino acquaintance-murder rates.

Results from research on neighborhoods also exhibit inconsistency, particularly when the results are compared across cities. In their analysis of drug-related homicides in Miami and San Diego, Martinez and colleagues (2004) find the share of the population that immigrated in the 1980s is unrelated to drug homicides in Miami but is positively related to such offenses in San Diego. In addition, the percentage of immigrants who entered the United States in the 1960s is negatively related to drug homicides in both cities, and the percentage of immigrants who entered in the 1970s is unrelated to drug homicides in either city. In a related study that focuses on the immigration-homicide link in the subsection of Miami disproportionately populated by African Americans and Haitians, Lee and Martinez (2002:374) discover that the magnitude of the negative immigration-homicide relationship is even stronger than the moderate relationship observed in the city-wide regression model. In another related study focusing on motive-disaggregated homicides (e.g., escalation, intimate, robbery, and drug-related) among Latinos and blacks in Miami and San Diego, Nielsen and associates (2005) report additional evidence of variability in the direction, magnitude, and statistical significance level of a measure of the prevalence of recent immigrants. Interestingly, they find that the impact of immigration on homicide varies across cities for the same racial/ethnic group or across racial/ethnic groups within the same city. And in yet one more related study where the city of El Paso is added to the analysis, Lee and colleagues (2001) document that the presence of recent immigrants in neighborhoods is significantly negatively associated with Latino homicide victimization in only one (e.g., El Paso) of the three cities examined. Lee and colleagues (2001) also find that recent immigration has a significant negative association with black homicide victimization in Miami but a significant positive association with black homicide victimization in San Diego. Most recently, Jacob Stowell (2007), Stowell and Martinez (2007), and Martinez, Stowell, and Jeffrey Cancino (2008) all report between-city differences in the census-tract level relationship between immigration and violence.

Two recent contextual studies, which examine the impact of immigration on violence net of individual characteristics (including immigrant status), support the thesis that immigration is negatively related to crime. Using data from Chicago, Robert Sampson, Jeffrey Morenoff, and Stephen Raudenbush (2005) report neighborhood-level immigrant concentration has a significant negative relationship with violence, controlling for individual-level characteristics. Likewise, using national-level data from the Census and the Add Health study, Scott Desmond and Charis Kubrin (forthcoming) find that communities with greater immigrant concentration generally have lower average levels of violence, but that contextual effect appears most salient for Hispanics, Asians, and the foreign born.

In sum, a substantial literature at the individual level indicates that, contrary to public opinion, immigrants are no more likely to engage in crime and violence than their native-born counterparts. A smaller but expanding literature at the aggregate level suggests less certainty with some studies documenting no relationship, some documenting a negative relationship, and a handful documenting a positive immigration-crime relationship. While this body of work provides a solid foundation for research in this area, we believe major questions and unresolved issues remain. Below we highlight two of the most critical.

Limitations of Immigration-Crime Research

In our view, research on the macro-level immigration-crime nexus contains two major shortcomings that have hampered our understanding of this issue. First, theories of the connection between immigration and crime have not, to date, been adequately tested empirically. Daniel Mears (2002) notes: "Although several criminological theories suggest certain hypotheses about criminal behavior among immigrants, these remain largely undeveloped and untested" (p. 287). As a consequence, Mears claims it remains unknown whether and to what extent immigration and crime are truly associated once covariates such as poverty, inequality, racial and ethnic composition, and drug and gun markets are controlled. Along these lines, we argue that while some aggregate-level studies do include theoretically salient covariates in their models, they are typically added as control variables without any systematic attempt to determine whether, and to what extent, they mediate or help to explain the relationship between immigration and crime. This is crucial because the theories delineated above primarily predict that the immigration-crime relationship is indirect, operating through changes in demographic, economic, and family structures.

Second, past research has left unanswered the exceptionally important question of the *longitudinal* relationship between immigration and crime. As far back as the 1930s researchers have called for proper assessment of the dynamic relationship between immigration and crime (Taft 1933:69), but research in this area is overwhelmingly cross-sectional. In fact, our review uncovered only one published multivariate study that has examined this relationship longitudinally (Butcher and Piehl 1998a). In that study, Butcher and Piehl (1998a) assessed the relationship between changes in immigration and changes in total (index) and violent crime rates for a sample of metropolitan areas. They find that change in immigration is not related to change in crime rates. However, their analysis was restricted to only 43 cities, and therefore, may miss important effects of immigration occurring across a broader sample of urban locales. Equally important, Butcher and Piehl assessed the longitudinal immigration-crime relationship only for the decade of the 1980s. This limitation seems particularly acute because the heavy influx of the foreign born in the 1990s coincided with another important trend—the large and unexpected drop in crime rates (Blumstein and Wallman 2000). While recent scholarly speculation (Sampson 2006) has proffered a link between the two temporal trends, rigorous theoretical and empirical analysis of that link remains in short supply.

In summary, although several additional shortcomings in the immigration and crime literature remain (see Mears 2001 for a discussion), we believe the most pressing include: (1) accurately investigating the direction and magnitude of the *longitudinal* relationship between immigration and crime; and (2) testing the various explanatory frameworks that posit *intervening* mechanisms by which change in immigration affects change in crime rates at the macro-level. In response to these limitations, the current study investigates the nature of, and theoretical explanation(s) for, the longitudinal immigration-crime relationship. We choose to examine the effects of immigration on violent crime (although, as noted below, we perform similar analyses for property crime rates for comparison) because public opinion on immigration and crime overwhelmingly centers on the idea that immigrants are violent and that increasing immigration into an area increases rates of violence. In particular, public perception is that immigrants are heavily involved in criminal gangs that frequently perpetrate assault and homicide. Immigrants are also commonly believed to be regular participants in the drug trade and sponsors of the violent interactions thought to be concomitant of illegal drug markets. Our focus on violent crime is thus intended to directly assess whether immigration and violence are positively associated, as popular perception suggests.

Data and Methods

Units of Analysis

Our analysis focuses on large U.S. cities observed during the 1980 to 2000 period. We include cities with a minimum population of 100,000 persons in 1980, 1990, and 2000. While 173 cities

meet these criteria, missing data reduce the number of available cities to 159. When we pool available 1980, 1990, and 2000 observations for each city, the total number of city-year observations in our multivariate models is 463.[2]

Dependent Variable

The dependent variable in the analysis is the violent crime rate. This rate is computed by summing counts of homicides, robberies, aggravated assaults, and rapes and dividing that sum by the city population (expressed in units of 100,000).[3] Data on violent offenses are obtained from the Uniform Crime Report (UCR) compiled by the FBI and made available by the Inter-University Consortium for Political and Social Research (ICPSR) at the University of Michigan. To create more stable estimates, violent crime rates for each time point are based on the sum of three consecutive years of UCR data. In other words, the 1980 rate is computed on the basis of summed violent crime and population counts from the 1980 to 1982 data files, the 1990 rate is based on 1990 to 1992 data, and the 2000 rate is calculated with 2000 to 2002 data.

Independent Variables

Following prior research (e.g., Lee et al. 2001; Reid et al. 2003; Sampson et al. 2005), we initially selected two variables as proxies for immigration. First, the percentage of the population made up of foreign-born persons who immigrated in the past ten years. Second, as a measure of linguistic isolation, we include the percentage of the population that speaks English "not well" or "not at all." As one might expect, these two measures are highly correlated ($r = .84$). Moreover, they are highly collinear with a third variable, the percent Latino ($r = .68$ and $r = .88$, respectively). We initially conceived of this latter item as an essential covariate of aggregate crime rates, as indicated in prior research (Butcher and Piehl 1998a; Butcher and Piehl 1998b). Yet high levels of covariance between the percent Latino and the two immigration items suggests that estimating their unique effects on crime would be difficult. Moreover, since Latinos have constituted the largest immigrant group entering the United States in recent decades (Martinez 2006:9; Rumbaut and Ewing 2007:3), it is apparent that our measure of *within-city change* in the percentage Latino is, in fact, reflective of an important dimension of recent immigration. For these reasons, we created a three-item immigration index by summing the z-scores of these measures.[4] The Cronbach's alpha for the immigration index is .94.[5]

2. Each of the 159 cities has nonmissing data for at least one time point but a few are missing information for one or two decennial census years. Therefore, the total number of observations (463) is slightly less than the total number of cities multiplied by the total number of time points (i.e., $N = 159$, $T = 3$, $N \times T = 477$).

3. In preliminary analyses we modeled each violent crime outcome individually. Results from those analyses are very similar to one another and also closely resemble the results reported in Table 2. Thus, to eliminate redundancy, we present results only for the summary index of violent crime. Results from models predicting specific violent crime measures are available on request.

4. Here we wish to note the substantive findings reported below do not hinge upon the use of the three-item index. If we replace the immigration index with any one of the three items, the substantive conclusions remain unchanged. Likewise, if we substitute any combination of two of the three items, we reach the same set of conclusions. Those points aside, we recognize that if the single-item measures were used separately, the interpretation of results would vary somewhat depending upon the particular measure utilized. For example, interpretation of the results for the percent Latino measure would differ to some degree from the interpretation of results based on the percent recent foreign-born measure. Still, given the strong correlations among the items, we believe the three-item index is tapping an underlying immigration component and that it is not feasible to parse out and interpret each item's unique effect. For these reasons, we have chosen to utilize the three-item index in the analysis reported below. Results from analyses that use the single-item measures are available upon request.

5. The data for these items come from Summary File 3 of the 1980, 1990, and 2000 Censuses of Population and Housing (U.S. Census Bureau 1982, 1992, 2002). The lone exception is for the item measuring recent foreign born for the year 1980, which was obtained online from the National Historical Geographic Information System (Minnesota Population Center 2004).

To the extent possible, the analyses include measures of each theoretical perspective discussed above.[6] To measure the demographic transition explanations, we include three proxies obtained from Summary File 3 of each of the past three decennial censuses. The first is simply a measure of the overall city population (change). Second, we gauge residential instability with an item that reflects the percentage of persons (aged 5 and older) not living in the same house as five years ago. The third measure, percent of the population comprised of males aged 15 to 34, is included to account for the thesis that changes in the gender and age structure of the population may account for changes in violent crime rates.

Four items drawn from the Census serve as proxies for the argument that changes in economic deprivation and labor market structure are key intervening factors that link changes in immigration to changes in crime rates. First, we measure the percentage of persons living below the government-designated poverty line. Second, we tap employment difficulties by computing the percentage of civilian persons aged 16 and over who are unemployed. And third, to represent the decline in the industrial base and the increase in high-skill sector jobs, we incorporate a measure of the percent of persons working in the manufacturing industry and a measure of the percent of persons employed in professional or managerial occupations.

To account for the explanation that immigration may affect serious crime rates by first increasing the scope of the illegal drug trade, we include a measure of the rate of arrests for the sale/manufacture/distribution of cocaine and opiates. Although this measure reflects law enforcement as well as drug market activity, past research suggests it is correlated with other measures of drug activity and serious crime, including homicide and robbery (Baumer et al. 1998; Fryer et al. 2005; Ousey and Lee 2002, 2004, 2007). The data for the 1980 and 1990 measurements of this variable are obtained by summing three years of data (i.e., 1980 to 1982 and 1990 to 1992) from the UCR extract file compiled by Roland Chilton and Dee Weber (2000), while data for the year 2000 measurement is computed from the 2000 to 2002 versions of the *Uniform Crime Reporting Program Data: Arrests by Age, Sex and Race* (FBI 2006a, 2006b, 2007).

Finally, we measure changes in family structure with two items, which are combined into a family instability index by taking the sum of their z-scores. The first is the percent of the population (aged 15+) that is divorced. The second is the percent of family households not headed by married couples. The within-city, over-time correlation between these measures is $r = .88$. Both items are obtained from the Census Summary File 3 (U.S. Census Bureau 1982, 1992, 2002).

The means and standard deviations for all variables used in the analysis are presented in Appendix A.

Analytic Strategy

Since our interest centers on investigating the nature and explanation of the longitudinal relationship between immigration and violent crime, we estimate fixed-effects linear regression models via the xtreg procedure in STATA version 10. Fixed-effects (FE) models constitute one of several alternatives to analyzing panel data, with others including random-effects (RE) and generalized estimating equation (GEE) models. Generally speaking, all of these models are preferable estimators of longitudinal data over ordinary least squares (OLS) regression because their standard error estimates adjust for the fact that repeated observations on the dependent variable for a particular city are likely to be correlated. Moreover, all methods yield consistent estimates of parameters if model assumptions hold. Yet, we prefer the FE model because it focuses solely on the within-unit (change) variation in the variables and requires less restrictive assumptions than the alternative models. In our case, the RE and GEE models assume that

6. One exception is we have no direct measure of the "self-selection" thesis. Thus, our analyses do not directly investigate that hypothesis. However, to the extent that the immigration index has a direct negative effect on violent crime rates after controls for the other theoretical models are accounted for, that result could be interpreted as consistent with a self-selection effect.

time-varying explanatory variables (e.g., the immigration index) are uncorrelated with unmeasured city-specific, time-invariant factors (i.e., the "random-effect"), whereas the FE makes no such assumption. In fact, the FE method actually controls for the influence of *all* time-invariant predictors whose effects are time-stable while RE and GEE models do not.[7] This means that the FE model controls for a key source of omitted variable bias that the RE and GEE models essentially assume is not present.[8] In addition to accounting for the city-specific fixed-effects, we also include in our models dummy variables for the years 1990 and 2000 (1980 is reference), which helps to account for the influence of unmeasured city-invariant, period-specific effects on violent crime rates. Finally, we employ the Huber/White/Sandwich or "robust" variance-covariance matrix in our computation of standard errors. This adjustment helps to account for other deviations from usual "normal error" linear regression assumptions.

Results

Results from the regression models examining whether within-city, over-time change in immigration (and other covariates) affects within-city over-time change in violent crime rates are reported in Table 2. This table contains results from a series of seven regression models. The first column reports a baseline regression model in which change in violent crime is predicted only by the immigration index and time-period dummy variables. In each subsequent model, we progressively expand on that initial model by adding measures reflecting the theoretical perspectives discussed above. Consistent with our objectives, this model-building strategy allows us to gauge the extent to which the observed relationship between change in immigration and change in violent crime is mediated—or explained—by proxies for those theories.

In the first model of Table 2 we find evidence of a statistically significant relationship between change in immigration and change in violent crime. Specifically, a one unit increase over time in the immigration index is associated with a decrease of 253 violent crimes (per 100,000 persons). In standardized terms, these results indicate that a one standard deviation increase in the immigration index corresponds with a .3 standard deviation decrease in the violent crime rate.[9] Interestingly, the direction of this coefficient contradicts popular perception that immigration is a major contributor of increased crime rates, but is consistent with theoretical models proffering rationales for why immigration has an inhibiting impact on violence.

In the second model, we introduce measures representing the various demographic transition explanations. Recall these arguments suggest that immigration leads to greater residential instability and a larger young male population, both of which should raise crime rates. The results of the second model provide very little support for that expectation. Although changes in residential mobility affect changes in the violent crime rate, neither changes in total population nor the percent of males aged 15 to 34 are significantly associated with the dependent variable. Moreover, controlling for all three measures only marginally affects the magnitude of the immigration coefficient.

The third model introduces variables that tap changes in labor market structure and economic deprivation. Generally speaking, the economic deprivation/labor market structure hypothesis posits that the inflow of immigrants into the shrinking low-skill job base of the post-industrial labor market will produce increases in poverty and unemployment rates, which in turn, will yield a rise in crime rates. Of the four economic deprivation and labor market

7. Thus, important time-invariant correlates of crime—such as region—are automatically controlled in the fixed-effects framework.

8. For an excellent discussion of the merits and potential biases of RE models for analyzing panel data, see Brame, Bushway, and Paternoster (1999).

9. For interested readers, supplemental analyses that include the percent recent foreign born in place of the immigration index indicate that cities experiencing a one percentage point increase in the percent recent foreign born exhibited, on average, a decrease of 74 violent crimes per 100,000 persons. The associated standardized coefficient is −.21.

Table 2 • *Fixed-Effects (Within-City) Linear Regression Models Predicting Violent Crime Rates, 1980–2000*

Predictors	*Model 1*	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*	*Model 7*
Immigration index	−253.09*	−254.24*	−253.49*	−256.96*	−256.75*	−128.83†	−129.86†
	(−.30)	(−.30)	(−.30)	(−.31)	(−.31)	(−.15)	(−.15)
Percent males, 15 to 34		3.68	4.33	−11.57	−12.27	−10.29	−9.59
		(.01)	(.01)	(−.02)	(−.02)	(−.02)	(−.02)
City population (Ln)		95.39	20.54	−25.55	2.40	352.60	370.64
		(.02)	(.003)	(−.004)	(.0004)	(.06)	(.06)
Residential instability		8.93*	9.91*	9.14*	9.17*	7.74†	7.83†
		(.08)	(.09)	(.08)	(.08)	(.07)	(.07)
Percent below poverty			−3.08	−9.55	−8.85	−1.15	−1.76
			(−.01)	(−.04)	(−.03)	(−.004)	(−.007)
Unemployment rate			13.31	26.07	25.02	16.62	17.46
			(.02)	(.04)	(.04)	(.02)	(.02)
Percent manufacturing			5.83	5.69	5.80	15.04	15.00
			(.03)	(.03)	(.03)	(.08)	(.08)
Percent professional/managerial			−30.17†	−32.31*	−32.00*	11.76	11.55
			(−.18)	(−.19)	(−.19)	(.07)	(.07)
Drug market arrests (Ln)				114.96*	114.61*	106.75*	105.68*
				(.20)	(.20)	(.18)	(.18)
Police officers per capita					31.00	−13.46	−21.40
					(.01)	(−.01)	(−.01)
Family instability index						432.75*	414.47*
						(.62)	(.60)
Percent black							6.58
							(.02)
Time trend dummy variables							
Year 1990	1299.11*	1321.95*	1465.01*	1256.47*	1248.07*	379.02†	398.41†
Year 2000	1185.33*	1181.47*	1535.72*	1418.48*	1398.47*	−231.13	−200.09
Model summary information							
R^2 (within-unit)	.428	.434	.440	.464	.464	.501	.501
Corr (u_i, XB)	−.377	−.362	−.302	−.290	−.274	−.078	−.140
Total number observations ($N \times T$)	463	463	463	463	463	463	463
Total Number Cities (N)	159	159	159	159	159	159	159

*$p < .05$ †$p < .10$ (two-tailed tests)

Standardized coefficients reported in parentheses.

structure variables entered into the equation in Model 3, only the percent employed in professional and managerial occupations shows evidence of a statistically discernible relationship with the change in violent crime rates. Specifically, a unit increase in professional/managerial occupation employment is associated with a drop of 30.17 (per 100,000 persons) in the violent crime rate. In contrast, change in manufacturing employment, poverty rates, and unemployment rates are not significantly associated with within-city changes in violent crime rates. Moreover, controlling for these economic deprivation/labor market structure variables does not materially affect the magnitude of the association between immigration and violent crime. Thus, these results offer little support for the argument that immigration affects crime by altering economic deprivation or labor market opportunities in large U.S. cities.[10]

In the fourth model, we examine the thesis that immigration may contribute to changes in the prevalence of the illegal drug trade, with consequences for violent crime rates. Consistent with the viewpoint that within-city changes in drug markets are factors in over-time changes in homicide (see Ousey and Lee 2002, 2004, 2007), the results suggest that increases in drug arrests are significantly associated with increases in the overall violent crime rate as well. However, despite this finding, there is little evidence that drug markets serve as a main process behind the longitudinal immigration-violent crime association. Indeed, the direct effect of immigration on violent crime changes very little between Models 3 and 4. Thus, although changes in drug markets may be an important piece of the puzzle regarding violent crime trends since 1980, they do not appear to be major intervening mechanisms that link within-city changes in immigration to changes in violent crime rates.

Controlling for the impact of within-city changes in formal social control capacity—as measured by police officers per capita—also appears to have little efficacy in explaining the immigration-violent crime relationship. As shown in Model 5, the results indicate that within-city over-time variation in police officers per capita has little association with temporal change in violent crime rates and no impact on the immigration-violent crime coefficient. Thus, our models do not support the thesis that immigration has influenced the number of police officers on the street, which in turn drove inter-decade changes in city-level violent crime rates (but see Levitt 2004; Marvell and Moody 1996).

In the sixth model, we introduce the family instability measure into the equation to assess the hypothesis that immigration contributes to lower crime rates by decreasing the prevalence of family breakdown and increasing two-parent, married households and the concomitant social capital that such family structures bring to communities. Consistent with this thesis, the results indicate that family instability has a positive relationship with the violent crime rate. Specifically, a one unit change in the family instability index is associated with an increase of nearly 433 violent crimes (per 100,000 persons). Both the t-ratio (5.54) and the standardized coefficient (.62) for this variable are far and away the largest in the model, indicating a statistically significant and fairly strong effect of within-city over-time changes in family structure on within-city changes in violent crime rates. Equally important, the results of Model 6 suggest that family structure is an important mediator of the effect of immigration on the violent crime rate. With the inclusion of the family instability index, the negative coefficient for the immigration index decreases by nearly 50 percent and the t-ratio drops from 3.74 to 1.73, indicating the direct effect of immigration on violent crime is no longer significant at the .05 level. In sum, the results from the sixth model are consistent with the hypothesis that increases in the immigrant population lead to less violent crime in large part by altering family

10. While the absence of significant effects for variables like age structure and poverty may strike some readers as unusual, we note that much of the macro-level empirical evidence regarding links between these variables and crime is drawn from *cross-sectional* studies of *homicide* rates. Longitudinal evidence of the effects on broader measures of violent crime is actually quite scarce. Moreover, results from longitudinal research examining the effects of similar age structure and poverty measures on measures of crime are decidedly mixed (cf. LaFree and Drass 1996; Marvell and Moody 1991; McCall, Parker, and McDonald 2008; Ousey and Lee 2002; Phillips 2006b; Worrall 2005).

structure.[11] More specifically, immigration appears to have a dampening influence on family instability, which in turn, lowers violent crime rates.[12]

Supplemental Analyses

We conducted several supplemental analyses to ensure the robustness of our findings. First, because it is commonly found to be a correlate of macro-level violent crime rates, we examined whether our findings were affected by the inclusion of a measure of change in the percent black population. We note the initial exclusion of this variable was rooted in theoretical considerations. In our view, the theoretical models we assess do not make a compelling case for its inclusion. More importantly, if percent black were to be included, it is not obvious where within the theoretical elaboration/model-building process it would best fit. Indeed, while percent black is sometimes used as one indicator of economic disadvantage, it could arguably also proxy for immigration—to the extent immigration processes affect racial composition—or simply as another measure of demographic transitions. Likewise, because percent black has long been the main measure of racial threat processes, it can be argued that it should be included either with the drug arrests variable (e.g., the drug war focus on black communities) or with the measure of formal social control (police per capita). In short, the lack of theoretical specificity of percent black suggests that its inclusion with any particular set of explanatory variables may weaken, rather than strengthen, the measurement properties of the theoretical models that are the focus of our analysis. Nevertheless, to determine if our results are contingent on its inclusion, we added it to a final regression model (Model 7), presented in Table 2. Two results are noteworthy. First, both the direct effect of the immigration index and the effect of the family instability index remain significant after percent black is controlled. And second, there is no direct effect of percent black on violent crime. Thus, our results are generally unaffected by whether or not percent black is controlled.[13]

Second, we examined whether our results were sensitive to other specific modeling issues. For instance, one reviewer suggested that because many cities may have very small immigrant populations, our analyses may be unduly affected by the inclusion or exclusion of "low immigration" cities. To examine this issue, we repeated our analyses after alternately imposing a minimum immigrant population criterion of: (1) 2,000 recent immigrants; and (2) 5,000 recent immigrants. Results from each set of supplemental models are substantively identical to those presented in Table 2.

On the advice of another reviewer, we also considered whether there was nonlinearity in the effect of immigration on violent crime. The logic here is that in cities with relatively few immigrants, the impact of a unit increase in the immigration measure may be more dramatic than it would be in cities where the immigrant population base is already substantial. We probed this possibility in two alternative ways. First, we estimated a model that included an interaction between a measure of within-city change in the immigration index

11. Although our primary substantive interest centers on violence, we also investigated whether the immigration-family structure-crime linkage was evident for property crime (measured as an index comprised of burglary, larceny, and motor-vehicle theft). As demonstrated in Appendix B, the immigration index has a significant negative effect on property crime rates, which becomes attenuated and—in this case—completely nonsignificant after controlling for family instability.

12. A reviewer of an early draft of this paper argued that because "family breakdown" increased between 1980 and 2000 (see Appendix A), the notion that immigration was working to bolster intact family structures was not supported. However, it should be noted that changes in the means of the family variables reflect *overall* trends, not immigration-specific trends. In general, divorce rates and single-parent family households did increase over time in large cities in the United States. Yet, as illustrated in Appendix C, our analyses suggest that immigration countered those upward trends to some extent. Stated another way, if the influence of immigration was removed, the increase in divorce and single-parent families across the three time points would have been even more substantial than what is observed in Appendix A.

13. As shown in Appendix B, percent black does have a positive association with the property crime rate that is significant at the .10 level. However, other substantive results in the property models are unchanged by its inclusion.

(deviations from the city-level mean) and a measure of the average level of immigration (the city-specific over-time mean of the immigration index). Second, we computed a model that included the immigration index and its quadratic. Not surprisingly, the results from both models are similar and they suggest that the immigration index has an additive, rather than multiplicative, effect on violent crime. In other words, the effect of a unit change in immigration appears to be fairly constant regardless of whether a city has a relatively small or large immigrant population base.[14]

Finally, because multicollinearity is commonly troublesome in macro-level research, we examined the degree to which high levels of collinearity are evident in our set of explanatory variables. While some explanatory variables are moderately correlated with each other, no correlations are strong enough to suggest a near linear dependency. This conclusion is supported by our analysis of the variance inflation factors (VIFs) computed for the models estimated. For example, in the final model reported as Model 7 in Table 2, the average VIF is 2.35 and all substantive variables have VIFs below 3.4. Only the VIF for the year 2000 dummy variable is above 4 (5.13), and that finding only underscores the importance of controlling for the time-period fixed-effects. Interestingly, once the city- and time-specific fixed effects are partialled out, the VIFs for the substantive explanatory variables are quite low, with an average of 1.51 and a maximum of 2.28. In short, there is little indication of problematic multicollinearity levels that would threaten the stability or interpretability of the results reported above.

Summary and Conclusion

Common belief holds that immigration creates more crime and violence. This belief is rooted in the notion that either individual immigrants have a greater propensity for violent criminal behavior than natives or that an influx of foreigners disrupts existing mechanisms of social regulation. While this belief has held firm in the public conscience, an accumulated body of research has tested the idea that the foreign born and/or their offspring are more involved in criminal behavior than natives. Findings on this question of "immigrant criminality" generally contradict the popular belief that immigrants are particularly crime prone. In fact, much work suggests that first-generation immigrants engage in less criminal activity than natives.

In contrast to the relative clarity of findings from research at the individual level, there exists more uncertainty on the question of the macro-level impact of immigration on crime. While the logic of social disorganization theory, at least as traditionally conceptualized, has long provided a scientific basis for the expectation that immigration causes crime, empirical assessment of that hypothesis as well as other theories of the immigration-crime nexus has been limited. Indeed, despite a surge in immigration-crime research since 2000, the extant body of empirical work remains relatively small and to date has produced a somewhat inconsistent pattern of results. Moreover, while much theorizing about the macro-level connection between immigration and crime is founded on the notion that immigration is a process of *change* that affects crime rates by altering the demographic, economic, and social organization of society, virtually all empirical findings are based on cross-sectional analyses that do not measure over-time change in immigration, crime, or other relevant social factors.

To begin addressing these limitations, the current study pooled 1980, 1990, and 2000 Census data on crime, immigration, and various demographic, economic, and social factors for 159 large U.S. cities to assess the nature of the longitudinal relationship between immigration and violent crime. Using a fixed-effects panel data regression approach, we first

14. All supplemental analyses described herein are available from the first author.

investigated whether within-city, over-time change in immigration was associated with within-city change in a violent crime index. We then examined the efficacy of several alternative theories on the link between immigration and crime by assessing whether changes in factors such as demographic structure, economic deprivation, labor markets, illegal drug markets, police force capacity, and family structure could account for the observed longitudinal immigration-crime association.

Our analyses yielded a number of key findings. First, unlike the long-held popular view that immigration is a major factor contributing to higher crime rates, our results suggest the opposite. The baseline regression models indicate that within-city change in immigration has a significant negative association with within-city change in violent crime. In other words, on average, cities that experienced increases in immigration from 1980 to 2000 experienced a decrease in violent crime rates. Second, while our results show the measure of illicit drug market arrests has a positive association with changes in crime rates, that consistent pattern of results is not repeated for most of the measures of demographic transitions, economic deprivation and labor market structure, or formal social control. Indeed, among the variables reflecting those arguments, we find that only the percent employed in professional and managerial occupations shows any consistent association with within-city changes in violent crime rates. Third, our analyses indicate that the city-level longitudinal immigration-crime relationship is not explained by the demographic transition, economic deprivation, drug market, or formal social control theoretical arguments evaluated in our analysis. We continue to find evidence of a moderate negative relationship between within-city change in the immigration index and within-city change in violent crime after controlling for percent young males, population size, residential instability, economic deprivation, labor market characteristics, illegal drug market activity/arrests, and the relative size of the police force.

Our analyses do, however, suggest that the family structure/social capital theoretical framework offers an important clue to the longitudinal immigration-crime relationship. As predicted by that framework, we find evidence that changes in family structure are an important factor linked to changes in violent crime. Equally important, controlling for changes in family structure substantially mediates the within-city immigration-crime relationship. That is, it appears the negative relationship between immigration and violent crime is due, in part, to the fact that immigration is negatively associated with divorce and single-parent families, which in turn, are positively related to violent crime rates. We believe these results buttress "immigrant revitalization" arguments that have appeared in recent scholarship on the impacts of immigration (Lee and Martinez 2002). At the same time, we acknowledge there is a plausible alternative interpretation of the results we've presented. Namely, it can be argued that our analyses show simply that after all relevant controls are included there is a modest and marginally significant negative effect on violent crime, with no direct effect observed for property crime. Although our interpretation of the data is intuitively appealing because it identifies a logical mechanism by which immigration may affect crime rates at the macro level, this alternative interpretation is also viable and cannot be rejected on empirical grounds.

On face value these findings support some tentative conclusions. One is that violent crime is not a deleterious consequence of increased immigration. Rather the results are consistent with Sampson's (2006) recent speculation that immigration may be a key factor contributing to the crime drop of the 1990s. Thus, in line with the individual level finding that immigrants are less inclined to commit crime than the native born, our work suggests that the macro-level process of immigration may have notable protective effects with regard to crime. A second conclusion is that immigration also may have beneficial impacts on important social institutions. While our findings do not indicate any clear influence of immigration on city economies, there certainly is evidence to support the notion that immigration may bolster the family by increasing two-parent families and lowering divorce rates in U.S. cities. Indeed, in a supplemental analysis shown in Appendix C, we find evidence that

net of controls for time trends, changes in the immigration index are negatively related to family instability in our sample of cities. These results are somewhat consistent with findings reported in other studies. For instance, in their multi-level analysis of data from Chicago, Sampson and colleagues (2005) report that having married parents is one of the key protective factors that help explain black/white and Mexican American/white gaps in violent offending—although they do not find that having married parents mediates the effect of immigrant status on offending.

While these conclusions suggest a positive outlook with regard to the impacts of immigration, we point out that additional research on the connections among immigration, family structure/family values, and crime is needed. Some scholars, in fact, are sharply critical of the notion that immigration is driving a revitalization of the kinds of traditional family structures and values that otherwise have been steadily declining in the United States (Mac Donald 2006). The crux of this counterargument is that due to Americanization, out-of-wedlock childbirth is quickly becoming normative among immigrants. Thus, one speculation is that as recent immigrants grow older and produce successive generations, pro-family cultural elements will be eroded by their sustained exposure to relatively high levels of deprivation, with the end result being lower marriage rates and higher rates of single parenthood. Along these lines, some studies find that later-generation Mexican Americans are less likely to be married than comparable generation non-Hispanic whites (Oropesa and Landale 2004). Simply put, immigration is a complex issue and though our results support the view that immigration has beneficial consequences in the immediate term, it remains an open question as to what the longer-term results will be once recent immigrants become increasingly socialized into American culture.

While we believe our study has begun to fill important gaps in the literature, there are several directions that future studies could take to expand upon our efforts. First, our work suggests that immigration has a negative influence on the change in violent crime rates but our analysis was unable to determine if those effects are general, experienced equally by the entire population, or are limited to certain population subgroups. Although our measures were computed on the basis of the total population, much insight may be gained if future studies are able to investigate whether there are differences in the effects of immigration on family structure and crime rates between different ethnic/racial groups or between immigrants and nonimmigrants. Likewise, an important extension to the current research involves attempting to isolate if the apparent beneficial impact of immigration differs by sending country or immigrant group. Clearly, there are cultural and skill differences between immigrants coming from, for example, the Caribbean, South America, Asia, and Eastern Europe. The extent to which those differences translate into varied impacts on crime as well as intervening demographic, economic, and family structures remains unclear.

Future research should also investigate the degree to which the longitudinal macro-level relationship between immigration and crime is affected by the "immigrant generation" issue discussed earlier. As individual-level studies have suggested, second- and later-generation immigrants become more Americanized in terms of their involvement in crime than first-generation immigrants. Thus, research that attempts to dissect the unique effects of over-time changes in first- and later-generation immigrant population bases on social organization and crime would be an essential next step in the development of the macro-level literature on the immigration-crime nexus.

Finally, given that past work suggests the protective effects of immigrant communities often are a result of the existence of enclaves, an important extension of the current research would be to investigate whether the impact of immigration on the change in crime rates is contingent upon the extent to which enclave characteristics are evident in a city. Do cities that have immigrant enclaves have especially lower crime rates? If so, what aspects of enclaves inhibit criminal activity? Answering these questions will go a long way towards empirically testing a key argument theorized for the negative immigration-crime relationship.

Appendix A

Table A1 • *Means and Standard Deviations of Variables in the Analysis*

	1980		1990		2000		Within-City Change[a]	
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*
Violent crime rate	1705.73	880.43	2804.60	1458.72	2478.94	1198.17	−3.14	751.20
Immigration index	−1.01	2.06	−.28	2.78	.61	1.17	.004	.895
Percent recent foreign born	3.53	4.31	5.23	6.15	6.84	5.48	.011	2.14
Percent speak English poorly	2.87	4.94	4.16	6.08	6.04	6.55	.006	1.91
Percent Latino	9.25	13.75	12.14	15.92	17.39	18.19	.019	4.63
Percent males aged 15 to 34	18.85	2.53	17.63	2.32	15.89	2.25	−.011	1.43
City population	333,173	639,026	361,498	670,951	393,244	729,661	.001[b]	.120[b]
Residential instability	50.90	8.25	48.10	7.23	51.31	5.96	−.016	6.92
Percent poverty	13.65	5.13	16.45	6.21	16.55	5.88	−.005	2.90
Unemployment rate	6.79	2.75	7.38	2.64	7.29	2.71	−.002	1.10
Percent employed manufacturing	19.92	9.27	15.49	6.66	12.42	5.75	−.022	3.84
Percent professional/managerial occupations	23.65	5.43	27.28	6.36	33.53	8.31	.018	4.47
Sale cocaine/opiates arrest rate	15.52	27.06	140.28	186.24	88.05	179.44	−.004[b]	1.28[b]
Police officers per capita	2.04	.82	2.15	.86	2.33	.90	−.0003	.276
Percent black	19.22	17.08	21.03	18.24	22.03	19.19	.0095	2.53
Family instability index	−1.18	1.12	.28	1.30	1.17	1.46	−.005	1.08
Percent divorced	8.20	1.71	10.20	1.94	11.10	2.08	.011	1.40
Percent families single-parent headed	23.30	6.84	28.34	8.17	32.49	9.02	−.006	4.20

[a]Refers to variables expressed as deviations from their city-specific, over-time mean (i.e., "group-mean centered").
[b]These values refer to the log transformed measures.

Appendix B

Table B1 • *Fixed-Effects (Within-City) Linear Regression Models Predicting Property Crime Rates, 1980–2000*

	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*	*Model 7*
Predictors							
Immigration index	−470.57*	−537.67*	394.48*	−399.71*	−399.47*	−181.08	−188.87
Percent males aged 15 to 34		110.16	39.59	15.60	14.79	18.18	23.45
City population (Ln)		427.57	−614.06	−683.61	−651.45	−53.55	82.36
Residential instability		12.10	13.84†	12.67	12.71	10.28	10.28
Percent below poverty			−107.44*	−117.22*	−116.41*	−103.26*	−107.91*
Unemployment rate			72.37	91.62	90.41	76.06	82.43
Percent manufacturing			10.97	10.77	10.89	26.67	26.38
Percent professional/managerial			−41.11	−44.35	−43.98	30.72	29.16
Drug market arrests (Ln)				173.45*	173.05*	159.63*	151.58*
Police officers per capita					35.67	−40.23	−100.09
Family instability index						1477.69*	601.10*
Percent black							49.51†
Time Trend Dummy Variables							
Year 1990	208.83†	384.52*	763.38*	448.73	439.06	−1044.67*	−898.55*
Year 2000	−1528.51*	−1163.36*	−658.20	−835.10	−858.12	−3640.35*	−3406.45*
Model Summary Information							
R^2 (Within-unit)	.520	.526	.550	.569	.569	.595	.595
Corr (u_i, Xb)	−.356	−.396	−.496	−.507	−.498	−.123	−.123
Total number observations ($N \times T$)	463	463	463	463	463	463	463
Total number cities (N)	159	159	159	159	159	159	159

*$p < .05$ †$p < .10$ (two-tailed tests)

Appendix C

Table C1 • *Fixed-Effects Estimates of the Effect of the Immigration Index on Family Instability Measures, 1980–2000*

	Family Instability Index	*Percent Divorced*	*Percent Single-Parent Families*
Immigration index	−.359*	−.653*	−.603*
Year 1990	1.73*	2.45*	5.55*
Year 2000	2.94*	3.95*	10.28*
Model summary information			
R^2 (Within-unit)	.862	.821	.832
Corr (u_i, *XB*)	−.335	−.463	−.062
Total number observations ($N \times T$)	463	463	463
Total number cities (N)	159	159	159

*$p < .05$ (two-tailed tests)

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DOES IMMIGRATION INCREASE HOMICIDE? Negative Evidence From Three Border Cities

Matthew T. Lee
University of Akron

Ramiro Martinez, Jr.
Florida International University,
National Consortium on Violence Research,
National Institute of Justice Du Bois Fellow

Richard Rosenfeld
University of Missouri—St. Louis,
National Consortium on Violence Research

Understanding the complex relationship between immigration and crime was once a core concern of American sociology. Yet the extensive post-1965 wave of immigration to the United States has done little to rekindle scholarly interest in this topic, even as politicians and other public figures advocate public policies to restrict immigration as a means of preventing crime. Although both popular accounts and sociological theory predict that immigration should increase crime in areas where immigrants settle, this study of Miami, El Paso, and San Diego neighborhoods shows that, controlling for other influences, immigration generally does not increase levels of homicide among Latinos and African Americans. Our results not only challenge stereotypes of the "criminal immigrant" but also the core criminological notion that immigration, as a social process, disorganizes communities and increases crime.

Understanding the social consequences of immigration and urbanization were instrumental concerns of those who created American sociology as a distinct academic discipline (M. Waters 1999). The founders of the Chicago School of sociology were troubled by the urban problems linked to newcomers, and early sociologists wrestled with the connections between immigration and crime. This anxiety produced some of American sociology's most enduring conceptual and empirical work, including classics on social disorganization (Thomas and Znaniecki 1920; Shaw and McKay 1931) and culture conflict (Sutherland 1934; Sellin 1938). Despite the potentially significant effect of immigration on current levels of crime and the high-profile role of this relationship in the history of the discipline, contemporary sociological studies of the link between crime and immigration are scarce.

Direct all correspondence to Matthew T. Lee, Dept. of Sociology, University of Akron, Akron, OH 44325-1905; e-mail: mlee2@uakron.edu

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In this study, we evaluate the effects of recent immigration on the presence of homicide in urban neighborhoods.[1] In addition to its explicit focus on immigration, our study is based on four distinctive analytic approaches. First, the data were purposively drawn from three "border" cities—El Paso, Miami, and San Diego—all of which are major destination points for recent (legal and nonlegal) immigrants and places with some of the longest, most consistently sustained waves of immigration in U.S. history (INS 1987; Martinez and Lee 2000b). The availability of data from three distinct sites allows us to isolate the effects of each city's local context on homicide. These cities exhibit both similarities and differences along key dimensions such as economic deprivation, family structure, the presence of new immigrants, and the level of homicide and can therefore reveal variations in local patterns that are obscured in studies based on aggregated national data sets or on data from a single city. Second, we disaggregate homicide data to the level of the census tract, a meaningful unit of analysis (that roughly approximates neighborhoods) for within-city comparisons, particularly those involving crime (Crutchfield 1989; M. R. Lee 2000).[2]

Third, because the macrosocial factors that influence homicide vary across racial/ethnic groups (Ousey 1999), ethnically disaggregated homicide data were culled directly from each city's homicide investigation unit, which permits the linking of group-specific homicides aggregated to the tract level with census-derived population characteristics. Our goal is to assess the effects of immigration and other population characteristics on the levels of homicide of two historically disadvantaged ethnic groups in the three cities, Latinos and African Americans. Non-Latino whites are only briefly discussed in this investigation because they are less likely than blacks and Latinos to live in communities with elevated levels of deprivation, instability, and criminal violence, that is, the types of communities where recent immigrants tend to concentrate.

Finally, we employ an appropriate estimator to analyze multivariate patterns in homicide: Poisson regression (Long 1997). Ordinary least squares, which dominates previous studies of urban homicide, may not be robust against the highly skewed and truncated distribution of homicide events across areas within cities and therefore may produce biased estimates of the effects of community characteristics on homicide.

SOCIOLOGICAL IMAGES OF IMMIGRANTS AND CRIME

Even though studies of European immigrants in the early twentieth century found that immigrants were generally *less involved* in crime than natives, the stereotype of the criminal immigrant has persisted (Ferracuti 1968; T. Waters 1999; Martinez and Lee 2000b). But with few exceptions (e.g., Hagan and Palloni 1998, 1999), researchers have not investigated the criminal involvement of the post-1965 wave of non-European, largely Latino immigrants. The few available contemporary studies echo the findings from the early research, that immigrants are not particularly crime-prone relative to the native-born, even though they experience deleterious social conditions rivaling or surpassing those of the most disadvantaged native groups (Butcher and Piehl 1998; Hagan and Palloni 1998; Martinez and Lee 2000a). Yet public opinion of recent immigrants remains mixed, at best, and stereotypes endure that provide ideological support for restrictive immigration legislation (Butcher and Piehl 1998; Martinez and Lee 2000b).[3] For example, section 1 of California's controversial Proposition 187 declares that resi-

dents "have suffered and are suffering personal injury and damage caused by the criminal conduct of illegal aliens in this state" (quoted in Butcher and Piehl 1998, p. 457).

Ironically, well-established sociological perspectives on crime provide the theoretical underpinnings for the popular image linking immigration to crime. Although few scholars today take seriously the once popular view of immigrants as biologically deficient and therefore inherently criminally disposed, nearly all of the major theories of crime predict that recent immigrants should exhibit high levels of criminal involvement. We briefly sketch the implications for the immigrant-crime link of the three leading perspectives on the social sources of crime and delinquency: opportunity structure, cultural theories, and social disorganization (for more extensive discussions, see Bankston 1998; Martinez and Lee 2000b).

Opportunity Structure

Opportunity structure theories stress the material and social structures that shape the values and activities of different social groups (Bankston 1998). Because legitimate opportunities for wealth and social status are not equally available to all groups, some will "innovate" by taking advantage of available illegitimate opportunities. This type of explanation, in Robert K. Merton's (1938) original formulation, draws attention to the ways in which disadvantaged groups (which often include recent immigrants) may be denied the legitimate means (education, decent jobs) to attain culturally prescribed success and goals (a middle-class lifestyle). Applied to homicide, this perspective suggests that blocked opportunities may result in "instrumental" killings that are fundamentally related to economic motives, such as those committed during the course of robberies, arising out of conflicts over the protection of gang or drug "turf," or as retaliation for thefts, unpaid loans, or gambling debts. Many, if not most, homicides are "expressive" in nature, that is, they are caused by social and emotional, rather than strictly economic and instrumental, reasons. However, Mertonian strain theory can help make sense of these homicides as well, since frustration associated with the means/goals disjunction has been a prominent explanation for violence, which can be understood as an expression of this frustration. In their macro level extension of Merton's theory, Steven F. Messner and Richard Rosenfeld (2001) contend that criminal violence results from an anomic cultural orientation that encourages people to pursue their goals by any means necessary, coupled with a weakening of the external restraints on criminal activity associated with family, educational, political, and other noneconomic institutions.

Scholars have long noted the tendency for new immigrants to settle in urban neighborhoods characterized by poverty, substandard housing, poor schools, and high crime rates (Thomas and Znaniecki 1920; Shaw and McKay 1931; Taylor 1931; Alba, Logan, and Bellair 1994; Hagan and Palloni 1998). Segregated in such neighborhoods, typically composed of earlier arrivals and long-standing residents from the same group, immigrants might be expected to turn to crime as a means to overcome economic opportunities blocked by language barriers, a lack of well-paying jobs in close proximity to home, and possibly overt discrimination. Historical examples abound of immigrants turning to organized crime to gain a foothold in political and economic networks from which they would have otherwise been excluded (Whyte 1943). Other writers have suggested that previously noncriminal immigrant groups may be "contaminated" by the abundance of criminal opportunities provided by natives in the neighborhoods in which immigrants

settle (Lambert 1970, p. 284; cf. Sampson and Lauritsen [1997], on the "proximity" hypothesis). According to the opportunity-structure view, immigrant criminality is a function of preexisting structural factors such as poverty (Yeager 1997), a preponderance of young, unattached males (Gurr 1989), or the availability of alcohol (Alaniz, Cartmill, and Parker 1998).

Cultural Perspectives

To this list of structural sources of immigrant criminality, scholars have added cultural influences on criminal involvement and on immigrant crime in particular. The "culture of poverty" thesis—low-income people adapt to their structural conditions in ways that perpetuate their disadvantaged condition—is a prominent example of a cultural explanation for the high levels of crime and violence among disadvantaged groups (Lewis 1965). Thus, engaging in crime as a means of acquiring social status draws children away from schoolwork, which then reduces the probability of future economic advancement. Recent work has stressed the connections between "normative inversions" and changeable structures of inequality, in contrast to viewing criminogenic norms as inherent in the culture of specific racial/ethnic groups (M. R. Lee 2000, p. 193; see also Bruce 2000, p. 181, on the "normative manifestations" of structure). Because immigrants, like ethnic minorities, are generally more likely than native whites to reside in areas where structural conditions have altered the status systems away from idealized middle-class norms and toward a culture of opposition (Anderson 1999), these cultural theories imply that immigrant communities should exhibit high rates of crime (Miller 1958; Wolfgang and Ferracuti 1967).

Another prominent strain of cultural theory that is especially well suited to the immigration-crime relationship developed out of Thorsten Sellin's (1938; see also Sutherland 1934) writings on culture conflict. Sellin (1938, p. 21) recognized that the criminal law reflects the values of the "dominant interest groups" in society and that the values of other social groups, particularly immigrants, were sometimes quite different. In cases in which the cultural codes of subordinate and dominant groups conflict, legal agents label as deviant the behavior of members of the subordinate groups. Nevertheless, the criminal may be acting according to subculturally accepted norms and feel no "mental conflict" when violating the law. Thus, immigrants may violate the law more often than natives due to conflicts at the level of cultural codes and associated problems of acculturation in a new environment characterized by heterogeneous conduct norms (see Y. H. Lee 1998, for a recent study of acculturation and delinquency).

Social Disorganization

The social disorganization perspective, while not denying the effects on crime of cultural influences or the structure of economic opportunities, adds to the other perspectives a concern with the breakdown of community social institutions that results from social change. Robert J. Bursik (1988, p. 521) concisely describes disorganized areas as possessing an "inability to realize the common values of their residents or solve commonly expressed problems." In organized neighborhoods, local institutions (families, schools, churches, businesses, social agencies) work together to realize community goals, protect values, and limit the criminal behavior of community members, especially the young. In

addition to making the realization of common values more difficult, a key source of social disorganization is population heterogeneity, which impedes communication and cooperation among distinct cultural groups (Bursik and Grasmick 1993). Carl L. Bankston (1998) notes that immigration may destabilize local institutions by increasing population turnover. The implication is that, when institutions weaken, social control is diminished and crime increases.

William I. Thomas and Florian Znaniecki (1920) set forth one early influential statement of this perspective in their five-volume classic published in 1918–1920, *The Polish Peasant in Europe and America*. They wrote about the many social changes affecting Polish immigrants a century ago, including the disorganizing influences inherent in moving from simple, homogeneous, rural areas of Poland to the complex, heterogeneous, urban areas of the United States. They defined social disorganization as "a decrease of the influence of existing social rules of behavior upon individual members of the group" (Thomas and Znaniecki 1920, p. 2). The effectiveness of social rules, including criminal law, derives from the individual's investment in them in the form of attitudes favorable to conformity. In organized groups, congruence exists between group rules and individual attitudes. Disorganization indicates a gap between rules and attitudes, such that an individual does not feel bound by the rules and is free to disobey them.

Viewed in this light, disorganization was a neutral term emphasizing both the positive and negative consequences of social change. Individual liberation from oppressive community standards, for example, is a positive consequence. However, the concept of disorganization generally has been applied in studies of crime and other social problems. An important contribution of this classic disorganization perspective is the recognition that crime is not only a function of enduring structural (e.g., poverty) or cultural (e.g., "subculture of violence") forces, but it is intimately tied to the fundamental processes of social change. As a major agent of social change, therefore, immigration should inflate levels of crime due to its disorganizing influence on community institutions.

THE COUNTERCLAIM

There are good sociological reasons, then, for anticipating a positive association between levels of immigration and crime. Immigration may be criminogenic through its association with economic disadvantage, conflicting conduct norms, weakened social control, or a combination of economic, cultural, and institutional factors. There are equally good reasons, however, for questioning the standard sociological images of immigration and crime. In fact, current public discourse on the effect of immigration on urban areas is beginning to portray immigration as a positive, stabilizing force. For example, on a recent broadcast of *Talk of the Nation* on National Public Radio (2001, pp. 2, 16), William Frey, a demographer with the University of Michigan, articulated the position that cities with a high proportion of recent immigrants are experiencing a rebirth:

> Immigrant cities and the high-tech cities are the ones that are going to be the shining stars of the future. . . . The ones that are breathing and growing . . . and living are the immigrant cities, the immigrant magnet cities. This is what had made cities to begin with in this country, and I think a lot of these areas are the ones that are really going to breed that diversity and that vitality and that sense of community in ways that many of the other cities don't have.

Far from being a disorganizing and possibly criminogenic force, this view posits immigration as an essential ingredient contributing to the continued viability of urban areas that had experienced population decline and community decay in previous decades.

In this article, we evaluate the counterclaim to the proposition that immigration increases crime. Strong ties to the labor market and family, we argue, offset the potential crime-producing consequences of disadvantage, culture conflict, and community instability and thereby suppress the level of crime in immigrant communities. Specifically, we test the hypothesis that *communities with higher proportions of recent immigrants, other things being equal, will exhibit lower levels of criminal homicide.*

Although several recent studies have called into question the immigration-begets-crime thesis (Butcher and Piehl 1998; Hagan and Palloni 1999; Martinez 2000), and even classic works failed to find a consistent empirical link between immigration and crime (T. Waters 1999; Martinez and Lee 2000b), scholarship that directly investigates the contemporary immigration/crime link is currently in short supply. It is certainly plausible to hypothesize that noncitizen immigrants may be deterred from crime by the potential threat of deportation, thus helping to suppress crime in immigrant neighborhoods.[4] But in addition to this line of reasoning, existing research unrelated to crime offers substantial indirect support of our counterclaim. For example, Alejandro Portes and Alex Stepick (1993) found that, rather than disorganizing communities, immigrants stabilized and revitalized Miami's economic and cultural institutions. The implication is that community social control may actually be strengthened by immigration, an image that is at odds with criminological theory. In fact, sociologists have long argued against the "myth" (Portes 2000, p. 5) that residents of impoverished urban areas are poor (or victimized by criminals) because they are "disorganized" in the sense that they lack common values or strong ties to each other.

Turning to attachments to the labor market, Pidi Zhang and Jimy Sanders (1999) found that immigrants with low-paying jobs may have a greater work incentive than similarly situated natives due to different frames of reference. In effect, the experience of being socialized in a relatively impoverished homeland results in immigrants placing a greater value on jobs than natives who were socialized in the richer host country. Immigrants therefore work longer hours than natives, even coethnic natives (e.g., Mexican immigrants work more hours than American-born Latinos); they also perceive more opportunities for upward mobility, which in turn may lead to high rates of upward mobility vis-à-vis similarly situated native groups (Zhang and Sanders 1999). These perceptions are shaped by the availability of social networks and opportunity structures, with immigrants possibly having greater access to these resources than segregated native-born black populations. For example, some immigrant groups (e.g., Koreans) arrive in the United States better educated than the average native-born American, and therefore more qualified to find jobs in the primary labor market (Alba and Nee 1997). In some cases, immigrants have structural supports that natives lack, such as "transnational networks" or the existence of "ethnic enclaves" that facilitate social and economic stability (Portes 1997; Logan, Alba, Dill, and Zhou 2000). Finally, some immigrant groups are particularly entrepreneurial, such as Cubans in Miami or Koreans in southern California (Logan et al. 2000). All of this implies that immigrants may not display cultural adaptations to structural inequality (i.e., crime) to the extent of disadvantaged native groups, because of immigrants' social capital or differential experiences of relative deprivation.

Another suggestive line of research focuses on perceptions of social justice (NCOVR 2000). The guiding hypothesis of this work is that individuals who believe society is fundamentally unjust will be more likely to break the law than those who believe that society is generally fair. For example, African Americans' experiences of racism and inequality may lead to a sense of social injustice, which in turn plays a mediating role in this group's disproportionate involvement in crime. Immigrants, while also subject to discrimination and deleterious social conditions, may perceive less injustice than African Americans due to different socialization experiences or perhaps to a greater sense of optimism about their prospects for advancement in American society. While immigrant-specific data on this issue are not available, one recent study of southern California found that Latinos were more likely than either whites or African Americans to believe in a just world (Hunt 2000).

These examples are part of a growing body of literature that supports images at odds with standard criminological notions of immigration as a process that facilitates crime. Given the potential for refining theory and informing policy—coupled with the fact that the experiences of recent immigrants (criminal or otherwise) cannot be inferred from those of previous waves (Portes 1997)—an empirical test of our counterclaim is long overdue. We therefore assess the effect of immigration on homicide across three cities that serve as major destinations for immigrants. We focus on Latino and non-Latino black homicide, since these two groups exhibit elevated homicide involvement, high levels of economic disadvantage, and other deleterious structural conditions. Immigrant Latinos also tend to settle in older Latino neighborhoods, many of which are expanding into adjacent non-Latino black communities. Although non-Latino whites are not an explicit concern in this article, we do report results for white models in a note. Our approach allows us to examine differences in the direction and magnitude of the immigration effect for the different ethnic groups and for ethnic differences in the covariates of homicide, such as economic disadvantage and community instability.

In addition, by estimating the effect of immigrant populations, comprised largely of Latinos, on the homicide risk of African Americans, we can assess the *contextual* influence of immigration in urban communities. Immigration may directly influence the crime rates within the immigrant population, but it also may indirectly affect those of other groups by changing the community context in which immigrant and nonimmigrant populations are situated. For example, if high levels of immigration impede communication and consensus on values in a community, social disorganization theory would predict a general increase in crime—and not simply increased criminal involvement of immigrants. The data used in this study, then, enable us to move considerably beyond the narrow focus of prior research on the crime rates of immigrant groups to assess the broader community impacts of immigration.

DATA AND METHODS

In order to assess the effects of immigration and other community characteristics on homicide in the three cities, we gathered data on all criminal homicides (excluding police shootings and excusable homicides) that occurred 1985–1995 directly from the homicide investigation units of the Miami and San Diego police departments and for 1985–1994 in El Paso. We collected all available information on the 3,345 homicides known to police in these three cities during this time period (1,527 in Miami, 412 in

El Paso, and 1,406 in San Diego). Direct access to detailed internal files was essential for distinguishing Latinos from other ethnic groups, which is not fully possible with the FBI's Uniform Crime Reports. The address of the homicide incident is also contained in these files, which enabled us to link each case to data from the 1990 decennial census at the tract level (USBC 1990).[5]

The units of analysis for this study are the 352 census tracts with more than five hundred residents in the three cities: 86 tracts in El Paso, 70 in Miami, and 196 in San Diego.[6] The dependent variables are the black and Latino homicide counts, aggregated to the tract level for the 1985–1995 (1994 in El Paso) period. For reasons explained below, our dependent variable is a "count" rather than a "rate." Thus, we include a measure of the black and Latino populations in our analysis to control for differences in aggregate homicide risk across tracts.

Following the emphasis of social disorganization theory on residential instability as a cause of crime (Bursik and Grasmick 1993), we have constructed an "instability index" that combines measures of vacancy and population turnover. Vacancy rates were computed by dividing the number of vacant housing units by the total number of housing units. The population turnover measure is the number of people who lived in a different housing unit in 1990 than they did five years earlier, divided by the total population. Each of these measures was standardized by subtracting the mean and dividing by the standard deviation; the resulting z-scores were then added to create the index.

We have included the percentage of the population living below the poverty line as a measure of economic deprivation. Since we analyzed tract-level data, we avoided the problem of capturing the spatial clustering of poverty that plagues city-level studies (M. R. Lee 2000). We controlled for the percentage of males between the ages of 18–24 to capture variation in the age group at especially high risk for serious violence. Consistent with past research suggesting that family structure influences homicide (Sampson 1987), we included the percentage of female-headed families with children. Labor market theory (Crutchfield 1989; Crutchfield and Pitchford 1997) suggests that a community's job structure will exert an effect on crime beyond that captured by economic deprivation measures such as the poverty rate. In addition, Latino populations in urban neighborhoods are often characterized as the "working poor." To assess the impact of labor market participation on homicide risk, we included the percentage of jobless males, defined as the number of males not in the labor force divided by the total number of males. Except for the instability index, we employed group-specific measurements for each of these indicators (i.e., black poverty and Latino poverty, family structure, joblessness, and age structure).[7]

Finally, we included a measure of the percentage of "new" immigrants in each tract, defined as those who arrived in the United States in 1980–1990—a period of "explosive" growth in the immigrant population (Logan et al. 2000, p. 99). In line with our previously stated rationale, we controlled for the percentage of foreign-born residents arriving within the past ten years in a given tract. This is not an attempt to measure legality or citizenship—the data do not allow those distinctions—but rather to assess the effects of recent settlement into well-established barrios or enclaves, typical destination points for newcomers.[8]

For reasons stated earlier, although sociological *research* has generally failed to find a link between immigration and crime, sociological *theory* offers ample and rather compelling reasons for supposing that immigration produces crime by increasing disadvantage, weakening community controls, and heightening culture conflict. Moreover, politicians, pundits, and pseudo-academics (Lamm and Imhoff 1985; Tanton and Lutton

1993) have suggested that the newest immigrants are a crime-prone group, a claim seized on by the media with regard to episodes such as illegal border crossings or the 1980 Mariel boatlift in Miami (Martinez 1997). Additional research is necessary, therefore, to assess the adequacy of both the theoretical and popular images of the immigration-crime connection. The current study is the first to examine the impact of recent immigration on homicide risk, independent of other variables known to influence homicide, such as economic disadvantage, residential instability, and age structure, at the neighborhood level across three high-immigration cities.

Estimation Procedures

Homicides are rare events distributed across ecological units such as census tracts in a highly skewed manner. Traditional ordinary least squares regression is not an appropriate estimator for such distributions when transformations of the dependent variable (such as logarithmic transformation) do not induce normality. To accommodate the nonnormal distribution of homicides, we employed a maximum likelihood estimator that is designed to deal with counts. Specifically, homicides are conceptualized as counts of events per unit of time, a description suggesting that use of a Poisson random component (or the related negative binomial specification) is most appropriate (Kohfeld and Sprague 1988; Osgood 2000).[9]

In Poisson regression, parameters are estimated through a maximum likelihood procedure that has a number of desirable properties, including an asymptotic distribution, consistency, and efficiency (Fienberg 1984). Further, the Poisson coefficient divided by its standard error follows a standard t-distribution. Therefore, statistical significance as well as the direction of the effect of each independent variable can be assessed. The general analytic strategy is to assess the independent effect of immigration on the frequency of homicides across census tracts within each of the three cities, controlling for aggregate risk associated with group population size (logged), the structural covariates described above, and spatial autocorrelation in the homicide distributions.

Spatial autocorrelation refers to nonrandom clustering in homicides that is not a function of the explanatory variables in the model. Such clustering may result from diffusion or spillover processes in which high rates of violence in one community result in increases in adjacent or nearby communities (Anselin, Cohen, Cook, Gorr, and Tita 2000). Alternatively, negative spatial autocorrelation, in which high homicide counts in a tract are associated with low counts in nearby tracts, might result from enforcement activity or other conditions that displace crimes across neighborhood boundaries. In other words, because a given tract's homicide level may be influenced by processes external to tract boundaries, researchers must statistically control for these processes or risk incorrectly attributing their effects to internal conditions, such as tract-specific levels of poverty or immigration (see Cliff and Ord [1981] for the classic discussion of spatial autocorrelation models).

While attempts to model external effects represent a relatively recent development in the criminological literature, Kenneth C. Land and Glenn Deane (1992) offer an accepted strategy. In this article, we follow their advice to model spatial autocorrelation in two stages. In the first stage, a "spatially lagged dependent variable" (Land and Deane 1992, p. 237) was created by obtaining the fitted values of the dependent variable (homicide) from a Poisson (or negative binomial) regression on the independent variables. These fitted values were transformed into a spatial lag variable, which was then

included with the independent variables in a second-stage regression.[10] The results from the second-stage models are reported here.

For each of the cities we compare models of homicides involving Latino and African American victims. In this way, we can detect the direct influence of immigration in the population groups in which the immigrants are concentrated—Latinos—and the contextual influence of immigration in a nonimmigrant group with an elevated risk for homicide—African Americans. Any differences in these comparisons connected to the larger urban context also can be observed, as well as similarities and differences in the effects on Latino and African American homicide of the neighborhood-level structural covariates.

RESULTS

Table 1 presents descriptive statistics for each of the variables in the analysis. The average number of homicides per tract is higher for Latinos than for blacks in San Diego and El Paso and slightly greater for blacks than Latinos in Miami. These differences reflect, in part, the larger size of the Latino population in the three cities. When converted to

TABLE 1. DESCRIPTIVE STATISTICS

	El Paso Mean (SD)	Miami Mean (SD)	San Diego Mean (SD)
Black homicides	.29 (.59)	9.79 (16.29)	1.95 (4.95)
Annualized rate (per 1,000)	.16	.71	.37
Latino homicides	2.88 (2.84)	8.53 (6.88)	2.78 (6.23)
Annualized rate (per 1,000)	.07	.24	.22
Non-Latino blacks	187 (215)	1259 (1714)	486 (851)
Latinos	4130 (2346)	3179 (2527)	1127 (1399)
Instability Index	−.701 (1.29)	.513 (2.03)	.124 (1.48)
% Poverty			
Black	18.72 (25.73)	38.96 (21.87)	15.80 (19.42)
Latino	29.78 (16.77)	31.95 (16.01)	17.25 (12.89)
% Female-headed families			
Black	19.88 (26.87)	37.01 (30.82)	24.56 (31.11)
Latino	22.54 (9.23)	29.23 (20.71)	19.88 (19.08)
% Male joblessness			
Black	28.98 (28.48)	33.91 (23.89)	18.18 (20.36)
Latino	25.89 (10.43)	29.13 (16.40)	19.08 (13.57)
% Young and male			
Black	8.53 (9.68)	5.27 (5.61)	9.29 (11.00)
Latino	9.14 (2.55)	6.33 (2.86)	10.48 (6.37)
% New immigrants	8.74 (5.80)	26.63 (14.36)	9.62 (8.70)
Spatial lag			
Black	.29 (.06)	9.40 (3.44)	2.13 (.85)
Latino	3.00 (.48)	8.81 (.96)	2.89 (1.12)
Tract N	86	70	196

Note: The homicide values are the tract means for 1985–1995 in Miami and San Diego and for 1985–1994 in El Paso. All other variables (except spatial lags) are from the 1990 census.

average yearly rates per 1,000 tract population, the homicide risk for blacks is considerably higher than for Latinos in each of the cities, a finding that is consistent with previous research (Martinez and Lee 1999). We also observed pronounced differences in risk across the cities for both groups. El Paso has the lowest rates and Miami has the highest, although the difference between San Diego and Miami for Latinos is negligible.

Somewhat less consistent group differences are observed for the poverty, instability, family structure, and joblessness indicators. Poverty rates are higher for Latinos in El Paso, for blacks in Miami, and roughly equal for the two groups in San Diego. Rates of female-headed families are higher for blacks in Miami and San Diego and very similar to those for Latinos in El Paso. Differences between the two groups in joblessness are small in all of the cities, although in El Paso and Miami joblessness is somewhat greater for blacks. We observe no important group differences in the young male fraction of the population.

There are notable differences across the three cities in levels of instability and in the key variable of interest, recent immigration. In both cases, Miami stands out, with instability values several times those for the other two cities and immigration levels almost three times greater. The question to which we now turn is whether recent immigration influences levels of homicide within and across the three cities.

Latino-Specific Models

Consider first the Latino-specific models presented in Table 2. Other than the expected effects for tract population, the only variable with significant effects (at the .01 level) on Latino homicide victimization across all three cities is the poverty rate. Higher levels of poverty are strongly associated with higher levels of homicide. The spatial lag variable is also consistently related to homicide, although only at the .05 level in El Paso and Miami. Specifically, we found that Latino homicide in a given tract is positively related to the characteristics of surrounding tracts, suggesting that a process of diffusion may be at work (Anselin et al. 2000). The instability index is significantly associated with homi-

TABLE 2. POISSON OR NEGATIVE BINOMAL REGRESSION RESULTS FOR LATINO MODELS

	El Paso	Miami[b]	San Diego[b]
Intercept	−9.37** (1.47)	−2.29** (.792)	−5.44** (.953)
(Ln) Population[a]	2.26** (.338)	.762** (.170)	1.32** (.408)
Instability index	.119* (.069)	.002 (.036)	.152* (.085)
% Poverty[a]	.015** (.008)	.032** (.007)	.036** (.011)
% Female-headed families[a]	.010 (.013)	−.012** (.005)	−.015* (.009)
% Male joblessness[a]	.020 (.013)	−.003 (.006)	.005 (.010)
% Young and male[a]	−.003 (.036)	−.040 (.033)	−.008 (.023)
% New immigrants	−.046** (.024)	.000 (.006)	−.007 (.019)
Spatial lag[a]	.483* (.266)	.171* (.095)	.514** (.126)
Log-likelihood (N)	−153.66** (86)	−190.00** (70)	−297.31** (196)

Note: Standard errors are in parentheses.
[a] Group-specific data.
[b] Negative binomial model.
* $p < .05$; ** $p < .01$ (one-tailed).

cide in El Paso and San Diego, with less stable neighborhoods exhibiting higher levels of homicide. The percentage of female-headed families is significantly related to homicide in Miami and San Diego; contrary to expectation, the relationship is negative. Communities with a higher fraction of female-headed families have lower levels of homicide. Neither male joblessness nor the young male measure are linked to homicide levels in the three cities. Finally, net of other influences, the percentage of recent immigrants in the neighborhood is significantly associated with Latino homicide levels only in El Paso, where the direction of this effect is negative.

In sum, the results for the Latino models differ somewhat across the three cities. The only substantively important effect that was consistently significant at the .01 level is for poverty. High poverty rates are associated with higher levels of Latino homicide. The lack of a positive relationship between recent immigration and Latino homicide was also uniform; the two were unrelated in two cities and negatively related in El Paso. Other results are less consistent. One possible interpretation is the presence of multicollinearity in the models, which would produce inflated standard errors and instability in the parameter estimates. Inspection of the correlation matrices and diagnostics for the Latino models indicates the presence of collinearity in the El Paso model involving the measure of female-headed families and the poverty rate ($r = .73$) and also between the new immigration variable and poverty ($r = .79$). An examination of the variance inflation factors (VIFs) indicated that the poverty variable was the potential problem (i.e., VIF > 4). We therefore reestimated the El Paso equation with the poverty rate removed. The only difference in results is that the coefficient for the female-headed family measure becomes significant in the model without the poverty rate. There are no indications of multicollinearity in the other models.[11]

The inconsistent results across the cities, therefore, do not appear to be due to instabilities induced by the correlations among the regressors. In any event, we found no inconsistency for the construct of greatest interest, the percentage of recent immigrants. Thus, immigration is not related to higher Latino homicide levels in these three cities.

Black-Specific Models

The results for the black-specific models are displayed in Table 3. Here we found that the population variable is significant in Miami and San Diego. The lack of relationship for this measure of homicide risk in El Paso is due to unique characteristics of the black population in this city, a subject we address below. The instability index is related to black homicide levels in El Paso and San Diego, suggesting that blacks are less likely to be homicide victims in more stable neighborhoods.

Unlike the Latino models, the poverty rate is associated with black homicide in the expected positive direction in only two of the three cities (Miami and San Diego), although male joblessness is consistently related to higher homicide levels. As with the Latino models, the female-headed family measure does not exhibit the positive relationship with black homicide predicted by the literature; it is negative and significant in San Diego and null in Miami and El Paso. The spatial lag term is significant only in San Diego. In other words, controlling for other influences, San Diego neighborhoods where black homicide risk is high tend to be located close to neighborhoods where it is also high. Finally, the immigration measure is negatively related to black homicide levels in Miami, positively related to black homicide in San Diego neighborhoods, and null in El Paso.

TABLE 3. POISSON OR NEGATIVE BINOMAL REGRESSION RESULTS FOR BLACK MODELS

	El Paso	Miami[b]	San Diego
Intercept	−3.83** (1.47)	−2.27** (.647)	−7.03** (.420)
(Ln) Population[a]	.832 (.557)	1.07** (.165)	1.95** (.145)
Instability index	.291* (.169)	.057 (.049)	.368** (0.65)
% Poverty[a]	−.000 (.011)	.014** (.007)	.013** (.005)
% Female-headed families[a]	.002 (.008)	.001 (.006)	−.006** (.003)
% Male joblessness[a]	.022** (.010)	.009* (.006)	.023** (.004)
% Young and male[a]	−.030 (.030)	.024 (.023)	−.015 (.011)
% New immigrants	−.040 (.052)	−.025** (.007)	.015** (.007)
Spatial lag[a]	2.38 (4.03)	.055 (.046)	.492** (.109)
Log-likelihood (N)	−50.52** (86)	−152.83** (70)	−205.11** (196)

Note: Standard errors are in parentheses.
[a] Group-specific data.
[b] Negative binomial model.
* $p < .05$; ** $p < .01$ (one-tailed).

Inspection of the correlation matrices and regression diagnostics revealed no multicollinearity in the black-specific models.

DISCUSSION AND CONCLUSION

Looking across Tables 2 and 3, we observe a great deal of variation by race and place in the factors that shape group-specific homicide risk within urban communities on the border. However, with only one exception, we found either no relationship or a significant negative relationship between homicide and recent immigration—a trend that also holds for non-Latino white homicide events.[12] The exception is the positive effect of immigration on black homicide in San Diego. One possible interpretation of this result is that homicides in San Diego are more likely to involve intergroup conflicts involving new immigrants and black residents. Another is that new immigrants tend to settle in San Diego communities with preexisting high levels of black-on-black homicide. The latter possibility complements the complex relationships among race/ethnicity, immigration, and crime uncovered by Richard D. Alba, John R. Logan, and Paul E. Bellair (1994) in metropolitan New York. It is plausible that immigrants who settle in predominantly black neighborhoods are not present in sufficient numbers to strengthen local institutions and may in fact be contributing to the disorganization of these areas. Regardless of the explanation, we should point out that San Diego has a low homicide rate relative to cities of a similar size (i.e., over one million people, see Martinez and Lee 2000b).

It is also important not to obscure the general thrust of our results, which in these three border cities does not support either the popular stereotypes of the impact of immigration on crime or the expectations derived from sociological theories.[13] For example, the relationship between immigration and black homicide in Miami provides especially strong support for our counterclaim, both in the statistical results presented here and a separate spatial analysis (M. T. Lee 2000). This research has found that Haitian immigrants in Miami have suppressed homicide levels in the predominantly black areas

in which they have settled, while black areas receiving few immigrants continue to have the highest homicide levels in the city. In fact, Haitians in Miami are among the most disadvantaged groups in the country, experiencing both poverty and discrimination, yet they have exceedingly low homicide involvement (Martinez and Lee 2000a). Our knowledge of urban crime would clearly benefit from additional studies of how groups like Haitians maintain low involvement in crime despite adverse social conditions.

Another fairly consistent finding from our research is that indicators of economic deprivation, labor market involvement, and residential instability are related to Latino and black victimization in the three cities. Unlike our result for immigration, these results are entirely in keeping with the expectations of opportunity structure and social disorganization perspectives on the conditions that give rise to crime in urban areas. The poverty rate is the most consistent of the three indicators, exhibiting a significant, positive association with homicide in five of the six models. Instability is significant in four models and joblessness is significant in three. The latter finding is in line with recent research that suggests Latinos are poor but working, while blacks are more likely to be both poor and jobless, since male joblessness was a significant predictor of homicide in all three black models but in none of the Latino models (Wilson and Martin 1982; Logan, Alba, and McNulty 1994; Waldinger 1996). So, although we find some differences across these indicators for the different cities and ethnic groups, together they provide a theoretically coherent picture of the economic and social conditions associated with elevated levels of violent victimization for urban Latinos and African Americans.

The measures of age and family structures (i.e., young males and female-headed families with children) produced findings that are at odds with predictions from the literature. In no case did either of these variables have the expected positive, statistically significant effect. The lack of a positive association between young males and homicide is consistent with other research (M. R. Lee 2000). The female-headed family variable, long thought to contribute to a host of community problems such as crime, depression, and joblessness, among others, did not independently increase homicide in this study—a finding that is in line with recent research that challenges the conventional wisdom on the "pathology of matriarchy" (Biblarz and Raftery 1999, p. 322; see also Mizell 1999). In fact, in two cities we observed a negative relationship between female-headed families and homicide (Latinos in Miami and blacks in San Diego). Of course, the null findings might simply reflect the covariation of the family structure indicator with the poverty rate. If the primary influence of female-headed families on homicide is through their elevated levels of poverty, then controlling for poverty should eliminate their effect on homicide. Regardless of the empirical indications that single-parent families do not inevitably produce crime or other social pathologies, there is little basis in existing criminological theory or prior research for anticipating a *negative* relationship between female-headed family structures and homicide, with poverty and other measures of economic marginalization controlled.

Although our present focus is on immigration, our results suggest that decomposing the relatively crude measure of family structure employed in most studies of homicide (including this one) offers great potential for theoretical advancement. Researchers have devoted substantial efforts to explicating the complex relationship between material resources and family structure (Corcoran 1995), and homicide research must begin to consider these complexities. For example, there are good reasons for economically disadvantaged women to avoid marriage, with many believing that marriage will not

contribute additional resources to the family unit, although it may have *negative* consequences (e.g., domestic violence, see Edin 2000). In addition, classic research by Carol Stack (1974) suggests that kinship networks may mediate the potentially deleterious effects of female-headed families. By building on the insights of this work, researchers can construct more sophisticated models of the social processes and networks that influence family structure in urban areas, thus contributing to our ability to place crime in its proper social context.

Having explored the similarities and differences across models, we now turn to the special case of black homicide in El Paso, the one example where the idiosyncrasies of race and place superseded many of the expected relationships between homicide and common covariates. Recall that in this model, neither the risk variable (population) nor poverty was significantly related to black homicide. Without an understanding of the social context of the black population of this city, these results would seem counterintuitive. However, El Paso does not have the problem of concentrated black poverty to the same extent as the deindustrializing cities of the Northeast and Midwest—cities on which so much of our knowledge of homicide has been based. In fact, El Paso has a relatively small number of black residents compared to most large cities, and many of them are employed by the military and therefore have stable employment. Although poverty and the number of black residents do not exert independent effects on black homicide, neighborhoods with high levels of black male joblessness and residential "instability" (not a race-specific variable) exhibit comparatively high levels of black homicide. The differences in findings across the three cities reinforce the view that policies designed to reduce violence must consider important differences in the local context in which violence is situated.

Some caveats are warranted. First, in order to focus on immigration as a macrological process this study ignored individual-level issues regarding whether either the perpetrators or victims of homicide were immigrants. We have directed our attention to the conditions of communities that are associated with variation in group levels of criminal violence and made no claims about the individual-level relationships among violence, poverty, immigrant-status, and other factors within communities. Although this approach avoids the ecological fallacy (i.e., the attribution to the individual level of group-level relationships), future work should integrate both individual and community levels of analysis to the extent that data availability will allow. Furthermore, the circumstances under which people immigrate (e.g., political or economic), as well as the segmented assimilation of specific immigrant groups into the core and peripheral economies of urban areas (Logan et al. 2000), likely have important implications for urban crime. The recent immigration of diverse groups of people into large American cities has significantly complicated the relationship between race/ethnicity and violent crime. Yet most studies continue to gloss over this complexity, often dropping high-immigration cities like Miami from their analyses (Parker and McCall 1999). While it was useful to examine the effect of "immigration" on crime, given our current lack of knowledge, future work must attempt to model the effects of specific groups of immigrants, or different segments within a particular immigrant group (e.g., professionals versus laborers), in order to more fully understand the immigration/crime nexus.

Second, distinguishing legal from illegal immigrants might be a potentially useful strategy in crime research and immigration policies (e.g., deportation) that could prevent future violence in many communities. There is however little evidence that "illegal" immigrants are over involved in violent crime, or at least much more so than "legal" immigrants

(Hagan and Palloni 1998), even though it is clear that most immigrants live on the economic margins of society. Also, "illegal aliens" cannot be thoroughly separated from legal ones at the local level. It is difficult to single out a community where illegal aliens outnumber legal immigrants, or even a series of street blocks where "illegals" are distinguishable from others. Instead, many are blended into poor Latino families and neighborhoods. Forcibly separating them from their communities through deportation tends to break up families and weaken social bonds and community cohesion, thereby disrupting local conditions. This policy "solution" thereby wreaks more havoc on disadvantaged communities, adding to the consequences of adverse structural conditions. Rather than encouraging a clandestine life, steps to integrate those without "papers" into legal residents would probably prove less harmful in the long run than the traditional use of deportation.

A third concern relates to the cross-sectional nature of our data. A longitudinal analysis is required to advance our understanding of the causal ordering among variables such as immigration, residential instability, poverty, and the ethnic composition of neighborhoods. For example, it is possible that immigration into a given neighborhood may contribute to an increase in residential instability at a later time, which might then shape subsequent homicide levels in that neighborhood or surrounding areas. Future research should therefore employ a longitudinal design in order to disentangle the causal ordering of neighborhood-level social processes that unfold over a period of years and that influence patterns of violent crime.

Notwithstanding these concerns, the major finding of this article—that recent immigration generally does not increase community levels of homicide—has implications for policies that target immigration as a social problem. Our results offer little support for claims that immigration fosters homicide in three large and ethnically diverse border cities. Indeed, native and immigrant groups alike could profit greatly from research into the counterclaim that immigration can be a stabilizing force that suppresses criminal violence.

In the absence of such a research agenda, claims makers will likely continue to link immigrants with crime. The far-ranging consequences of this discourse, which reappears with each successive wave of immigrants, despite its lack of empirical support (Sellin 1938; Hagan and Palloni 1999; Martinez and Lee 2000b), demonstrate the constitutive power of the symbolic order in shaping racial/ethnic identities as well as the social, political, and economic capital of ethnic groups (O. Lee, 1998). For example, Edward J. Escobar's (1999) history of the Los Angeles Police Department's public campaign to connect Mexicans with crime shows clearly the central role this image played in the formation of a crime-prone Mexican public identity—an identity that justified the continued subordination of this group and even ethnically motivated violence (e.g., the Zoot suit riot). Similarly, the symbolic force of the mythology of the "criminal immigrant" contributes to contemporary criminal justice system practices like racial profiling by law enforcement and harsh treatment by the court system, leading to the incarceration of disproportionate numbers of immigrants despite evidence that immigrants as a group are less crime-involved than natives (Hagan and Palloni 1999).

This study offers an empirical assessment of competing claims about immigration and crime that have significant consequences for both immigrants and natives. Our findings suggest that future research along these lines has much to offer sociological theories of crime, popular understandings, and perhaps most importantly, public policies that bear on the complex relationship between immigration and crime. Sociologists have argued that explanations of urban crime require a "somewhat complex story line" (Alba et al.

1994, p. 417). Our study suggests that the transformation of large U.S. cities by the latest wave of immigration has added a theoretically unexpected plot twist that, if replicated, will substantially alter the way this story is told.

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APPENDIX: COMBINED CORRELATION MATRIX FOR THE BLACK AND LATINO MODELS

	1	2	3	4	5	6	7	8	9	
1. Black homicide	—	.44	.07	.45	.15	.20	−.05	.59	.55	1. Latino homicide
2. (Ln) Black population	.46	—	−.17	.34	.09	.19	.05	.47	.29	2. (Ln) Lat population
3. Instability index	.31	.18	—	.26	.17	.09	−.08	.12	.21	3. Instability index
4. Black poverty	.31	.09	.19	—	.47	.38	.01	.43	.39	4. Latino poverty
5. Blk female-headed family	.36	.48	.16	.31	—	.25	−.00	.14	.21	5. Lat fem-head family
6. Black male joblessness	.19	.14	.02	.18	.19	—	.11	.19	.28	6. Lat male joblessness
7. Young black males	−.02	.17	.01	−.11	.09	.13	—	−.05	−.24	7. Young Latino males
8. New immigrants	.10	.04	.12	.37	.22	.19	−.12	—	.70	8. New immigrants
9. Black spatial lag	.59	.32	.30	.39	.31	.16	−.10	.49	—	9. Latino spatial lag

NOTES

1. We chose to focus on homicide for several reasons. First, it is the most reliably recorded crime. Second, the criminological theories we discuss here have been used to explain all types of street crime, including homicide. Finally, public anxiety about urban homicide is high, making our focus especially policy relevant.

2. Census block groups also are valid units for neighborhood level analysis. However, their small size poses difficulties when studying comparatively rare events, such as homicide, partitioned by ethnicity of the victim.

3. The fear that too many crime-prone immigrants are entering the United States is reflected in a national best-selling book promising "common sense about America's immigration disaster," by Peter J. Brimelow (1996, p. 182), a writer at *Forbes* and *National Review* magazines: "Immigration is not the *only* cause of crime. It may not even be the major cause of crime. But it is a *factor*." Such alarmist writings assume a link between immigrants and crime without offering supportive evidence. In fact, Brimelow (1996, p. 182) claims that "there has been no serious academic study of the impact on crime" of the post-1965 wave of immigration.

4. David Greenberg suggested this possibility in a personal communication.

5. We are interested in the *presence* of homicide in a given tract. In order to measure tract-level homicide incidents as accurately as possible, we based our homicide counts on the victim's race for two reasons. First, information on the offender or suspect is missing or possibly unreliable in the substantial number of cases not cleared with an arrest. In addition, Short (1997) points out that findings based on victimization data are also valid for offenders, given the intraracial nature of the vast majority of homicide events.

6. Seven tracts with more than five hundred individuals were excluded in San Diego because they were military bases, airfields, or contained mostly institutionalized populations (e.g., a hospital).

7. We initially included a measure of tract-level ethnic diversity, which was not a significant predictor of homicide, did not change the effect of immigration, and was subsequently dropped in an effort to conserve degrees of freedom.

8. Although there is some overlap between our immigration measure and one of the ten census variables used to construct the instability index (i.e., the number of 1990 Miami city residents living abroad in 1985), the bivariate correlation between recent immigration and the instability index ($r = .12$) in the three cities is quite weak (Appendix). The implication is that the most recent wave of immigration is not a major factor driving neighborhood instability, an issue more fully explored in the multivariate analysis that follows.

9. Due to overdispersed data in three models (Miami blacks and Latinos and San Diego Latinos), we also estimated negative binomial models and present these results in Tables 2 and 3.

10. The procedure we used for creating the spatial lag term was as follows. First, in the program *SpaceStat* (Anselin 1995), we created a distance matrix based on tract centroid coordinates and then merged the file containing this variable with a file containing the fitted values of the dependent variable that we created in *Limdep version 7.0*. Second, we transformed the distance matrix to an inverse distance weights matrix (no limits were imposed so that all tracts received a weight based on their distance). Third, we row standardized the inverse distance weights matrix to adjust each weight so that values across a row summed to one. Finally, we created a spatial lag average using the fitted values of tract-levels of black and Latino homicide and exported this variable back into *Limdep*.

11. The correlation matrices and collinearity diagnostics are available from the authors on request. The Appendix contains a combined matrix for all three cities for the Latino and black models, although the combined relationships vary somewhat from those found in the city-specific matrices.

12. We focused on Latinos and non-Latino blacks because urban homicide and its structural covariates disproportionately affect these groups, and because immigrants in the three cities tend

to settle in Latino or black neighborhoods. However, we also conducted analyses (available from the authors) to test the effect of immigration on non-Latino white homicide levels. We found that recent immigration is a significant, *negative* predictor of white homicide only in San Diego, but not a significant predictor in Miami or El Paso. There is little empirical evidence or theoretical reason to expect that immigration should be positively related to white homicide, given settlement patterns of these groups as well as the generally intraracial nature of homicide.

13. Given the overall lack of relationship between immigration and homicide in this study, we decided to conduct separate analyses (available from the authors) to determine whether immigration was influencing variables commonly associated with neighborhood disorganization: residential instability, female-headed families, and poverty. Using each of these as dependent variables, instead of homicide, we estimated ordinary least squares models for both Latinos and blacks. Immigration was a positive and significant predictor of these dependent variables in only four out of eighteen models: Latino residential instability and poverty in El Paso, Latino poverty in San Diego, and black residential instability in El Paso. In other words, immigration generally is *not* "disorganizing" these communities, with a handful of exceptions. Clearly, these empirical variations are more complicated than extant theories, but it is apparent that sociological explanations of crimes such as homicide must confront the fact that immigration is not the disorganizing influence that many scholars have assumed (see also Portes [2000] on the "myth" of immigration as a disorganizing community process).

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By
GARTH DAVIES
and
JEFFREY FAGAN

Immigration and crime have received much popular and political attention in the past decade and have been a focus of episodic social attention for much of the history of the United States. Recent policy and legal discourse suggests that the stigmatic link between immigrants and crime has endured, even in the face of evidence to the contrary. This study addresses the relationship between immigration and crime in urban settings, focusing on areal units where immigrants tend to cluster spatially as well as socially. The authors ask whether immigration creates risks or benefits for neighborhoods in terms of lower crime rates. The question is animated in part by a durable claim in criminology that areas with large immigrant populations are burdened by elevated levels of social disorder and crime. In contrast, more recent theory and research suggest that "immigrant neighborhoods" may simply be differentially organized and function in a manner that reduces the incidence of crime. Accordingly, this research investigates whether immigrants are associated with differences in area crime rates. In addition, the authors ask whether there are differences in the effects of immigration on neighborhood crime rates by the racial and ethnic makeup of the foreign-born populations. Finally, the authors examine the effects of immigration on patterns of enforcement.

Keywords: immigration; ethnicity; law enforcement; generalized propensity scores

Garth Davies is an associate professor at Simon Fraser University. His current work examines the intersections of immigration, segregation, crime, and policing. He is also interested in alternative techniques for analyzing neighborhood effects.

Jeffrey Fagan is the Isidor and Seville Sulzbacher Professor of Law and director of the Center for Crime, Community and Law at Columbia Law School. He also is a senior research scholar at Yale Law School. His recent scholarship examines policing, the legitimacy of the criminal law, capital punishment, and juvenile crime and punishment. He is a fellow of the American Society of Criminology.

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Historically, immigration has been associated with crime and other social ills in popular and political culture (Hagan and Palloni 1999; McDonald 2009). Waves of immigration in the nineteenth and twentieth centuries brought with them moral panics on drugs and alcohol, gangs, delinquency, organized crime, wage suppression, and drains on public resources (Hagan, Levi, and Dinovitzer 2008; Sampson 2008). More recently, the presumed link to crime has provided a significant rationale for a variety of punitive responses to immigration. In Arizona, the 2010 killing of Robert Krentz made tangible generalized anxiety about immigrant crime (Archibold 2010). Although no suspect was identified, speculation that the killer was an illegal alien helped secure the passage of Arizona's draconian (anti)immigration bill. The specter of immigrant crime similarly aided in generating public support for "copycat" legislation in Georgia and Alabama. Even laws that ostensibly were more about the economics of immigration, such as California's Proposition 187, nonetheless were buttressed by allusions not just to the potential for financial burdens on labor markets, education, and health care from illegal immigration, but also to criminal activity among some illegal immigrants.

Thus, as part of the broader debate about immigration, the putative connection between immigration and crime is of considerable political and practical import. The presumptive connection has motivated new efforts to link local law enforcement with immigration enforcement (Varsanyi et al. forthcoming). The new intersection of criminal enforcement with immigration has led some scholars to label illegal immigration as "crimigration," a linguistic and conceptual move to broaden the mandate of local police to systematically and routinely engage in immigration enforcement (Sklansky forthcoming).

Still, despite the recent involvement of local police in immigration enforcement, research and theoretical interest in immigration in criminology has been episodic (Hagan, Levi, and Dinovitzer 2008). In the early twentieth century, immigration figured prominently in theories of neighborhood and crime, particularly *social disorganization* (Shaw and McKay 1943; Bursik 1988; Bursik and Grasmick 1993; Sampson 2008). Often arriving in poverty, many immigrants, principally from Southern and Eastern Europe, were forced to settle in the poorest areas of the city. Subsequently, immigrants were scapegoated for the problems attendant with these urban slums. Studies of street gangs in the 1920s in Chicago depicted gang conflict as a function of ethnic and racial conflicts among first- and second-generation immigrant youth (e.g., Thrasher 1927). The early juvenile courts in Chicago and other urban areas targeted the children of immigrants for a paternalistic form of social control (Tanenhaus 2004; Platt 1969; Sealander 2003). Shaw and McKay's (1943) critical insight was that rates of delinquency remained pretty stable among Chicago's neighborhoods between 1900 and 1933, despite dramatic changes in the composition of these neighborhoods. In other words, crime rates were not contingent upon the racial and ethnic makeup of an area; rather, crime was related to neighborhood conditions, specifically poverty, anonymity, and heterogeneity. In this way, the legacy of social

disorganization in relation to immigrants is somewhat mixed. On one hand, the theory offers a strong refutation of the notion that immigrants are inherently criminal. On the other hand, the theory does predict higher crime rates in areas inhabited by newly arrived immigrants.

Interest in immigration among criminologists has ebbed and flowed with immigration itself. Between the 1920s and 1965, restrictions on the number of immigrants entering the United States pushed immigration to the background. When the 1965 amendments to the Immigration and Nationality Act ended quotas based on nationality, immigration rates leaped dramatically, and the stigmatic link between immigration and crime again became part of the political and popular discourse on immigration. Still, interest in immigration among criminologists has revived only in the past decade, as theories of neighborhoods (Sampson, Morenoff, and Gannon-Rowley 2002) and social networks (Kasinitz, Mollenkopf, and Waters 2009) began to incorporate the unique contexts of immigration. Much of this more recent research has challenged stereotypical assertions and suggested that the connection between immigration and crime is more myth than reality: immigrants are not more "crime-prone" than native-born Americans, and, on balance, immigration does not increase neighborhood crime rates (Butcher and Piehl 1998). In fact, emerging evidence indicates that immigration may improve neighborhood conditions and reduce levels of crime.

Nonetheless, research on the immigration-crime nexus continues to be a work in progress, and a number of important issues remain. Among the most important of these concerns is the disaggregation of immigration. As noted above, *total* immigration does not appear to promote higher crime rates. But this population is hardly homogeneous. Immigrants are drawn from a wide range of diverse countries, cultures, and racial and ethnic groups. Nowhere is this more evident than in New York City (NYC) (Kasinitz, Mollenkopf, and Waters 2009). For this reason, this study examines patterns of crime among racial and ethnic subgroups in NYC and the effects of New York's Order Maintenance Policing strategy (Fagan et al. 2010; Zimring 2011) in immigrant neighborhoods.

Perspectives on Immigration, Crime, and Enforcement

As in the past, the specter of elevated crime rates among immigrants has animated or reinforced specific law enforcement tactics that seek to counter unique forms of criminal activity or organizations among immigrants (Rumbaut and Ewing 2007). Programs such as the federal Secure Communities Program (U.S. Immigration and Customs Enforcement [ICE] 2009; Homeland Security Advisory Council 2011) up the ante in this debate by attempting to establish a link between "ordinary" crime, immigration violations, and national security threats (Hagan, Levi, and Dinovitzer 2008; Varsanyi et al. forthcoming; Sklansky forthcoming). Some gang violence suppression initiatives also focus on the nexus of immigration and street gangs, and these links become even more contentious

when immigration is linked to violence from drug trafficking (Stuntz 2002), as in the Robert Krentz case in Arizona.

These efforts are predicated on assumptions that immigrants commit more crimes and that urban neighborhoods of immigrant concentration are important targets for enhanced law enforcement. Whether immigration contributes to or attenuates neighborhood crime rates is a contentious debate (see, e.g., Butcher and Piehl 1998, 2007). The sociological and criminological literature offers grounds to both support and reject these claims. Below we briefly sketch out the evidence on these competing perspectives on the effects of immigration, and we follow that with empirical tests from NYC, a place that has been an important immigration reception context throughout the history of the United States (Kasinitz, Mollenkopf, and Waters 2009; Portes and Rumbaut 2006).

The bad: Why might immigration increase neighborhood crime rates?

The presumptive association between immigration and crime does not reflect only simple nativism and prejudice. Immigration was, from the start, built into theories of crime in neighborhoods. Social disorganization theory was formulated by Shaw and McKay (1943) in response to the disruptions fostered by successive waves of immigration into Chicago. It was, in other words, largely a theory about the effects of immigration and political economy, and the basic argument linking immigration with crime endured even as the terms of the debate changed (Bursik and Grasmick 1993).

For example, the key construct linking social disorganization and crime is informal social control (or lack thereof). Neighborhoods can accomplish collective goals, including crime control, through mechanisms of informal social control, such as local institutions, social networks, and shared expectations regarding behavioral regulation (Sampson, Raudenbush, and Earls 1997). One version of the immigration link to crime focuses on the compromising effects of immigrant concentration on local efforts at informal social control. Often arriving with few financial resources and limited job prospects, immigrants tend to be sorted or self-selected into already disadvantaged areas (Sampson 2008). Immigrants have little attachment to these neighborhoods upon their arrival, and they leave for better social and material conditions as soon as possible, only to be succeeded by other immigrant groups (Shaw and McKay 1943). The population turnover caused by the influx and outflow of immigrants destabilizes these areas. The resulting racial and ethnic heterogeneity hampers interpersonal communication, renders trust more elusive, and, at the extremes, leads to territorial conflict and ethnic rivalry. In short, the rapid social change brought on by widespread immigration serves to undermine local institutions and networks and weakens the foundations of informal social control.

The demographics of immigration may also increase crime rates, net of any individual criminal tendencies of newly arrived immigrants or their second-generation children. In general though hardly uniformly, immigrants tend to be young, male, relatively poor, and uneducated (Portes and Rumbaut 2006). In

other words, immigrants tend to share a demographic profile consistent with groups that already commit a disproportionate share of crime. So immigration may increase crime levels simply by increasing the pool of more likely offenders. At the same time, immigration may also elevate crime by reinforcing existing disadvantage in high-risk neighborhoods. With low incomes and limited skills, immigrant populations can sustain the lower socioeconomic status of these areas. Over time, this status may become entrenched. In extreme circumstances, neighborhoods may reach a tipping point, with spiraling levels of crime and violence.

Opportunity structures may also be implicated in immigrant crime patterns. Often residing in distressed neighborhoods and lacking job skills, connections, and social capital, immigrants are confronted by blocked economic opportunities. One potential response to attenuated access to capital and legal markets is for immigrants to "innovate" (Merton 1938) by resorting to crime as an alternative avenue for advancement. The probability of innovative crime is enhanced by greater access to illegitimate opportunity structures, which are more prevalent in disadvantaged areas (Cloward and Ohlin 1960).

Finally, immigrants may increase crime not as offenders, but as victims. Immigrant populations are at greater risk of victimization for several reasons. Immigrants tend to reside, at least initially, in high-crime areas. Alba, Logan, and Bellair (1994) found that Hispanics who were born outside the United States, had immigrated only recently, or were less linguistically assimilated lived in areas with higher crime rates than did other groups. Increasingly, research has paid particular attention to particular types of immigrant victimization, such as hate crimes (Hendricks et al. 2007) and sexual violence against women (Decker, Raj, and Silverman 2007). But immigrants are also vulnerable to a wide range of exploitations, including unscrupulous landlords, bankers, financiers, and employers (Chiswick and Miller 2008). Language barriers and cultural mistrust of the police may make immigrants reluctant to report crime and otherwise interact with authorities, making immigrants more open to predation.

The good: Why might immigration reduce crime?

Running through the traditional theoretical perspectives are a series of assumptions about the process of immigration (see, e.g., Lewis 1966; Portes and Sensenbrenner 1993). The simplistic image of socially isolated immigrants with little education and limited employment potential being funneled into disadvantaged neighborhoods is becoming anachronistic. The post-1965 wave of immigration has been marked by greater diversity in education, job skills, and access to employment networks (Wadsworth 2010). These changes to immigrants' profiles have forced a reconceptualization of the connection between immigration and crime. Rather than serving to increase crime rates, there are a number of ways in which immigration might instead be a protective factor that reduces crime.

One reason that immigration might mitigate crime is what Wadsworth (2010) has referred to as the "healthy immigrant thesis." Immigration is not a random

process, and immigrants do not represent a random sample from their countries of origin. They are among the most highly motivated individuals, with the lowest propensities toward criminal behavior. After immigrants have arrived, they have greater stakes in conformity and are much less likely to jeopardize their achievements by participating in crime. It is also possible that immigrants have different frameworks for evaluating their neighborhood environments and work conditions. The "disadvantaged" neighborhoods and "low-skilled" jobs immigrants might encounter in the United States are still improvements compared to conditions in their countries of origins.

Similarly, there may be aspects of culture that serve to protect immigrants from crime. Immigrant communities foster closer social ties and networks, provide social support, promote cultural preservation, and aid in the maintenance of traditional norms and values. It has been suggested that immigrants may place greater emphasis on marriage and intact families (Ousey and Kurbrin 2009). More recent immigrants also benefit from the existing coethnic networks, enclaves that can provide assistance as immigrants navigate the new circumstances. The growth and concentration of ethnic enclaves may also offer economic opportunities that were previously unavailable to immigrants. Ethnically organized or segmented economies often provide jobs in businesses established by earlier immigrants (Portes and Zhou 1992), affording opportunities perhaps inaccessible in the broader labor market. Niche employment may also improve the prospects of some immigrants who might otherwise find it difficult to find work (Kasinitz, Mollenkopf, and Waters 2009). Some groups may be particularly entrepreneurial.

At the same time, widespread immigration can provide advantages that extend beyond the confines of enclaves, by repopulating moribund neighborhoods, rejuvenating local economies, and encouraging local development. More generally, the potential boon of immigration is captured by the "immigrant revitalization" perspective. Martinez (2006) has posited that the influx of immigrants fosters "new forms of social organization and adaptive social structures" that may mediate the deleterious consequences of residing in disadvantaged areas. In addition to reinvigorating economies and nurturing familial ties and social networks, large-scale immigration can expand and strengthen community institutions (Velez 2009). Immigrant revitalization does not represent a complete rebuttal of social disorganization but, rather, suggests that in some circumstances, instability is decoupled from the inhibition of social control and, thus, does not necessarily function as a precursor to crime (Martinez, Stowell, and Lee 2010).

The evidence thus far

Although immigration and crime have long been conflated, both on theoretical grounds and in the popular imagination, there is strikingly little evidence to support this nexus. There is a smattering of supporting examples (see, e.g., Taft 1933; Hagan and Palloni 1999), but these analyses are more anecdotal

than systematic. Most early research on immigration and crime is beset with methodological shortcomings (Tonry 1997). In a detailed review of more contemporary aggregate-level studies, Ousey and Kubrin (2009) found that the supposition that immigration contributes to crime rates was unwarranted. With few exceptions, immigration either (1) was not associated with measures of crime and violence or (2) actually reduced violence levels.

These "neutral" or "protective" effects of immigration have been shown across a number of research settings and levels (see Sampson [2008] for a review). For example, Butcher and Piehl (1998) concluded that, after controlling for demographic characteristics, immigration had no effect on either crime rates or changes in crime rates in a sample of 43 metropolitan areas. Analogous results were presented by Martinez (2008) in a study of 111 cities with at least 5,000 Latinos. Immigration was positively associated with total homicide rate and negatively associated with Latino homicide rates, but neither relationship was significant. Using a random sample of 150 Metropolitan Statistical Areas and Primary Metropolitan Statistical Areas (MSAs and PMSAs), Reid et al. (2005) found recent immigration had a significant negative effect on homicide rates, but otherwise showed no effect for robbery, burglary, or larceny rates. In a longitudinal study of 159 cities between 1980 and 2000, immigration had a mildly significant negative influence on violent crime rates (Ousey and Kubrin 2009).

Cities are an important unit of macro-level analysis, but it is possible that their size masks important variation between neighborhoods. A series of census tract–level studies by Martinez and colleagues have generally confirmed the results from the cities. For example, recent immigration did not affect Latino homicide rates in Miami or San Diego neighborhoods or black homicide rates in El Paso (Lee, Martinez, and Rosenfeld 2001). Recent immigration was negatively, and significantly, related to Latino homicide in El Paso and black homicide in Miami. Only for black homicide rates in San Diego was immigration a significant positive predictor. Similar results were produced when homicide was disaggregated by motivational types (Nielsen, Lee, and Martinez 2005). When more nuanced measures of immigration were used, ethnic-specific models showed no association between immigration and violent crime rates in Houston and mostly significant negative effects for San Diego (Stowell and Martinez 2007, 2009). Finally, in a longitudinal study of San Diego neighborhoods between 1980 and 2000, neighborhoods with higher proportions of immigrants had lower levels of total, Latino, and non-Latino white victimization (Martinez, Stowell, and Lee 2010).

Taken together, these studies, utilizing a variety of crime types, measures of immigration, research designs, and units of analysis, offer a preponderance of evidence demonstrating that "immigration reduces crime." Lee and Martinez (2009) have characterized this proposal as an "emerging scholarly consensus," and several authors have begun to explain the precipitous crime decline of the 1990s as a function of rising immigration (Sampson 2008; MacDonald, Hipp, and Gill 2008; Stowell et al. 2009; Wadsworth 2010). At a minimum, the findings of contemporary research call into question previous theoretical perspectives supposing

a positive correlation between immigration and crime. Nonetheless, research on the immigration-crime nexus continues to be a work in progress, and a number of important issues remain. One such issue concerns the disaggregation of racial and ethnic effects. Much of the current wave of research has focused on Latino immigrants. Few if any studies have extended their comparisons to include African, West Indian, and Asian populations. The present study analyzes criminogenic effects across this wider spectrum of groups.

Enforcement

Interest in the overlap of policing and immigration has been expressed on a number of fronts (Stuntz 2002; Hagan, Levi, and Dinovitzer 2008; Sklansky forthcoming; Epp, Maynard-Moody, and Heider-Markel forthcoming). At present, special attention is being paid to the issue of illegal immigration, the security of U.S. borders, and the broader "war on terrorism." Once a primarily federal issue, these concerns are increasingly playing out on more local stages (Harris 2006). September 11, 2001, led to a sea change, as the Department of Justice began utilizing local police to supplement immigration enforcement (Gladstein 2005), blurring the lines between immigration offenses under federal law and criminal offenses under state statutes. This, in turn, has altered substantially and problematized the relationship between immigrant communities and local agencies. Most notably, the shift in emphasis has proved difficult to reconcile with the dictates of "community policing" (Tyler, Schulhofer, and Huq 2010). Tyler and colleagues (2010) show, in a survey of Muslim Americans in NYC, that the trust and legitimacy required for effective community policing is elusive when many residents fear that contact with the police could lead to deportation. In fact, the degradation of police-community relations has been most acute in immigrant communities (Jones and Supinski 2010).

A second line of inquiry has involved an expansion of the literature on public perceptions of the police to include immigrant populations. Race and ethnicity have long been central to understanding disparate levels of support for and satisfaction with the police, but only recently has immigrant status been incorporated into this literature. Some of the findings regarding immigrant attitudes are consistent with previous opinion surveys. For example, quantity of police contacts was negatively related to rating of the police for Chinese immigrants in NYC, while quality of interaction was a key determinant of satisfaction with the police (Chu, Song, and Dombrink 2005). However, evaluations of the police may also reflect considerations unique to immigrants, such as their former experiences with and perceptions of the criminal justice systems of the countries from which they emigrated (Davis and Henderson 2003). The specific information about the police conveyed through social networks of family, friends, acquaintances, neighbors, and coworkers similarly plays a pivotal role in the development of sentiments toward the police (Menjívar and Bejarano 2004).

As the scale of immigration has grown, the challenges inherent in policing immigrants and immigrant communities have also begun to garner more consideration.

Perhaps the most prominent obstacle confronting police-immigrant relations is language. The inability to communicate effectively and efficiently is a source of considerable delay and frustration for officers. Moreover, many immigrants arrive from countries where violence, corruption, and incompetence are endemic to the police (Mears 2001; Skogan 2009). The fear of the police that may be imported from these originating countries can pose another significant barrier to communication. As well, newcomers often lack knowledge pertaining to the criminal justice system, which complicates relationship-building with the police. In certain circumstances, the specific nature of police-immigrant contacts may be a point of contention. Culver (2004) noted that the sizable number of interactions involving traffic and false identification offenses in one small Midwestern community resulted in disproportionately negative contacts between the police and the Hispanic community.

Law enforcement has always been an important corollary to crime, but the correlation between the two has never been perfect. There are a host of factors that can influence patterns of enforcement independent of crime. Of particular interest here are the contextual effects of neighborhoods on policing. In contrast to the issue of crime, the contingent relationship with law enforcement continues to be largely absent from the criminological treatment of immigration. To be more precise, the aggregate-level impact of immigration levels on rates of enforcement has not, as yet, received sufficient attention. This research is a first step in trying to understand patterns of enforcement in immigrant neighborhoods.

Research Questions

As the quintessential "gateway" city, NYC provides a crucial platform for exploring the effects of immigration on local crime and enforcement. The sheer scale and variety of immigration in NYC allows for analyses to be disaggregated by racial/ethnic groups. The rich immigrant history facilitates a number of conceptually important comparisons, including those involving "newer" immigrants and native coethnics. And as with other large U.S. cities, patterns of immigration, crime, and enforcement continue to evolve. The period of time under consideration here, from 1990 to 2000, saw a notable increase in the diversity of NYC's immigrant population. In light of these ongoing changes, this study updates and reconsiders a series of questions relating to NYC neighborhoods.

First, where do immigrants settle in NYC? The choice of residential locations is not random. Rather, immigrants self-select into neighborhoods. Immigrants have traditionally settled in disadvantaged areas characterized by high levels of poverty, racial heterogeneity, mobility, and crime. If settlement patterns have changed, past theoretical explanations of immigration and crime need to be revisited. This study investigates the social, economic, and crime conditions that affect the self-selection process for immigrants. The nonrandom nature of residential location has an important methodological dimension as well. Criminological evaluations have not sufficiently addressed the potential for *selection bias* resulting from self-selection. That is, prior work has not accounted for the propensity of newly arriving groups to select specific areas for settlement where people look

like them and have similar resources. This study uses a generalized propensity score (GPS) approach to control for selection bias in the analysis of immigration and crime.

Second, do local crime rates vary by level of immigration? Most current research indicates that immigration either is unrelated to, or mitigates, neighborhood crime. This study utilizes a larger number of more diverse neighborhoods, and a wider variety of crimes, than have previous efforts. In addition to assessing the broad effects of immigration on crime, this study also examines whether local crime rates vary by race- and ethnic-specific immigrant groups.

Third, is there a relationship between immigration and patterns of police enforcement? Prior research has not examined how immigration may influence neighborhood patterns of enforcement of "regular" crime. It is important to evaluate enforcement in addition to crime, given what is known about the complex dynamics of policing in relation to neighborhood and race more generally. Finally, as with crime, this study also disaggregates effects by various immigrant groups.

Methods

The concentration of foreign-born population is used to estimate the effects of immigration on crime and enforcement in NYC neighborhoods from 2004 to 2008. We begin by estimating the selection of immigrants into a neighborhood by adjusting immigration rates by their correlation with observed characteristics of the neighborhoods, including social structure, political economy, and crime. We estimate the selection of immigrant groups into neighborhoods for four racial and ethnic groups, and also for specific varieties of crime and enforcement. We use generalized propensity score (GPS) analysis (Hirano and Imbens 2004; Feng et al. 2011) to estimate the probability (p) of a proportion of immigrants residing in a neighborhood conditional on the observed characteristics of each neighborhood. Like the standard propensity score approach, GPS aims to establish the effect of a "treatment" (in this case, level of immigration) with observational data where the treatment is not randomly assigned. But while propensity scores attempt to control for differences between treatment groups when the treatment is binary, GPS allows for continuous treatments (Imai and van Dyk 2004). The GPS approach begins by estimating a propensity score (the variables used to model immigration are illustrated in Table 1). GPS assumes that the disturbances for the estimation model are normally distributed and tests this assumption using the Kolmogorov-Smirnov equality-of-distributions test. Finally, the balance on the covariates is evaluated at specified intervals of the treatment variable (Bia and Mattei 2008).

Once the normality of disturbances and balancing properties have been confirmed, the GPS is used to identify the effects of immigration on crime and enforcement using a dose-response model. The dose-response model takes the

form of a regression equation

$$Y = T + \text{GPS},$$

Where

Y = outcome,

T = treatment, and

GPS = generalized propensity score.

That is, the dose-response model estimates the marginal effects of increases in the levels of concentration of immigrants in a neighborhood (the "treatment") on the outcomes of interest. Similar to the use of propensity scores to estimate the nonrandom selection of individuals into neighborhoods (MacDonald, this volume), this technique allows us to include both immigrant concentration and the control variables in models of both crime and enforcement. This estimation strategy produces an unbiased test of the effect of immigrant concentration on crime.

Data

Immigration. Immigration here refers to foreign-born individuals. Immigration data were obtained from decennial census data on tracts in NYC in 2000. Race- and ethnic-specific data were taken from STF4 census files. "Newer" immigrants are those individuals who arrived in the United States between 1995 and 2000. We include immigrants from Caribbean and African nations among "black" immigrants, despite potential differences in the countries of origin in each of these populations. One reason to combine these groups is the potential reactivity of police officers to Afrocentric features that are shared among this population as well as native-born African Americans (Eberhardt et al. 2006). We also include Latino immigrants who self-identify in the census as both "black" and "Hispanic origin" among blacks.

Neighborhood social structure. Data estimating the socioeconomic and demographic characteristics of neighborhoods were obtained from the 1990 and 2000 decennial census. Housing price sale data were obtained from an archive of real estate transactions maintained by the Department of Finance in NYC. Transactions were recorded by address and were geocoded and assigned to census tracts.

Crime. Homicide and assault rates between 1990 and 2000 were computed from the records provided by the New York City Department of Health and Mental Hygiene. Homicide data were obtained from Vital Statistics records. Assault data were obtained from emergency room and hospital admissions for intentional injuries. These data were geocoded and assigned to census block groups and tracts. Detailed data on crime incidents from 2004 to 2008 were obtained from the Oniform records system of the New York City Police Department (NYPD), which is the method for compiling crime complaints.[1] Each record included spatial coordinates (i.e., latitude and longitude) of the

TABLE 1
Modal Neighborhood Comparisons for Foreign-Born Populations

	Foreign-Born Black		Foreign-Born Latino		Foreign-Born Asian		Foreign-Born White	
Variables	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
Foreign-born 2000 (%)	29.56	13.31	37.20	16.21	48.47	11.50	3.49	14.89
Foreign-born 1990 (%)	22.08	12.79	28.87	14.51	36.04	12.30	25.20	12.23
Black (%)	54.87	23.94	22.44	22.28	5.77	8.16	5.61	9.75
Latino (%)	27.69	19.21	34.52	16.09	22.33	17.10	17.21	15.20
Asian (%)	4.37	7.23	11.76	13.69	2.58	3.44	12.59	12.24
White (%)	8.12	13.37	25.03	22.81	44.34	23.21	61.15	22.41
Poverty (%)	27.67	12.69	24.81	13.47	16.60	9.07	13.01	1.74
Public assistance (%)	12.99	7.75	1.12	7.90	4.78	3.47	3.26	3.75
Gini index	.44	.07	.42	.06	.38	.07	.33	.08
Less than high school (%)	34.20	11.53	33.96	11.95	27.17	11.64	19.01	12.60
Unemployment (%)	14.83	7.54	11.84	7.50	7.29	3.22	6.20	4.15
Not in labor force (%)	45.09	7.42	45.04	9.01	42.81	6.89	38.52	1.81
Female-headed households (%)	17.74	8.81	13.13	9.33	5.79	3.71	4.70	4.36
Supervision ratio	.64	.18	.58	.20	.47	.13	.41	.23
White isolation index	.10	.14	.27	.24	.46	.23	.63	.21
Residential mobility (% < 5 years)	62.36	7.83	6.59	7.76	6.37	7.86	59.83	11.87
Housing vacancy rate (%)	7.93	5.38	5.48	3.87	3.81	2.48	4.78	3.23
Renters (%)	71.11	23.10	73.31	19.58	61.28	2.65	58.18	24.70
Population (logged)	8.09	0.56	8.17	0.52	8.21	0.42	8.14	0.64
Average housing price	29.92	9.10	34.96	19.55	48.29	133.48	62.79	116.13
Average homicide rate	2.95	1.79	1.82	1.60	0.80	0.65	0.64	0.73
Average assault rate	18.09	1.35	12.44	11.89	5.32	3.97	5.92	8.16

crime location, as well as details on the specific offense (New York State Penal Law section). Crime rates were averaged over the five-year period to provide stability and were logged to minimize the influence of outliers.

Enforcement. Detailed data on arrests were obtained from the Online Booking System (OLBS) of the NYPD. Records include both offense and arrest location, as well as details on the specific Penal Law section.

Data on street stops were also obtained from the NYPD for the years 2004 to 2008. Records include the specific location of the stop, the suspected crime, the rationale for and outcome of the stop, and demographic characteristics of the person stopped.

Arrests and street stops were combined to create an overall index of enforcement.[2] Rates of enforcement were created by using crimes as a denominator. Thus, enforcement rate is a measure of enforcement per crime. Enforcement rates were averaged over the five-year period and logged.

Results

Where do immigrants settle?

Comparisons of modal neighborhood characteristics for each race- or ethnicity-specific population group are shown in Table 1.[3] In the main, each group tends to sort into tracts where people look like them and have similar resources. The proclivity is most pronounced among white immigrants, who reside in largely white (61 percent) neighborhoods. Black immigrants similarly settle in predominantly black (55 percent) areas. While Latino immigrants also live where Latinos comprise the majority of residents, the distribution of race/ethnic groups is more even. Latino immigrants are much more likely than black immigrants to reside near whites and Asians. In contrast to other groups, Asian immigrants are living not in Asian neighborhoods, but in primarily white locations. It is possible that Asian immigrants have greater financial resources and are less constrained in terms of choice of residential neighborhood.

Black immigrants tend to settle in places where social and economic disadvantage are greatest. Relative to the other groups, modal black immigrant neighborhoods have the highest rates of poverty, public assistance, unemployment, and female-headed households. These areas also have lower access to wealth, as indicated by housing prices. A similar pattern is found in relation to violent crime. In addition to socioeconomic distress, black immigrants reside in areas with much higher levels of homicide and assault. At the other end of the spectrum, white and Asian immigrants face much less danger in their neighborhoods. Similar to immigrants of African descent, Latino immigrants also tend to live in disadvantaged neighborhoods, but these places are less economically isolated than the places where black immigrants settle. Whites tend to avoid socioeconomic or structural disadvantage and enjoy greater access to wealth in the neighborhoods where they settle. Areas occupied by Asian immigrants are less well-off than those of whites but nonetheless are more like white neighborhoods than are those occupied by other groups.

The results of the GPS models confirm these descriptive statistics. Table 2 shows the factors that predict the concentration of the total and race/ethnic-specific foreign-born population in 2000. As the goal of the propensity score approach is to improve estimates of immigration by using a large number of relevant variables, the coefficients presented in Table 2 should be interpreted cautiously. However, the results nonetheless reveal some interesting patterns. The large effect for immigrant concentration at the outset of the 1990 to 2000 decade

is not surprising and suggests that immigrant neighborhoods remain stable as reception contexts for new arrivals in NYC. The effect size for the 1990 baseline dwarfs the strength of the other factors. Beyond the stability of immigrant concentration, the effects of wealth and safety are predictors of where immigrants settle. The results in Table 2 for total immigration reflect the skew toward wealth or less disadvantage for white immigrants: immigrant concentration is higher in neighborhoods with fewer indicia of disadvantage and in places that are safer.

Immigration and crime

Next, GPS regressions of total and recent immigration on crime were estimated. Figure 1 shows the results of the full models, which include controls for GPS score for immigration at 2000. Race- and ethnic-specific immigration breakdowns are presented in Figure 2.[4] Immigration is a protective factor for both total crime and three specific categories of crime. Total immigration shows a significant (lower than –2) effect for all crime categories. The preventive effects of immigration are less pronounced for recent immigrants, as significant negative coefficients are found only for total and drug crime rates.

Immigration effects vary by the specific crime and racial/ethnic group. Figure 2 shows that white immigration, mostly from Russia and other Eastern European countries, has strong insulating effects. Since white immigrants tend to settle in areas with low crime rates and greater access to economic wealth and resources, the strong negative parameter suggests that white immigration exerts a protective effect beyond other factors that also reduce crime risks.

Total and violent crime rates are lower when concentrations of foreign-born persons of African descent are higher. This is particularly important given the racial components of the distribution of crime across NYC neighborhoods. We assume that these are largely Caribbean immigrants, although immigration from the African continent has increased in the past decade.

The effects of Latino and Asian immigration on crime are more modest. The direction of the effects is negative, but they do not reach significance in any of the models. It is possible that this attenuated influence of immigration reflects the heterogeneity of residential settlement that is characteristic of these groups. Many Latino immigrants, for example, settle in diverse areas of the city, including both low-crime places (northwest Queens) and high-crime areas (Sunset Park in Brooklyn). The overall average effect may mask some important micro-effects.

Immigration and enforcement

The relationship between immigration and enforcement is illustrated in Figure 3. The effects of immigration on enforcement are quite different from its protective effects on crime. Enforcement for total, violent, and property crimes is substantially higher in places with greater proportions of immigrants. This effect is especially pronounced for newer immigrants, those who have been in the

TABLE 2
Generalized Propensity Score Models—Foreign-Born 2000, Overall and by Race

	Overall		Black		Latino		Asian		White	
	Est.	*SE*	Est.	*SE*	Est.	*SE*	Est.	*SE*	Est.	*SE*
Foreign-born 1990	.139***	.003	−.005	.015	.009	.033	2.122***	.205	.198***	.018
Poverty	−.042***	.006	−.087*	.035	−.175*	.075	1.164*	.474	−.144***	.042
Public assistance	−.014**	.005	−.253***	.030	.340***	.064	1.457***	.404	.136***	.036
Gini index	.051***	.005	.214***	.029	−.183**	.062	.758	.392	.285***	.035
% less than high school	.034***	.005	−.403***	.024	.656***	.052	.327	.328	−.143***	.029
% unemployment	−.011**	.004	.016***	.021	−.039	.045	−.500	.284	−.024	.025
% labor force nonparticipation	.003	.004	−.091***	.020	.063	.042	.820**	.267	.163***	.024
% female-headed households	−.009	.005	.383***	.027	−.198***	.058	−4.638***	.367	−.249***	.033
Supervision ratio (pop 25–50/5–15)	−.014***	.003	.104***	.019	−.036	.042	−1.101***	.264	−.075**	.024
White isolation index	−.030***	.004	−.561***	.022	−.322***	.047	−1.666***	.295	1.175***	.026
Residential mobility	−.027***	.003	.155***	.016	−.296***	.034	−2.002***	.212	−.132***	.019
Housing vacancy	−.015***	.003	.023	.015	−.158***	.031	−.598**	.198	−.081	.018
% renters	−.021***	.004	−.086***	.023	.332***	.050	−1.986***	.317	.035	.028
Population (logged)	−.006*	.003	.096***	.015	.440***	.032	2.637***	.201	.053*	.018
Mean housing sale price	.000	.002	.001	.012	−.078**	.026	−.045	.166	−.017	.015
Homicide rate per 100,000	−.007	.004	.137***	.020	−.160***	.043	−1.349***	.274	−.110***	.024
Assault rate per 100,000	.003	.003	−.002	.017	−.055	.037	−.107	.233	−.033	.021
Log likelihood	−1,811.890		−1,459.010		−2,984.620		−6,622.130		−1,845.390	
Wald chi-square	5,806.540***		4,002.670***		1,783.070***		1,233.480***		10,036.320***	

*$p < .05$. **$p < .01$. ***$p < .001$.

FIGURE 1
Effects of Immigrant Concentration on Crime by Type of Crime (Z-Score)

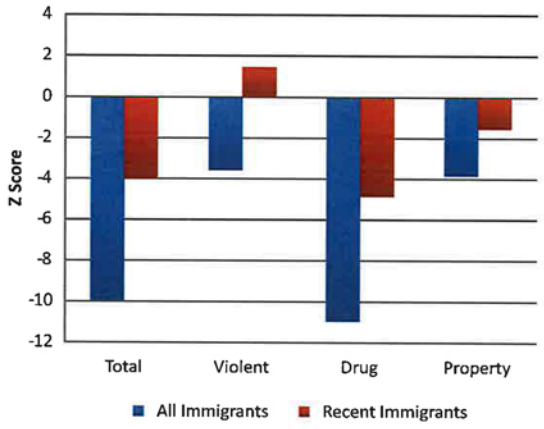
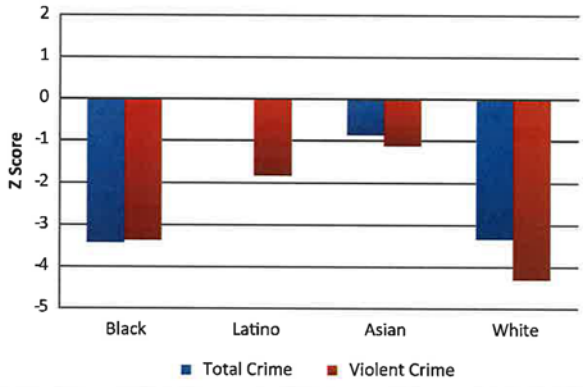
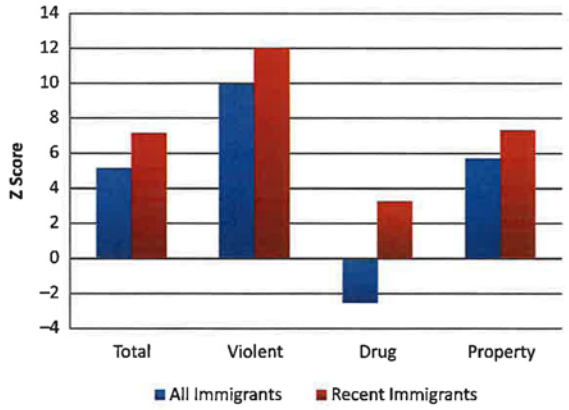


FIGURE 2
Effects of Immigrant Concentration on Crime by Ethnicity (Z-Score)



country for fewer than five years. Although crime is on balance lower in neighborhoods with higher immigrant concentrations, the ratio of stops and arrests to crime is higher in these same places. The police response per crime seems to be more aggressive and legally formal in immigrant neighborhoods, despite the lower crime rates in these neighborhoods. The reasons for this disparity

FIGURE 3
Effects of Immigrant Concentration on Enforcement by Type of Crime (Z-Score)



are neither obvious nor unambiguous. For example, one effect of the higher enforcement rates in immigrant neighborhoods may be lower crime overall, a trend we observed in Figures 1 and 2. But the higher crime rates in black neighborhoods generally are not observed here: immigration exerts a protective effect on crime for black and white immigrants, yet both groups are policed less intensively compared to Latino or Asian neighborhoods. Still, the effect of enforcement on crime is difficult to statistically identify and remains ambiguous absent a more complete empirical understanding of the simultaneous relationships between police stops, race or ethnicity, immigration, and crime.

The disaggregation of race- and ethnic-specific effects presented in Figure 4 further complicates the enforcement narrative. No particular group is driving the overall results from Figure 3; that is, the higher rates of enforcement activity are not concentrated among immigrants of particular races/ethnicities. Latino and Asian locales experience higher enforcement ratios, but the magnitude of the association fails to reach significance. Enforcement is lower in neighborhoods with higher concentrations of white immigrants, after controlling for crime, but again the effect is negligible. Only for African immigrant neighborhoods is there a significant relationship to enforcement, and here the effect is negative: enforcement in black immigrant neighborhoods is far lower than in other immigrant enclaves. This finding is quite unexpected given that policing has tended to be a central feature of areas populated by native-born African Americans (Fagan and Davies 2000; Fagan et al. 2010; Geller and Fagan 2010).

FIGURE 4
Effects of Immigrant Concentration on Enforcement by Ethnicity (Z-Score)

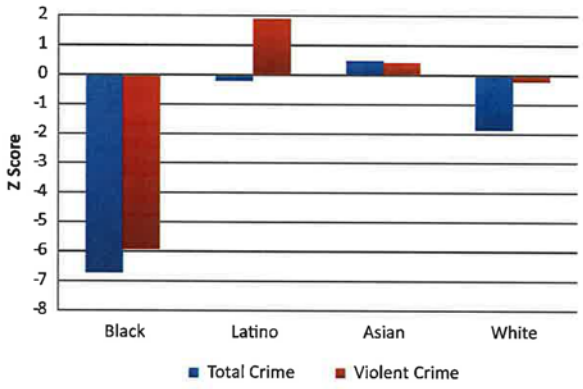
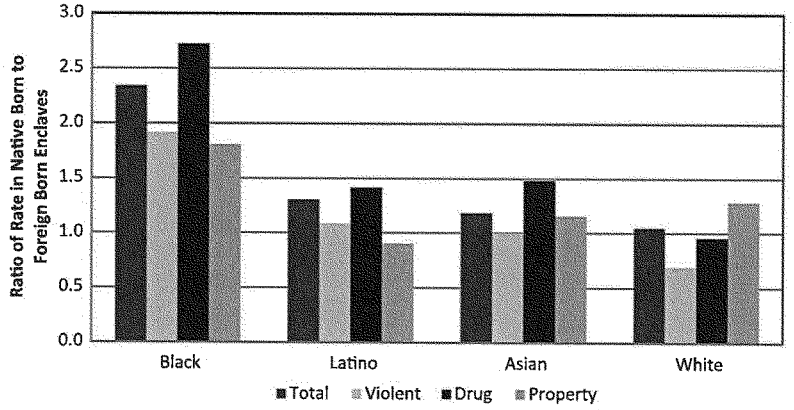


Figure 5 shows the extent of differences in enforcement patterns across foreign-born versus native-born black neighborhoods in NYC. For other racial/ethnic groups, the rates of enforcement between native- and foreign-born enclaves tend to be similar. Interestingly, for both Latinos and Asians, enforcement rates for most types of crime are marginally higher in native-born areas. It is only in white immigrant neighborhoods that immigrants see a greater concentration of enforcement. But for all groups, the differences are not striking, as the ratios of enforcement rates generally cluster around a value of 1.[5] Conversely, the ratios in African areas are markedly higher. For overall crime, the rate of enforcement is more than twice (2.34) as large in native-born neighborhoods; the ratio is higher still for drug offenses. Simply stated, largely native-born black areas experience a much greater concentration of enforcement than do places where foreign-born blacks live.

Discussion

Consistent with much contemporary research, this study confirms that immigration exerts a protective effect on crime, controlling for the characteristics of neighborhoods that tend to attract immigrants. In New York and other cities (Sampson 2008; Martinez et al., this volume), there is no evidence that crime rates are higher in places with higher immigration rates. On the contrary, immigration often functions as a prophylactic against crime. But the results here also demonstrate that all immigrants are not alike; immigration experiences vary

FIGURE 5
Ratio of Enforcement in Native-Born to Foreign-Born Enclaves, by Type of Enforcement (2004–2008)



SOURCE: U.S. Census Bureau, STF-3A; New York City Police Department, Stop and Frisk Archive, Online Booking System.

across racial and ethnic groups. To a considerable extent, these pathways are influenced by broader patterns of race relations in the United States. White immigrants have leveraged their relative advantage to reside in areas characterized by higher socioeconomic status and lower crime, while black immigrants, likely facing some of the same discriminatory forces as native-born blacks, have settled in more deprived, more dangerous areas. In other words, when disaggregated by race, the distributions of relevant characteristics in immigrant neighborhoods closely resemble those evident in native-born neighborhoods.

Race, however, does not tell the whole story. We find support for the notion that "culture matters" (Kasinitz et al. 2008). It is true, for example, that white immigrants such as Russian Jews are less encumbered by invidious discrimination in housing and employment. But compared to other groups, Russian Jews also tend to be more urban and better educated and have family structures (low levels of single-parent households and high levels of intergenerational living) that are more amenable to upward mobility (Orleck 2001; Kasinitz et al. 2008). They may also arrive here with more capital and entry into networks of well-assimilated predecessors. At odds with outmoded expectations of social disorganization and crime, these comparative advantages help explain why crime and violence are less prevalent in white immigrant communities.

A more ambiguous but no less telling example of cultural effects involves Haitians and Jamaicans, "West Indian" groups that together account for much of the black immigration to NYC. Haitians and Jamaicans both are doubly stigmatized. Phenotypically black, both groups are often identified simply as "black" by

whites and experience discrimination and residential segregation similar to native-born blacks. But owing to differences in cultures and languages, both are distinguished within the black population as immigrants (Stafford 1987; Foner 1987, 2007). Still, this "minorities within a minority" status has not increased crime and violence in black immigrant neighborhoods. Instead, there are several aspects of West Indian life that appear to reduce crime: families are frequently multigenerational, parental employment rates are high, and the community has a high level of home ownership (Kasinitz et al. 2008). Moreover, as English speakers, Jamaicans in particular are able to find employment in service industries. These advantages seem to translate into distinctly different treatment of immigrants of African descent compared to native-born African Americans.

The two largest Latino groups, Puerto Ricans and Dominicans,[6] face greater disadvantages than do other immigrant groups in terms of language and economic position upon entry into the United States. However, the interpretation of social class is relative. Ethnographic research with Dominican garment workers in NYC indicates that, despite their hardships, most of these women judged their social status to be "middle class." Pessar (1987) suggests that immigrants' ability to secure prestigious consumer goods that were not necessarily available to the middle class in the Dominican Republic provided the trappings of a middle-class life in the United States. To the extent that immigrants have tangibly improved their station, they have vested interests in their stable, if relatively poor, neighborhoods where isolation is both spatial and social, compounding their economic and other structural disadvantages.

Latino immigrants from South America, including Colombians, Ecuadorans, and Peruvians, while encountering similar barriers related to language, nonetheless tend to have higher levels of education and two-parent families and settle in less-distressed neighborhoods. The net effect of Latino immigration on total crime is negligible; it is negatively associated with violence, but the magnitude of the effect is not statistically significant.[7] The distinct social worlds of the various Latino subgroups highlight the need for research that further disaggregates ethnicity by sending country. This may be possible in the future, as the sizes of these subgroups grow.

The findings for Asian immigration are similarly ambiguous, and similarly illustrate the need for further refinement in the categorization of groups. Although they are growing quickly, Asians are the smallest of the race/ethnic groups. This is reflected in unique residential patterns. There are, for example, no solid Korean immigrant enclaves. There are "medium"-sized settlements of Koreans in Koreatown (Manhattan) and Sunnyside (Queens), but no Korean analogs to Dominicans in Washington Heights (Manhattan), Puerto Ricans in Spanish Harlem (Manhattan), West Indians in Jamaica (Queens), or Russians in Brighton Beach (Brooklyn). This is not merely a function of the size of the Korean population; as with the other groups, culture plays a role. The most recent Korean immigrants are urban, well educated, and middle to upper-middle class. Utilizing a model of small business entrepreneurship, Korean immigrants

have taken advantage of opportunities in commercial districts in minority neighborhoods presented by the outflow of "old immigrant" shopkeepers (Kim 1987). This strategy has yielded tremendous economic benefit, but it has also left Koreans dispersed throughout the city. In lieu of geographic proximity, Koreans have embraced modern institutions, including business associations and especially the Protestant Church, as substitutes for ethnic neighborhoods.

There is no mistaking the center of immigrant life for NYC's Chinese population, as Manhattan's Chinatown has continued to swell with the influx of settlers. Perhaps more than other groups, there is a distinct "old" versus "new" dichotomy at play in relation to Chinese immigration. It was once dominated by "sojourners" from Kwangtung Province, but many of the post-1965 arrivals originally hailed from North China, Shanghai, Hong Kong, Fukien, and Taiwan (Wong 1987). More recent arrivals differ from earlier Chinese immigrants in a myriad of ways, but essentially, the newer immigrants consider themselves to be more urban, literate, and modern than their predecessors. In addition, Cantonese has replaced the Toysan dialect as the primary means of communication, further adding to intergenerational difficulties. The superior social and economic resources of the current wave of Chinese immigrants may well herald lower rates of crime as they become a larger proportion of the population.

In short, the results presented here support the emerging consensus that immigration does not lead to higher rates of crime, and in some instances protects against crime. The story of immigration and crime in NYC comprises many chapters that reflect the unique experiences of different racial and ethnic groups. Each of the groups has added to the revitalization of the city in its own unique way, through repopulation, participation in niche economies, and entrepreneurial spirit. The nature of immigration has changed. At the very least, today's newcomers are better positioned to leverage established ethnic networks for employment opportunities and social support. Writing about the second generation of post-1965 immigrants, Kasinitz et al. (2008, 63) assert that "the parents have developed *cultural understandings* and *strategic repertoires*" for responding to the opportunities and challenges of immigrant life. These understandings and repertoires helped to maintain relatively low crime rates in immigrant neighborhoods.

Despite reduced crime, however, immigrant areas experience disproportionately higher levels of enforcement. The present study does not allow for definitive explanations, but several are possible. First, immigrant neighborhoods tend to be geographically adjacent to traditionally high-crime neighborhoods. Elevated enforcement practices in these immigrant areas may reflect a spillover effect, where the crime risks associated with proximate neighborhoods are also attributed to immigrant neighborhoods. Second, it is possible that the police are reacting to the immigrant status of the neighborhood. Extending the idea of neighborhood racial stigma (Sampson and Raudenbush 2004), the police may see and interpret disorder and crime risk differently in neighborhoods marked by higher concentrations of immigrants. Immigration, like race, may be imbued with social meaning that leads to greater exercise of coercive authority.

As with crime, the results of this study also underscore the importance of disaggregating the relationship between immigration and enforcement. Of particular interest is the significant negative association for black immigrants. We can only speculate about the reasons for this finding. Reduced enforcement in black immigrant neighborhoods may be indicative of underpolicing (Kane 2005). These areas, which are particularly socially isolated (even in relation to other immigrant neighborhoods) and have limited political capital, may experience a form of "malign neglect" in the form of attenuated police resources. Future research should further investigate the nature of this relationship, and whether it is unique to NYC.

The policy implications of this study are complicated, not unexpectedly given the complexity of the associations between immigration, crime, and enforcement. The results presented here add further voice to the growing chorus cautioning against the politically simplistic and expedient scapegoating of immigrants. The stereotypical but erroneous linkage between immigration and crime is increasingly unsustainable, especially to the extent that it may be implicated in unduly high levels of enforcement. There are dangers in this disparity for the second and later generations of immigrants, who experience more intrusive policing regimes than native-born New Yorkers yet are not as involved in crime. The potential alienation of immigrant communities—even law-abiding, cooperative individuals—from the criminal justice system can compromise safety through the loss of legitimacy and the withdrawal of citizens from cooperation with the police (Kirk et al., this volume). Cooperation with the police is an essential component of the production of security (Tyler and Fagan 2008), whereby citizens undertake both individual and collective actions to control crime. Cooperation and security are closely tied for all racial groups, whether immigrants or native-born. Cooperation implies collaboration with the police, and trust in the police is a predicate for such actions. Differential policing of immigrant neighborhoods when crime rates suggest otherwise corrodes trust and creates risks for crime, both now and in the future as immigrants take their place in American society.

Notes

1. The Omniform record collects the two hundred pieces of information for each crime complaint that are entered into the Compstat system used by the NYPD for crime analysis.

2. Since the arrest rates pursuant to street stops are less than 5 percent, there is little overlap in these two measures. See Fagan et al. (2010).

3. For each population group, census tracts with no immigrant population for that group were excluded. The remaining tracts were divided into three quantiles. Tracts in the second, or middle, quantile were categorized as modal neighborhoods.

4. Race- and ethnic-specific models also include the interaction of percentage foreign-born with the GPS score.

5. A ratio of 1 indicates that the enforcement rate in native-born enclaves is equal to the enforcement rate in foreign-born enclaves.

6. Although geographically West Indian, Dominicans are ethnically Latino.

7. It is important to note that there is substantial heterogeneity in Latino populations across cities.

The finding here may reflect New York City's specific composition, and the effect of Latino population may vary from other cities with different compositions.

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ARTICLE

CRIMINOLOGY

More immigrants, less death: An analysis of immigration effects on county-level drug overdose deaths, 2000–2015

Ben Feldmeyer[1] | **Diana Sun**[2] | **Casey T. Harris**[3] | **Francis T. Cullen**[1]

[1]School of Criminal Justice, University of Cincinnati

[2]School of Criminology & Criminal Justice, Florida Atlantic University

[3]Department of Sociology & Criminology, University of Arkansas

Correspondence
Ben Feldmeyer, School of Criminal Justice, University of Cincinnati, 660 Teachers-Dyer Complex, PO Box 210389 Cincinnati, OH 45221-0389.
Email: ben.feldmeyer@uc.edu

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Abstract

Public and political discourse has routinely suggested that immigration is linked to higher community levels of violence and drug problems. In contrast to these claims, research has consistently shown that immigration is not associated with greater violence at the macro level. However, few studies have examined the links between immigration flows and community drug problems. The current study seeks to address this gap in research by providing a county-level longitudinal analysis of immigration and drug overdose deaths both overall and by substance type for the 2000 to 2015 period and provides an analysis of homicide for comparison with prior immigration–crime research. In addition, this analysis compares immigration–overdose relationships across immigrant destination types. The current project relies on overdose and homicide data drawn from the Centers for Disease Control's Restricted Access Multiple Cause of Death Mortality files combined with data on county social, economic, health, and legal contexts drawn from multiple macro-level data sources. Findings

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reveal that immigration is not associated with higher levels of overdose or homicide deaths, and when effects are significant, immigration is linked to lower levels of overdose mortality across multiple substances and destination types.

KEYWORDS
communities, crime, drugs, immigration, macro level, overdose

The United States has a long and tumultuous history surrounding the issue of immigration and crime. As immigration flows into the United States have risen, so too have concerns that migrant populations would generate waves of crime and social problems. Sociologists and criminologists first observed these fears during the European "great migration" at the turn of the twentieth century (Sutherland, 1927; Taft, 1933). Nearly 100 years later, the country finds itself at similar crossroads as it faces a second "great migration."

Between 1970 and 2018, the United States experienced a resurgence in immigration as the foreign-born population increased from 10 million (4.8 percent of the population) to nearly 45 million individuals (13.7 percent of the population) (Budiman et al., 2020). In response to these shifts, many Americans have again voiced growing anxieties about the consequences of immigration. Recent polls reveal that nearly half of the U.S. public believes that immigration is making "the crime problem" worse (Gallup, 2019; see review in Kulig et al., 2021). In addition, such sentiment was perhaps most clearly illustrated when former President Donald Trump attracted support for his candidacy with claims that, "When Mexico sends its people, they're not sending their best. ... They're bringing drugs. They're bringing crime. They're rapists. And some, I assume, are good people" (Rappeport, 2015).

In sharp contrast to these claims, social science research has routinely found that foreign-born residents are less likely to engage in crime and delinquency than their native-born counterparts (Bui, 2009; Chen & Zhong, 2013; Greenman & Xie, 2008; Vaughn et al., 2014; Vega et al., 2011). Similarly, macro-level research has shown that immigration flows are unrelated to crime or, when significant, are linked to lower levels of crime and violence (Feldmeyer, 2009; Lee et al., 2001; Light & Miller, 2018; Lyons et al., 2013; Martinez et al., 2010; Ousey & Kubrin, 2018; Sampson, 2008; Xie & Baumer, 2018, 2019). In sum, despite public concerns and political rhetoric suggesting that immigration is an engine for crime, research has shown that it often either has no impact or may actually help protect communities. Such findings have led several prominent scholars to conclude that the link between immigration and crime is "misleading, to the extent of constituting mythology" (Hagan & Palloni, 1999, p. 630) and that cities with high immigrant concentrations may be "some of the safest around" (Sampson, 2008, p. 30).

Although there is no shortage of macro-level research on immigration and crime, studies to date have focused primarily on violence (often homicide) or on measures of total offending. In contrast, research has given limited attention to how aggregate patterns of immigration impact drug use or related outcomes, such as *drug overdose deaths*. This oversight is surprising for several reasons. First, drugs are often at the center of political rhetoric and public concerns about immigration and crime. Echoing former President Trump, critics of immigration have routinely pointed to drug crime and narcotics trafficking as potential risks of immigration, linking it to

images of Mexican cartels, immigrant drug mules, and reports of drug-related violence in border towns (Borunda, 2020; Fryberg et al., 2012; Greenberg & Qiu, 2016). However, apart from several noteworthy exceptions described below, research to date has been slow to examine whether the neutral or protective effects of immigration on community crime rates also extend to drug crime or drug mortality.

Second, this oversight is also noteworthy because the United States experienced one of the worst drug epidemics on record during the first two decades of the twenty-first century—a period that also saw rapid rises in immigration. Between 2000 and 2019, the United States faced more than 800,000 drug overdose deaths and as many as 100,000 per year (Ahmad et al., 2021). Notably, drug use and overdose trends have varied considerably by type of substance across communities over time. Where some communities have experienced surges in opioid use, other places and times have been plagued by rising levels of heroin, synthetic opioids, or cocaine (Gfroerer et al., 2007; Monnat, 2018). Taken together, overdose death rates more than tripled, rising from 6.1/100,000 to 21.6/100,000, making them the leading cause of injury death in the United States by 2019 (CDC, 2020; Hedegaard et al., 2021). Due in part to this spike in overdose deaths, the United States has begun to see declines in life expectancy for the first time in more than 50 years (Case & Deaton, 2015, 2017; Woolf & Schoomaker, 2019).

Although there are many reasons to suspect that the overdose crisis has not been driven by immigration (as described in the following sections), the fact that these two trends occurred simultaneously has led many to assume they are closely connected. Among conservative media and political spheres, immigration is not simply seen as a potential source of drug crime. It is portrayed as a leading source of drug problems and one of the central narratives for the overdose crisis that has plagued U.S. communities throughout the early twenty-first century (Arthur, 2022; Grassley, 2021; Portman & Katko, 2021; "Trump transcript: President Trump's full speech on immigration," 2019; Weigel, 2022). In light of these pervasive claims, the relationship between immigration and drug overdoses demands empirical attention. Yet, surprisingly few analyses have examined the macro-level relationships between immigration and drug problems to assess whether they are linked across communities over time (overall or by substance type).

Third, almost no research to date has explored the role of changing immigrant settlement patterns and their role in shaping relationships between immigration and drug overdose deaths. Beginning in the late 1990s and early 2000s, immigrants began to bypass traditional metropolitan areas that had historically served as entry points and increasingly settled in newer destinations (including rural or smaller metropolitan locales) throughout the United States (Lichter & Johnson, 2009; Singer, 2004). Social scientists have noted that these new destinations differ in important ways from established destinations, which may shape the impact of immigration on crime in such locales. Notably, prior research has found some indication that the protective effects of immigration may not be as strong in new destinations compared with traditional entry points, with some studies reporting that immigration could even have crime-generating effects in new destinations (Harris & Feldmeyer, 2013; Ramey, 2013; Shihadeh & Barranco, 2013; Xie & Baumer, 2019). However, it is unclear whether relationships between immigration and drug overdose rates vary across these different immigrant settlement contexts.

The current study seeks to address these gaps in research by providing a longitudinal analysis of immigration and drug overdose deaths both overall and by substance type for the 2000 to 2015 period, as well as an analysis of homicide (as a comparison with prior immigration–crime research). In addition, we examine whether there are differences in the relationships between immigration and drug overdose deaths across immigrant destination types. Assessing these relationships is important and helps advance criminological research and theory as well

as broader understandings of the overdose crisis in multiple ways. Theoretically, this holds key implications for the "immigrant revitalization" perspective (see Martinez et al., 2010; Ousey & Kubrin, 2018; Sampson, 2008), indicating whether it extends beyond violence to also insulate U.S. communities from related drug and health problems. Dating back to the work of Shaw and McKay (1942) and Sutherland (1927) and including more recent works of Gottfredson and Hirschi (1990) and Sampson and Wilson (1995), criminologists have long sought to identify "general" explanations and invariance in the factors that account for crime, death, and related behaviors. Evidence that immigration is associated with lower drug overdose rates would go a long way toward establishing the general explanatory power of the immigrant revitalization perspective on crime and related community health problems. In addition and more broadly, assessing whether immigrant flows are tied to overdose concentrations is critical for understanding the sources of the overdose crisis, potential solutions, and the validity of pervasive claims linking immigration and drug mortality.

To conduct this research, we rely on county-level overdose and homicide death data drawn from the Centers for Disease Control's (CDC) Restricted-Access Multiple Cause of Death Mortality data files (hereafter, MCOD), which we combine with measures of immigration and community context drawn from multiple sources over time, including the U.S. Census Bureau, U.S. County Business Patterns data, CDC data on opioid prescriptions, and Institute for Health Metrics and Evaluation [IHME] county profile data. The study relies on fixed-effects analysis (Allison, 2009) of the panel data constructed here to examine relationships between immigration and overdose deaths, including total overdoses, heroin, natural and semisynthetic opioids, synthetic opioids, and cocaine, as well as homicide deaths, net of changes in other county contextual measures.

1 | IMMIGRATION AND CRIME

Immigration has been central to the social fabric of the United States since the nation was first founded. Yet, public and political reception to immigration has, at times, been cold. When migration flows into the United States have risen, fears about the possible effects of immigration on local communities have often followed. These include concerns that immigration could contribute to economic and job competition, strain healthcare and educational systems, overburden social services, and change the existing culture and way of life in the United States (Rumbaut & Ewing, 2007). However, many of the most poignant fears about immigration have centered on public safety and on concerns that it will foster crime. Claims of immigration fueled "zones of lawlessness" have become commonplace among media outlets and partisan think tanks (Horowitz, 2001, p. 8), with similar claims occurring throughout conservative political circles. For example, during his 2008 presidential campaign, Fred Thompson suggested that "twelve million illegal immigrants later, we are now living in a nation that is beset by people who are suicidal maniacs and want to kill countless innocent men, women, and children around the world" (as quoted in Sampson, 2008, p. 29). Almost a decade later, Donald J. Trump offered similar claims, suggesting that Mexican immigrants were "bad hombres" flooding U.S. communities with drugs and violence (Jacobo, 2016; Kulig et al., 2021; Rappeport, 2015).

In response to these rising concerns, social scientists have produced a sizable body of research examining the links between immigration and crime. Notably, this research acknowledges several theoretical reasons why immigration could in fact contribute to elevated rates of crime (see reviews in Feldmeyer, 2009; Martinez & Lee, 2000; Ousey & Kubrin, 2018). For example, traditional versions of social disorganization theory suggest that immigration could increase cultural

and linguistic heterogeneity, the share of residents in poverty, and population turnover in ways that destabilize communities and erode social control. Likewise, strain theories propose that an influx of migrants with low wages, lower educational attainment, and high levels of structural disadvantage might contribute to increased community strain and, in turn, greater criminal offending (Lee et al., 2001; Martinez & Lee, 2000). In addition, even if immigrants are not particularly crime prone, immigration itself may generate more opportunities for crime and victimization because migrant populations are often viewed as vulnerable targets (Shihadeh & Barranco, 2010) and may be hesitant to report crime to police when it occurs (Xie & Baumer, 2018, 2019).

However, in stark contrast to these arguments, empirical research has consistently shown that immigration does not increase crime. At the individual level, foreign-born residents are less likely to commit nearly all forms of crime and delinquency, whether violent, property, or drug offending, compared with native-born populations (Bui, 2009; Chen & Zhong, 2013; Greenman & Xie, 2008; Vaughn et al., 2014; Vega et al., 2011). Similarly, at the macro level, which is the focus here, immigration flows tend to have either null or protective effects on community crime rates (Feldmeyer, 2009; Feldmeyer et al., 2015, 2018; Light & Miller, 2018; Lyons et al., 2013; Martinez et al., 2010; Ousey & Kubrin, 2018; Sampson, 2008; Shihadeh & Barranco, 2010; Xie & Baumer, 2018, 2019).

Although it is beyond the scope of this project to provide detailed reviews of each of these studies, the empirical state of the macro-level literature is perhaps most succinctly summarized in Ousey and Kubrin's (2018) meta-analysis, which synthesized results of 51 analyses of immigration and crime. Across all of these analyses, which include a wide range of study units, offense types, data sources, modeling strategies, and demographic groups, findings were remarkably consistent. As Ousey and Kubrin (2018) noted, immigration most often had no discernable impact on crime rates, but when the effects were statistically significant, immigration typically had protective, crime-mitigating effects. Moreover, not one of the 51 studies in the meta-analysis showed consistent "crime-generating" effects of immigration at the macro level.[1]

To explain such findings, several theoretical arguments have been advanced that have coalesced into the "immigrant revitalization" perspective (see Martinez et al., 2010; Ousey & Kubrin, 2018; Sampson, 2008). This perspective argues that, rather than disorganizing communities and contributing to crime, immigration provides resources and a stabilizing influence that insulates residents, both foreign born and native born, from social problems like crime. As this theory indicates, immigrants to the United States tend to be a select group of motivated individuals that are not particularly crime prone. They tend to have conventional values and goals (emphasizing work and family) and typically have higher than average levels of education, resources, and resilience compared with others in their home country, all of which help to insulate them from crime and drug activity (Chen & Zhong, 2013). The immigrant revitalization perspective also emphasizes that immigration flows commonly occur through chain migration into established ethnic communities where migrants can draw on resources from existing family and friendship networks (see Alba & Nee, 1997; Alba et al., 1999). These networks provide key sources of social capital and support that residents can leverage for a variety of needs, including financial assistance, securing housing or employment, overcoming language barriers, and accessing transportation and childcare (among others) (Feldmeyer, 2009; Lee et al., 2001; Martinez et al., 2010; Ousey & Kubrin 2009,

[1] There are some indications that immigration into "new" or "emerging" destinations may not be as protective as in traditional destinations (Harris & Feldmeyer, 2013; Ousey & Kubrin, 2018; Ramey, 2013; see reviews in Xie & Baumer, 2018, 2019). However, other research has suggested that this effect is temporary and quickly wears off as communities become acclimated to the changing immigration landscape (Barranco et al., 2017). Thus, a growing consensus among social scientists has emerged that immigration is not a key driver of crime.

2018). In turn, these resources are likely to foster social control and insulate communities from the strains and disadvantages that often contribute to crime.

In addition, immigration may generate greater social cohesion within a community as it reinforces common traditions, language, and customs with a new infusion of residents from the county of origin (Feldmeyer et al., 2016; Shihadeh & Barranco, 2010). Research suggests that strong connections to work, religion, and family among immigrant populations help to reinforce community social bonds and bolster these protective institutions as new migrants arrive (Martinez & Lee, 2000; Ousey & Kubrin, 2018). In addition, immigrants may lift local economies because they are often seen as an attractive labor pool that can draw outside investment into communities. Indeed, many small towns and rural areas that had been in decline in the early decades of the twenty-first century have been reinvigorated by the economic revival brought about by immigrant settlement (Carr et al., 2012; Lichter & Johnson, 2020).

Overall, the empirical literature and the arguments highlighted above provide a stark contrast to the persistent fears that immigration has increased crime in U.S. communities. However, this literature has given limited attention to macro-level relationships between immigration and drug-related outcomes. As we highlight below, the drug epidemic of the early twenty-first century coincided with the most recent wave of immigration. Yet, the types of substances and the temporal patterns of overdose that unfolded during this period of time remain complex, particularly as they might be linked to immigrant settlement over that same period.

2 | THE OVERDOSE CRISIS AND IMMIGRATION

Beginning around the turn of the twenty-first century, the United States entered one of the worst crime and public health crises in the nation's history as it faced an unprecedented epidemic of drug overdose deaths. Although it would be overshadowed by the COVID-19 pandemic of 2020, the overdose crisis remains one of the most devastating and deadly epidemics on record in the United States. The Centers for Disease Control and Prevention (Ahmad et al., 2021; CDC, 2020) report that drug overdoses have been responsible for nearly 1 million American deaths since 1999, peaking at more than 100,000 deaths over a 12-month period. For context, annual overdose deaths far surpassed the peak fatalities caused by the AIDS epidemic in the 1980s and 1990s (Torian et al., 2011) and accounted for more than double the peak annual homicide deaths occurring in the early 1990s (Cooper & Smith, 2012). As a result, overdoses are among the leading cause of accidental deaths in the United States, overtaking both automobile and gun deaths (CDC, 2020).

To explain the overdose epidemic, experts have often pointed to "supply-side" arguments, suggesting that increased opportunity and availability of drugs (i.e., more pills, more prescriptions, and more heroin and fentanyl supply) have fueled the overdose crisis (see Alpert et al., 2019; Cicero et al., 2014; Feldmeyer et al., 2022; Monnat, 2019; Monnat et al., 2019; Peters et al., 2020; Pitt et al., 2018; Ruhm, 2019; Sun et al., 2022; Thombs et al., 2020; Van Zee, 2009). However, public health and social science scholars have also increasingly offered "demand-side" explanations for the overdose crisis. Research suggests that economic restructuring patterns dating back to the late twentieth century, combined with the Great Recession of the early twenty-first century, have contributed to a growing presence of "communities of despair" (e.g., decline in manufacturing jobs, increased poverty and disadvantage, population decline, and weakened social services and support) that has fueled drug problems across U.S. communities (Case & Deaton, 2015, 2017; Feldmeyer et al., 2022; Monnat, 2018, 2019; Woolf & Schoomaker, 2019). Similarly, public health research suggests that unmet health needs have also likely contributed to the overdose crisis, as people responded

to chronic pain and persistent physical and mental health ailments by turning to opioids and other drugs (Becker et al., 2008; Case & Deaton, 2015, 2017; Feldmeyer et al., 2022; Guy et al., 2017; Popovich, 2016).

Central for our purposes, political and public narratives have also commonly suggested that immigration might be a key driver of drug-related crime and overdoses in U.S. communities. The most prominent voices issuing these claims have stemmed from conservative politicians and media outlets. For example, former Arizona Governor Jan Brewer claimed that most Mexican immigrants were drug mules (Jacobson, 2010). Similarly, former President Donald Trump routinely invoked images of drug cartels and Latin American gangs (e.g., MS-13) to suggest that "heroin was pouring across the southern U.S. border" and fueling nationwide drug problems (Greenberg & Qiu, 2016). Notably, prominent journalist accounts have also drawn links between immigration and the overdose crisis. In his award-winning book, *Dreamland*, Sam Quinones (2015) indicated that Mexican immigrant drug trafficking networks from Xalisco, Nayarit distributed large amounts of low-cost "black tar" heroin into U.S. communities and were key contributors to the national overdose epidemic. To be fair, Quinones acknowledged that drug trafficking was isolated to a small share of immigrants. However, combined with media coverage of drug-related violence in Mexican border cities and towns (e.g., Juarez, Mexico—see Borunda, 2020), such accounts have helped reinforce narratives in the public conscience linking immigration to drug crime and the drug crisis.

By contrast, few academic experts have voiced support for such claims (see reviews in DEA, 2015; Light et al., 2017). In fact, public health researchers have argued the opposite, suggesting that immigration may be linked to lower community levels of drug crime and drug mortality. At the individual level, scholars have noted that foreign-born individuals are less likely to use and abuse drugs than are native-born individuals, net of other factors (see, e.g., Bui, 2009; Canino et al., 2008; Ojeda et al., 2008). Likewise, ties between immigration, drug crime, and overdose mortality at the macro level would be somewhat surprising given prior research (highlighted above) showing that immigration consistently has either neutral or protective effects on community levels of violence or overall crime.

However, scholars have given surprisingly little empirical attention to macro-level relationships between immigration and drug problems. Although there is a sizable body of macro-level research on immigration and crime, only a small handful of these studies include any drug-related measures, and nearly all of those have focused on a single outcome of "drug-related homicides." For example, studies by Martinez et al. (2003, 2004) and by Nielsen et al. (2005) examined associations between immigration and drug-related homicides in Miami and San Diego neighborhoods. They found that immigration was generally linked to fewer drug-related homicides in Miami neighborhoods and for Latinos in both cities, but they report mixed effects on drug-related homicides in San Diego (depending on the race/ethnicity of the victim and the year of entry). Likewise, Martinez et al. (2003) found that victimization from drug-related homicides was similar across immigrant and nonimmigrant groups in Miami but with somewhat higher rates for Afro-Caribbean immigrants. Two studies by Ousey and Kubrin (2009, 2014) examined immigration effects on drug-related homicides across U.S. cities and found that immigration was associated with lower drug homicide levels. A study by Green (2016) found that undocumented immigration rates had a small but significant association with drug arrests (but not violent crime). Last, in what is perhaps the most direct macro-level test to date of immigration effects on drug use, Light and colleagues (2017) found that undocumented immigration was either unassociated with or linked to lower state overdose death rates.

2.1 | Extending Prior Research on Immigration and Drugs

Beyond these noteworthy exceptions, macro-level research on immigration and drugs, and especially the drug overdose crisis, remains limited; questions remain about whether immigration has (or has not) contributed to the overdose crisis. In addition to this scarcity of scholarship on the topic, we identify three further research gaps. First, the studies highlighted above have largely relied on cross-sectional data around the year 2000, well before the full onset of the overdose crisis (Green, 2016; Martinez et al., 2003, 2004; Nielsen et al., 2005). Light et al. (2017) offered one of the only existing longitudinal analyses of immigration and drug abuse with national coverage. However, their analysis was restricted to the state level, which may mask internal heterogeneity for immigrant versus nonimmigrant communities. Thus, a need exists to extend this work to examine these relationships at more local contexts over time.

Second, prior research has focused on a limited set of dependent variables, typically using measures of drug-related homicides (e.g., Martinez et al., 2003, 2004; Neilsen et al., 2005; Ousey & Kubrin, 2014) or occasionally measures of total drug arrests or overdose (see Green, 2016; Light et al., 2017). Yet, immigration might impact overdose rates for different substance types in unique ways. Indeed, prior research has shown that the correlates of overdose deaths differ across substance type (heroin versus natural opioids versus cocaine, etc.) (Peters et al., 2020), a finding that may also occur for immigration's effects on overdose.

Third, research on immigration and drug use has not explored differences in effects by the destination type of the receiving community. As noted, some evidence suggests that the protective or "revitalizing" effects of immigration may be stronger in traditional destinations with established immigrant communities (Harris & Feldmeyer, 2013; Ramey, 2013; Xie & Baumer, 2018). In contrast, new destinations, small destinations, and nontraditional entry points that are unaccustomed to immigrants may not experience the same protective effects of migration, and it is unclear whether these differing effects by destination type also extend to drug overdoses.

3 | THE CURRENT PROJECT, DATA, AND METHOD

In light of the gaps in research identified above, the current project seeks to examine the macro-level relationships between immigration and drug overdose deaths across U.S. counties for the 2000 to 2015 period. Specifically, we seek to identify how changes in immigration are related to total overdose death rates over time, as well as to overdose death rates for specific substances (natural and semisynthetic opioids, synthetic opioids, heroin, and cocaine). In doing so, we also examine immigration relationships with homicide for comparison purposes (and mirroring prior analyses of immigration and crime), and we examine differences in each of these relationships across types of immigrant destinations.

3.1 | Data

Data for the current project are drawn from multiple sources covering the 2000 to 2015 period. Information on drug overdose and homicide deaths are taken from the CDC Restricted-Access MCOD Mortality data (CDC, 2020). The CDC MCOD Mortality data contain all death records in the United States from 1999 to 2016 and report the underlying cause of death (e.g., overdose and

homicide), other contributing causes of death (including specific substance type present in the deceased based on toxicology reports), and location (county of residence) for each death.[2]

The Restricted-Access MCOD data have several key strengths that make them particularly well suited for this analysis. First, they offer one of the only available sources of data on overdose deaths (and homicide) covering the entire United States annually from 1999 to 2016 that can be aggregated across place, time, and drug type. Second, the restricted-access MCOD data report all deaths, including those in places with less than 10 overdoses in a year as compared with the CDC's public-access mortality files (CDC, 2020) that report data only for places with at least 10 overdose or homicide deaths in a year. Notably, this type of "censoring" in the public-access data eliminates places with few or no overdoses from the analysis, which is problematic for identifying communities that are insulated from overdose deaths (Feldmeyer et al., 2022). Third, the MCOD mortality data are advantageous for examining macro-level patterns of severe drug use because they are free from many of the biases associated with official statistics on drug crime. Indeed, there are few sources of nationally representative data on community patterns of drug use or drug crime that do not suffer from enforcement and reporting biases (e.g., FBI Uniform Crime Reports [UCR] counts of drug-related arrests and conviction data) or variation in criminal justice priorities and policies across place (see reviews in LaFree et al., 2006; Mosher et al., 2010).

Information on the ecological (e.g., social-economic, legal, and health) characteristics of counties used to create our independent variables is drawn from the following sources: 1) U.S. Census (decennial and American Community Survey [ACS], 5-year estimates), 2) U.S. Census County Business Patterns data, 3) IHME county profiles, 4) FBI UCR, 5) CDC Opioid Prescription data, and 6) the Bureau of Justice Statistics (BJS).

3.2 | Unit of Analysis

Data on mortality and ecological context were combined and aggregated to the county level, which is our unit of analysis. County-level measures were compiled at 5-year intervals to examine longitudinal changes in overdose, homicide, and county context. Following practices of prior research and because small counties have been shown to produce unreliable estimates in aggregate measures of mortality and county context, the current analysis includes only those counties with at least 5,000 residents in 2015 (Monnat et al., 2019; Rosenfeld et al., 2021). This resulted in a county-level panel dataset that includes 2,861 counties with measures at four time points: 2000, 2005, 2010, and 2015.

Although ecological studies of crime and social problems have used a variety of study units (census tracts, counties, cities, metropolitan statistical areas, states), counties are best suited for the current project for several reasons. First, counties are well-established study units for examining aggregate patterns of crime and overdose deaths (see Dwyer-Lindgren et al., 2016, 2018; Feldmeyer et al., 2022; Light & Harris, 2012; Monnat, 2019; Ruhm, 2019). Second, counties are the smallest study unit identified in the MCOD mortality data that may be used to aggregate deaths by location (CDC, 2020). Third, county-level analyses provide coverage for the nation as a whole rather than a

[2] At the time of our analysis, variables for the 2020 period were not yet available for all predictors. Thus, our analysis covers 2000 to 2015, which offers more complete data coverage. In addition, we begin our analysis in 1999/2000 because the MCOD classifications shifted from the Ninth Revision International Classification of Diseases (ICD-9) to ICD-10 in this year, and combinations of ICD-9 and ICD-10 may create inaccurate cause of death classifications (see Anderson et al., 2001).

sample of neighborhoods within one or two cities. In addition, they include both rural and urban areas, which is important given that drug overdoses have been a growing problem in both rural and urban communities (Cerdá et al., 2017; Dombrowski et al., 2016; Keyes et al., 2014).

To assess the impact of immigrant destination type on immigration–overdose relationships, we follow the categorizations of Xie and Baumer (2018, 2019) and divide counties into three destination types: traditional destinations, small destinations, and new destinations. Traditional destinations include counties where the foreign-born percentage of the population was higher than the national average in both 2000 and 2010. Small destinations include counties with foreign-born percentages that fell below the national average in both 2000 and 2010. New destinations include counties with foreign-born percentages that were lower than the national average in 2000 but higher than the national average in 2010.[3]

3.3 | Dependent Variables

The key dependent variables for the current study are *total overdose* and substance-specific overdose death rates for 1) *natural and semisynthetic opioids* (e.g., morphine and prescription pain killers), (2) *synthetic opioids* (e.g., fentanyl), (3) *heroin*, and (4) *cocaine*. For comparisons with prior research, county-level rates of *homicide* are also examined. County homicide and overdose rates were created using the CDC's restricted access MCOD Mortality data at 5-year intervals (2000, 2005, 2010, and 2015) based on the county of residence and the underlying cause of death reported (CDC, 2020).[4] All homicide and overdose rates were age adjusted following estimation procedures used in CDC calculations of overdose death rates (see Dwyer-Lindgreen et al., 2016; Hedegaard et al., 2021). In addition, death rates were calculated using 3-year averages (i.e., 1999–2001, 2004–2006, 2009–2011, 2014–2016) to add stability to estimates and minimize the impact of single year fluctuations (see LaFree et al., 2010; Steffensmeier et al., 2010, 2011). All dependent variables were square root transformed to account for skewness in mortality measures and to impose normal distributions on the data.[5]

3.4 | Independent Variables

Drawing from theory and prior macro-level research on immigration and crime, we include a variety of measures capturing county social, economic, health, and legal context for the 2000 to 2015 period. The primary predictor of interest is county-level *immigration*, measured as the percentage of the county population that is foreign born in 2000, 2005, 2010, and 2015. In addition to immigration, we also include several measures commonly used in prior macro-level research

[3] Prior research has employed alternative specifications for destination types, including use of growth rates between two points in time (Shihadeh & Barranco, 2013) or categorizations based on places with foreign-born populations above the national average in prior decades (Singer, 2004). Still others use a combination of such criteria (Harris & Feldmeyer, 2013). These alternative criteria produce similar classification schemes to those used here.

[4] Overdose deaths from specific substances are identified based on the following multiple-cause-of-death codes from the ICD-10 (1999–2016): heroin, T40.1; natural and semisynthetic opioids, T40.2; synthetic opioids excluding methadone, T40.4; cocaine, T40.5 (Hedegaard et al., 2021; Rudd et al., 2016).

[5] Preliminary analysis revealed that skewness remained with log transformation but not with square root transformation. However, we examined several additional models to explore alternative modeling strategies (log transformations, count-based models), which we describe in the Supplemental Analysis section.

to reflect the social and economic circumstances of places (Land et al. 1990; Ousey, 2017; Peterson & Krivo, 2010; Steffensmeier et al. 2010, 2011). Notably, these variables are also theoretically important for capturing patterns of socioeconomic decline occurring in the early twenty-first century across communities during the Great Recession. To capture county socioeconomic context, we created a *concentrated disadvantage index* ($\alpha = .756$). Following practices of prior macro-level research (Peterson & Krivo, 2010; Land et al., 1990; Steffensmeier et al., 2010), this measure was created using principal components analysis to combine measures of the percentage of residents in poverty, percent unemployed, percent aged 25+ with less than a high school education, and percentage of female-headed households with children younger than 18. All measures of immigration and disadvantage were drawn from U.S. Census data.[6]

We include a measure capturing county *manufacturing employment* rates (manufacturing jobs per 1,000 residents) for each time point drawn from U.S. Census County Business Patterns data (see Light & Thomas, 2019; Monnat, 2018; Ousey, 2017). Community institutional support is measured by the presence of *social assistance agencies* (number of civic associations, nonprofit organizations, social assistance, and healthcare firms) per 1,000 residents, drawn from U.S. Census County Business Patterns data (see Ousey, 2017; U.S. Census Bureau, 2018; Zoorob & Salemi, 2017). We include several measures of community stability commonly used in macro-level analyses of crime based on social disorganization theory and drawn from U.S. Census data, including measures of *residential mobility* (percentage of residents who moved in the last year) and *racial/ethnic heterogeneity* (an entropy index of diversity; Feldmeyer, 2009; Pratt & Cullen, 2005).[7]

Drawing from prior research, we created several measures of county drug availability and health context that may be related to county overdose patterns. We include a measure of *opioid prescription rates* drawn from the CDC's county opioid prescription data, measuring the number of retail opioid prescriptions per 100 county residents (see Dowell et al., 2017; Pardo, 2017; Patrick et al., 2016).[8] We include a measure of county *health insurance coverage* (percent residents with health insurance) drawn from the U.S. Census Bureau (2019), Small Area Health Insurance Rates program (SAHIE) that captures both private insurance and Medicaid, including Medicaid expansions from the Affordable Care Act. To measure county-level health problems, we created a *community health problems index* using principal components analysis to combine county-level, age-adjusted rates of cardiovascular disease (per 100,000), lung disease (per 100,000), and average life expectancy drawn from IHME county profile data (IHME, 2020).[9]

[6] Annual ACS data were used in combination with year 2000 and 2010 data to interpolate year 2005 estimates (for similar applications using interpolation for inter-census years, see Ousey, 2017; Steffensmeier et al., 2011). Factor loadings for variables included in the disadvantage index all exceeded .5, indicating that they loaded onto a single underlying construct.

[7] In preliminary analyses, we also explored several alternative measures of racial/ethnic composition, including %Black and %Hispanic (instead of the diversity index used here). However, these measures (especially %Hispanic) were highly correlated with several independent variables used in our models, including immigration.

[8] County opioid prescription data were available from 2006 to 2017 and cover 90 percent of all retail pharmacy prescriptions. Year 2000 and 2005 rates were interpolated based on average annual changes in prescription rates for later years (CDC, 2020). In supplemental models, we included measures of county opioid distribution (mg. per 1,000 residents) drawn from the U.S. Drug Enforcement Agency's (DEA) ARCOS data. However, these measures were limited to two time points (2005 and 2010) and were highly co-linear with our opioid prescription rate variable.

[9] Similar to the disadvantage index, the community health problem index has a mean of 0 and standard deviation of 1 for the total sample of county years. IHME data provide several additional county health measures, including rates of various forms of cancer, diabetes, asthma, and rates of smoking (among others). We created several alternative county health indices using these measures in preliminary analyses, which produced similar effects to those shown here.

In addition, we include several control measures commonly used in macro-level research to account for county criminal justice and demographic contexts that may impact crime or substance use (see reviews in Feldmeyer & Cochran, 2018; Pratt & Cullen, 2005). These include measures of the county *total population*, the *percent young males* (ages 15–24), *police per capita* (police officers per 1,000 residents), and the state *prison population*.[10]

Preliminary analysis (Moran's *I* tests) revealed significant spatial autocorrelation in both homicide and overdose death rates. Thus, we created *spatial lag measures* for all homicide and overdose measures. Following practices of prior research, we used a two-stage approach (Land & Deane, 1992; Monnat, 2018) in which we regressed each of our dependent variables onto the full set of independent variables and then created a spatial weight matrix (using GeoDA) to identify neighboring counties using the queen first-order contiguity criterion. We then used this matrix and the predicted values to generate spatial lag measures.

Variables drawn from the U.S. Census, the UCR, and BJS data had almost no missing values. Data on manufacturing employment and opioid prescription rates were missing for a small set of counties each year. Following the practices of prior research, we used multiple imputation to account for missingness in the data (Allison, 2000; Monnat, 2018).[11]

3.5 | Analytic Strategy

Using the panel data described above, we use fixed-effects models that include both county and time fixed effects to assess how changes in immigration (and other contextual measures) are associated with changes in county homicide and overdose rates across four time points (2000, 2005, 2010, and 2015). Notably, fixed-effects models are advantageous here for several reasons (Allison, 2009; Firebaugh et al., 2013; Light & Thomas, 2019). First, an advantage of fixed-effects models is that they move beyond cross-sectional models by identifying how *changes* in immigration and overdose deaths have occurred within counties in recent decades rather than assessing these relationships at single time points. Second, a key strength of fixed-effects models is that by focusing on within-unit changes in predictors and outcomes (e.g., within-county changes in immigration and overdose rates), they eliminate any potential omitted variable bias from unmeasured predictors that are time stable.[12]

To ensure that collinearity was not a problem, we paid close attention to associations among predictors. Evaluation of collinearity diagnostics showed variance inflation factor (VIF) values that were below standard cutoffs (less than 4.0) for all variables included here. Preliminary analysis revealed few outliers based on Cook's *D* test and DFFITS, indicating that extreme cases had little impact on the substantive findings. In addition, we explored several alternative modeling strategies and variables (e.g., log-transformed dependent variables and instrumental variable analysis), which we describe in the Supplemental Analysis section.

[10] Measures of county population and percent young males were drawn from U.S. Census data (decennial and ACS). Police presence data were drawn from the FBI's UCR: Police Employee data 2000 to 2015. Incarceration measures were drawn from the Bureau of Justice Statistics (BJS), National Prison Statistics (NPS) 2000 to 2015.

[11] Multiple imputation estimates were created using multivariate normal regression procedures with 10 iterations. Results using alternative multiple imputation estimates (e.g., Markov-chain Monte Carlo methods and 20 iterations) and case-wise deletion were similar but effects were less consistent in models using case-wise deletion.

[12] In preliminary analyses, we conducted Hausman tests to determine whether random effects or fixed effects were more appropriate. The Hausman tests for all models were significant, indicating that fixed-effects models were more appropriate for the current analysis.

Our analysis proceeds as follows. We first provide descriptive statistics of county patterns of homicide, overdose, and ecological characteristics for each time point in the analysis (2000, 2005, 2010, and 2015). Mirroring the approach of prior immigration and crime research, we display the fixed-effects multivariate models for homicide as a point of comparison for our subsequent overdose models. We then move to the main portion of the analysis and provide results of the fixed-effects analysis of overdose rates, overall and by specific substance type. Last, we replicate all models after disaggregating counties by destination type to assess whether immigration–overdose effects differ by context of reception.

4 | FINDINGS

Table 1 provides the descriptive statistics for county measures of homicide, overdose deaths, immigration, and other social and contextual characteristics covering the 2000 to 2015 period. Turning first to our outcome measures, table 1 shows that homicide rates were generally stable (between 5.4 and 5.9 per 100,000) during the study period, but county overdose death rates increased dramatically from 2000 to 2015. In 2000, the average county overdose rate was 4.3 per 100,000 people. By 2015, this rate had more than tripled to 15.5 per 100,000. Similar increases also occurred for each of the specific substance types. Natural and synthetic opioids saw more than five-fold increases in overdose death rates, rising from 0.85 and 0.34 (respectively) in 2000 to rates of 4.7 and 2.8 per 100,000 by 2015. The cocaine death rate doubled, and the heroin overdose rate rose from 0.30 per 100,000 in 2000 to 2.32 in 2015 (an 8-fold increase). In sum, our descriptive statistics mirror the growing body of research illustrating how overdose death rates escalated rapidly in the early twenty-first century. However, standard deviation values also indicate that these patterns varied markedly across place, with some counties experiencing surges in overdose rates that were more than four or five times the national average, whereas others saw minor increases in overdose deaths.

With respect to immigration, table 1 reveals that, on average, counties saw increases in immigrant concentration from 2000 to 2015. In 2000, foreign-born residents accounted for approximately 2.8 percent of county populations on average. By 2015, the average county percent foreign born had risen to nearly 4.0 percent. However, standard deviations again show that shifts in the foreign-born population varied widely across counties. Some counties saw the percent foreign born increase by 10 or 20 percentage points, whereas others experienced stable or even declining shares of immigration.

Regarding our other predictors, county levels of structural disadvantage and especially unemployment increased from 2000 to 2010 (likely capturing the economic decline of the 2008 Great Recession) but improved by 2015. Counties also saw steep declines in the presence of manufacturing jobs and in community health problems over time but steady growth in population size, racial/ethnic heterogeneity, and police per capita throughout the study period. The rate of opioid prescriptions increased from 2000 to 2010 but declined by 2015. Other county contextual characteristics were largely stable or saw only small changes over time.

4.1 | Multivariate Analysis

We now turn to the fixed-effects analysis to examine the longitudinal relationships between county changes in immigration, homicide, and overdose rates (along with other contextual

TABLE 1 Descriptive Statistics for Dependent and Explanatory Variables, 2000–2015

Variables	Overall		2000		2005		2010		2015	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Dependent Variables[a]										
Homicide	5.63	3.96	5.87	4.12	5.76	4.03	5.37	3.74	5.50	3.92
Overdose	10.27	9.44	4.31	4.07	9.38	7.54	12.20	9.82	15.48	11.12
Natural Opioid Overdose	2.86	4.57	.85	1.65	2.05	3.01	3.97	5.97	4.65	5.25
Synthetic Opioid Overdose	1.31	2.60	.34	.79	.83	1.55	1.33	2.05	2.84	4.13
Heroin Overdose	.84	2.16	.30	.75	.32	.80	.54	1.23	2.32	3.73
Cocaine Overdose	.94	1.64	.60	1.17	1.27	1.93	.72	1.22	1.19	1.98
Independent Variables										
% Foreign born	3.45	4.81	2.75	4.15	3.50	4.85	3.61	4.81	3.96	5.32
Disadvantage	.00	1.00	−.05	.99	.02	1.00	.15	1.01	−.12	.98
% Poverty	16.56	7.04	15.10	6.94	16.50	6.92	17.42	6.93	17.24	7.13
% Unemployment	7.31	3.56	5.99	2.91	7.28	3.28	9.07	3.90	6.84	3.33
% Family Structure	11.60	5.05	10.94	4.89	11.97	5.32	11.79	4.84	11.71	5.08
% Without HS Degree	19.04	8.52	24.68	9.01	18.98	7.80	17.31	7.28	14.97	6.58
Manufacturing Jobs	51.97	55.56	67.53	67.31	52.58	53.74	41.74	44.99	45.83	50.11

(Continues)

TABLE 1 (Continued)

Variables	Overall		2000		2005		2010		2015	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Social Asst. Agencies per 1,000	1.97	.80	2.01	.73	1.95	.79	1.94	.76	1.97	.90
Residential Mobility	14.24	5.64	15.71	7.37	14.55	5.08	13.45	4.68	13.22	4.57
Race/Eth Heterogeneity	.29	.18	.26	.17	.29	.18	.29	.18	.31	.18
Opioid Rx Rate	81.30	51.49	72.34	52.07	80.49	47.25	91.63	53.82	80.48	50.71
% Insured	84.04	5.67	84.87	5.00	82.42	5.48	81.35	5.12	87.80	4.77
Comm. Health Problems	.00	1.00	.39	.88	.08	.93	−.27	.97	−.22	1.07
Total Population	44,809.14	167,404.90	36,951.17	141,693.10	46,197.65	174,592.70	45,447.70	166,897.50	50,965.50	184,113.90
% Young Males	10.03	2.50	10.06	2.27	10.36	2.45	9.79	2.50	9.88	2.74
Police Per Capita	193.87	168.58	182.25	167.98	188.94	146.17	201.59	185.41	203.18	171.71
Prisoners	36,589.16	40,115.12	34,513.76	40,893.24	36,087.29	39,963.68	38,554.31	41,485.73	37,210.30	37,756.00
N	11,444		2,861		2,861		2,861		2,861	

Notes: Mortality rates are square root transformed in multivariate analyses to adjust for skewness in these measures. Nontransformed rates are presented here for ease of interpretation.
"Total homicide, overdose rates, and overdose rates for specific substances are age adjusted using 10-year age groupings.

changes) during the 2000 to 2015 period. The results for all counties are shown in table 2, with results for homicide death rates in model 1, total overdose death rates in model 2, and overdose deaths by specific substance type shown in models 3 through 6.

To provide a comparison with prior immigration–crime analyses, we first turn to the analysis of homicide in model 1. Similar to prior research (see review in Ousey & Kubrin, 2018), immigration's relationship with homicide was negative in direction but null at standard significance levels. Thus, county growth in immigration was not associated with significant rises in county homicide rates from 2000 to 2015.

Turning to the main focus of our analysis, table 2 shows that immigration was associated with lower overdose death rates, overall and for some specific substances. In model 2, increases in immigration were associated with significant declines in total overdose death rates ($b = -.045$, $p < .001$), net of other factors. Similarly, models 5 and 6 show that rising levels of immigration were associated with significant declines in lethal overdose rates for heroin ($b = -.012$, $p < .05$) and cocaine ($b = -.013$, $p < .05$). Immigration effects on both natural and synthetic opioids did not reach significance (models 3 and 4). However, we note that these effects were both negative in direction. Thus, the findings from table 2 indicate that county immigrant growth had either neutral effects or more often contributed to lower overdose death rates overall and across multiple substances.

We note that several control variables also had significant relationships with homicide and overdose death rates. Growth in manufacturing employment was associated with declines in overdose death rates for several of the substances examined here. An increasing presence of social assistance agencies was linked to lower homicide and overdose deaths for natural opioids. Rises in community health problems were associated with higher rates of homicide and natural opioid overdose deaths but with lower rates of cocaine and synthetic opioid overdoses. Rising levels of structural disadvantage and racial/ethnic diversity were linked to higher levels of homicide and some forms of overdose deaths, whereas population growth was tied to fewer homicides and total overdoses. Last, table 2 shows that spatial lag effects were significant in all models, indicating that homicide and overdose death rates rose when bordering counties that also experienced increases in these forms of mortality.

Taken together, the results from table 2 show that growth in immigration has not coincided with rising county levels of homicide or drug overdose deaths during the 2000 to 2015 period. In contrast, growing foreign-born populations were associated with significantly lower levels of drug overdose deaths, net of other factors. However, these effects may differ depending on the context of immigrant reception, which we turn to below.

4.2 | Multivariate Analysis Across Destination Types

As noted, research suggests that the relationships between immigration and crime may depend on the destination type of locales, with traditional destinations experiencing stronger "revitalizing" or protective effects of immigration on crime. In contrast, immigration may not have the same protective effects in small or newer destinations that are unaccustomed to the inflow of migrant groups (Harris & Feldmeyer, 2013; Shihadeh & Barranco, 2013; Xie & Baumer, 2018). To explore these possibilities, we replicated the fixed-effects models from table 2 after disaggregating counties into three different destination types: "traditional destinations" (table 3), "small destinations" (table 4), and "new destinations" (table 5).

TABLE 2 Multivariate Models of Homicide and Overdose Deaths by Substance Type for All Counties, 2000–2015

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Homicide		Overdose		Natural Opioids		Synthetic Opioids		Heroin		Cocaine	
Variables	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*
% Foreign born	−.002	.001	−.045***	.010	−.016	.009	−.007	.007	−.012*	.005	−.013*	.006
Disadvantage	.029***	.003	.092**	.035	−.012	.029	.032	.024	.026	.019	.015	.021
Manufacturing Jobs	.000	.000	.000	.000	−.001**	.000	−.001	.000	−.001**	.000	−.001*	.000
Social Asst. Agencies	−.004*	.000	−.050	.000	−.067**	.000	.020	.000	.005	.000	−.012	.000
Residential Mobility	.000	.000	.008**	.003	.002	.003	.003	.002	.002	.002	.002	.002
Race/Eth Heterogeneity	.513***	.036	.391	.456	.393	.381	.474	.315	.643**	.243	.154	.270
Opioid Rx Rate	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000
% Insured	.001***	.000	.001	.003	.003	.003	.003	.002	.001	.002	.002	.002
Comm. Health Problems	.100***	.005	.111	.066	.150**	.055	−.126**	.046	−.052	.036	−.153***	.040
Total Population	−5.230E-08**	.000	−5.750E-07*	.000	−2.530E-07	.000	−1.240E-07	.000	8.840E-09	.000	−1.280E-07	.000
% Young Males	.006***	.001	−.009	.013	−.004	.011	.002	.009	.005	.007	.007	.008
Police Per Capita	−4.880E-06	.000	1.284E-04	.000	1.012E-04	.000	−1.527E-04	.000	−4.420E-05	.000	3.200E-07	.000
Prisoners	−6.730E-07***	.000	1.780E-06	.000	1.710E-06	.000	3.160E-06*	.000	9.460E-07	.000	1.690E-06	.000
Spatial lag	.804***	.012	.460***	.018	.584***	.019	.785***	.017	.888***	.015	.536***	.023
Year 2005	.019***	.003	.602***	.040	.220***	.031	.003	.025	−.039*	.019	.114***	.022
Year 2010	.043***	.005	.862***	.066	.473***	.054	−.014	.042	−.035	.033	−.075*	.036
Year 2015	.032***	.005	1.237***	.068	.597***	.055	.279***	.043	.209***	.033	.067	.037
Intercept	.118**	.036	1.044**	.371	.089	.309	−.348	.255	−.183	.197	.075	.220

Notes: $N = 2{,}861$. ".000" values indicate small numeric values that extend beyond the thousands decimal place.

*$p < .05$; **$p < .01$; ***$p < .001$.

TABLE 3 Multivariate Models of Homicide and Overdose Deaths by Substance Type for Traditional Destination Counties, 2000–2015

Variables	Model 1 Homicide		Model 2 Overdose		Model 3 Natural Opioids		Model 4 Synthetic Opioids		Model 5 Heroin		Model 6 Cocaine	
	b	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*
% Foreign born	.004	.002	−.007	.018	−.005	.013	−.003	.010	−.017	.011	−.008	.012
Disadvantage	.050**	.016	.243*	.118	.083	.087	.061	.068	−.032	.070	.002	.075
Manufacturing Jobs	.000	.000	−.002	.002	.000	.001	−.119	.001	−.003*	.001	−.002	.001
Social Asst. Agencies	−.033	.000	.069	.002	.020	.001	.198*	.001	.138	.001	.252**	.001
Residential Mobility	.001	.001	.014	.011	−.000	.008	.007	.006	.012	.006	.008	.007
Race/Eth Heterogeneity	.579***	.156	.895	1.143	1.320	.857	2.682***	.669	1.700*	.694	.889	.738
Opioid Rx Rate	.000	.000	.003	.004	.003	.002	.001	.001	.001	.001	−.001	.002
% Insured	.004**	.001	.010	.010	−.008	.008	−.004	.006	−.006	.006	−.006	.006
Comm. Health Problems	.203***	.027	.154	.200	−.425**	.152	−.221	.118	.004	.125	−.124	.127
Total Population	−5.04E-08	.000	−9.750E-08	.000	−5.920E-08	.000	−2.560E-09	.000	5.790E-08	.000	−2.020E-08	.000
% Young Males	.009	.005	−.052	.042	.001	.031	.002	.024	.023	.024	.005	.026
Police Per Capita	−4.45E-05	.000	−9.400E-05	.001	−.001	.001	−.002*	.001	−.002**	.001	.001	.001
Prisoners	−1.31E-06**	.000	2.720E-07	.000	3.950E-06	.000	7.510E-06***	.000	3.470E-06	.000	1.100E-06	.000
Spatial lag	.682***	.061	.332***	.072	.222**	.075	.758***	.050	.694***	.057	.380***	.083
Year 2005	.075***	.018	.442**	.140	−.026	.098	−.217**	.077	−.084	.083	.159	.086
Year 2010	.120***	.029	.533*	.228	−.186	.161	−.381**	.126	−.071	.134	−.151	.139
Year 2015	.091**	.034	.983***	.256	.002	.180	−.020	.140	.203	.144	.037	.153
Intercept	.223	.197	1.129	1.249	.370	.911	−1.665*	.710	−.274	.737	−.032	.778

Notes: $N = 176$. ".000" values indicate small numeric values that extend beyond the thousands decimal place.

*$p < .05$; **$p < .01$; ***$p < .001$.

TABLE 4 Multivariate Models of Homicide and Overdose Deaths by Substance Type for Small Destination Counties, 2000–2015

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Homicide		Overdose		Natural Opioids		Synthetic Opioids		Heroin		Cocaine	
Variables	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*
% Foreign born	−.004*	.002	−.061	.031	−.005	.025	−.028	.021	−.006	.016	−.017	.018
Disadvantage	.025***	.004	.017	.060	−.058	.049	.043	.041	.054	.031	.042	.034
Manufacturing Jobs	.000	.000	.000	.001	.000	.001	.000	.001	.000	.000	.000	.000
Social Asst. Agencies	−.005	.000	−.075	.000	−.046	.000	.026	.000	−.024	.000	−.049*	.000
Residential Mobility	−.000	.000	.008	.006	.001	.005	.007	.004	.001	.003	.002	.003
Race/Eth Heterogeneity	.392***	.062	1.263	1.014	−.073	.822	.342	.685	.193	.513	.105	.572
Opioid Rx Rate	.000	.000	.000	.001	.000	.001	.000	.000	.000	.000	.000	.000
% Insured	.001*	.000	−.003	.006	.001	.005	.006	.004	.001	.003	.006	.004
Comm. Health Problems	.086***	.008	−.077	.134	−.013	.109	−.189	.090	−.062	.069	−.172*	.076
Total Population	2.68E-07'	.000	−1.70E-06	.000	−2.50E-07	.000	−5.73E-07	.000	2.22E-07	.000	−1.09E-07	.000
% Young Males	.007***	.002	−.025	.026	−.019	.021	.000	.018	.017	.013	.005	.015
Police Per Capita	2.25E-05	.000	1.57E-04	.000	−1.01E-04	.000	1.96E-05	.000	2.73E-04	.000	−1.16E-04	.000
Prisoners	−4.75E-07	.000	3.03E-06	.000	−1.76E-06	.000	1.42E-06	.000	−1.33E-06	.000	−1.37E-08	.000
Spatial lag	.873***	.019	.432***	.033	.633***	.032	.748***	.032	.865***	.024	.585***	.040
Year 2005	.013**	.004	.584***	.073	.196**	.057	.041	.046	−.029	.034	.091*	.038
Year 2010	.037***	.007	.841***	.124	.415***	.100	.034	.079	−.016	.059	−.057	.066
Year 2015	.031***	.007	1.214***	.130	.517***	.104	.300***	.082	.227***	.061	.062	.067
Intercept	.051	.055	1.459*	.689	.597	.557	−.495	.463	−.088	.347	−.179	.388

Notes: $N = 1{,}016$. ".000" values indicate small numeric values that extend beyond the thousands decimal place.

*$p < .05$; **$p < .01$; ***$p < .001$.

TABLE 5 Multivariate Models of Homicide and Overdose Deaths by Substance Type for New Destination Counties, 2000–2015

	Model 1 Homicide		Model 2 Overdose		Model 3 Natural Opioids		Model 4 Synthetic Opioids		Model 5 Heroin		Model 6 Cocaine	
Variables	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*
% Foreign born	−.003**	.001	−.035*	.013	−.020	.012	.000	.010	−.005	.007	−.011	.008
Disadvantage	.032***	.004	.134**	.045	.023	.039	.018	.032	.008	.025	−.000	.028
Manufacturing Jobs	.000	.000	−.001	.001	−.002***	.001	−.001*	.000	−.001**	.000	−.001*	.000
Social Asst. Agencies	−.001	.000	−.054	.000	−.116**	.000	−.005	.000	.042	.000	.229	.000
Residential Mobility	.000	.000	.007	.004	.002	.003	−.000	.003	.002	.002	.919	.002
Race/Eth Heterogeneity	.585***	.048	−.266	.567	.324	.487	−.091	.403	.225	.311	−.119	.348
Opioid Rx Rate	.000	.000	.000	.001	.000	.000	.000	.000	.000	.000	.000	.000
% Insured	.002***	.000	.006	.004	.009*	.004	.003	.003	.002	.002	.001	.003
Comm. Health Problems	.095***	.007	.132	.083	.231**	.072	−.116	.060	−.114*	.047	−.197***	.052
Total Population	−6.440E-08	.000	−4.240E-07	.000	−2.630E-07	.000	1.220E-07	.000	5.410E-07	.000	.017	.000
% Young Males	.006***	.001	.007	.016	.011	.014	.005	.012	−.002	.009	.008	.010
Police Per Capita	−3.300E-05	.000	3.430E-06	.000	2.633E-04	.000	−3.176E-04	.000	−3.233E-04*	.000	−.019	.000
Prisoners	−7.700E-07**	.000	1.520E-06	.000	1.710E-06	.000	5.140E-07	.000	−2.000E-06	.000	1.200E-06	.000
Spatial lag	.758***	.016	.477***	.023	.559***	.024	.796***	.022	.917***	.019	.505***	.030
Year 2005	.019***	.004	.602***	.051	.239***	.041	.002	.033	−.041	.025	.125***	.029
Year 2010	.036***	.006	.850***	.084	.536***	.070	−.006	.055	−.053	.043	−.096*	.047
Year 2015	.024***	.007	1.221***	.085	.653***	.071	.304***	.056	.196***	.042	.059	.047
Intercept	.166**	.050	.672	.474	−.421	.407	−.024	.336	−.061	.260	.209	.291

Notes: $N = 1{,}669$. ".000" values indicate small numeric values that extend beyond the thousands decimal place.

*$p < .05$; **$p < .01$; ***$p < .001$.

Table 3 provides the results for traditional destination counties. This analysis shows no significant relationships between percent foreign born and homicide or any of the overdose death rates examined. Thus, there is no indication that homicide or overdose death rates increased (or decreased) significantly in response to rising immigrant populations in traditional destinations. However, these null findings may in part reflect the small number of traditional destinations ($n = 176$) and limited statistical power for this analysis.

The results of the analysis for small destination counties are shown in table 4. Here we find that immigration was associated with significant reductions in county rates of homicide from 2000 to 2015. However, immigration had no discernable effects on overdose rates (overall or by substance type) for small destination counties. Thus, changes in the county percent foreign born largely had null effects on overdose deaths, net of other factors.

The results for new destination counties are shown in table 5. Here immigration was again associated with lower homicide death rates ($b = -.003$, $p < .01$). In addition, immigration was linked to significantly lower total overdose rates ($b = -.035$, $p < .05$). For natural and synthetic opioids, heroin, and cocaine, the relationship between immigration and overdose deaths was negative in direction but did not reach statistical significance. Taken together, the findings from table 5 indicate that immigration had either neutral effects or, when significant, was linked to lower levels of overdose (and homicide) deaths in new destination counties. Notably, these findings contrast with prior research indicating that immigration may have less protective or even criminogenic effects in new destinations. Instead, it appears that immigration may be *more* protective against overdose deaths in new destination communities, a point that we return to in the Discussion section.

With respect to the control variables, their relationships with homicide and overdose death rates generally mirror those observed in the earlier analysis of all counties (table 2). Notably, the sample sizes (and statistical power) are smaller in these models after disaggregating by destination type, which likely explains why there were fewer significant effects among predictors. However, the general relationships between control variables and county mortality rates in tables 3 through 5 remain consistent with those seen in table 2.

4.3 | Supplemental Analyses

To exhaust the data and assess the robustness of our findings, we conducted supplemental analyses using several alternative modeling specifications and variables. We replicated all models using natural log-transformed dependent variables (rather than square root transformation) as an alternative adjustment for skewness in mortality measures. Notably, we relied on square root transformations because substantial skewness remained in our dependent variables when using natural log transformations. However, the trade-off is that effect sizes in square-root-transformed models are more difficult to interpret than those obtained with log transformations. Thus, for the reader's reference, table 6a displays the immigration effects on homicide and overdose death rates obtained from our supplemental analysis using natural log transformations (all control variables included but not shown).[13]

[13] We also replicated all models using negative binomial fixed-effects models. Negative binomial models and log-transformed models were less stable as variables were added or removed. However, effects of our immigration measure remained null or significantly negative in nearly all models. Some significant effects (and nonsignificant effects) shifted by substance type in the supplemental models. Regardless, the results showed no indication that the percent foreign born was related to increased overdose death rates.

TABLE 6a Supplemental Analysis: Immigration Effects on Natural Log Homicide and Overdose Death Rates, 2000–2015

	Model 1 Homicide		Model 2 Overdose		Model 3 Natural Opioids		Model 4 Synthetic Opioids		Model 5 Heroin		Model 6 Cocaine	
Natural Logged DVs	*b*	*EXP(b)*	*b*	*EXP(b)*	*b*	*EXP(b)*	*b*	*EXP(b)*	*b*	*EXP(b)*	*b*	*EXP(b)*
% Foreign born												
All Counties	−.002**	.998	−.046***	.955	−.033*	.968	−.021	.979	−.027**	.973	−.032*	.968
Traditional Destinations	.003	1.003	.011	1.011	.000	1.000	−.007	.993	−.028	.972	−.017	.983
Small Destinations	−.003*	.997	−.080*	.924	−.029	.972	−.069	.933	−.021	.979	−.030	.970
New Destinations	−.002**	.998	−.027	.973	−.039	.962	−.017	.984	−.015	.985	−.030	.970

All Counties $N = 2{,}861$. Traditional Destinations $N = 176$. Small Destinations $N = 1{,}016$. New Destinations $N = 1{,}669$.

*$p < .05$; **$p < .01$; ***$p < .001$.

TABLE 6b Supplemental Analysis: Hispanic Foreign-Born Effects on Square-Root-Transformed Homicide and Overdose Death Rates, 2000–2015

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Homicide		Overdose		Natural Opioids		Synthetic Opioids		Heroin		Cocaine	
Square Root Transformed DVs	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*
% Hispanic Foreign born												
All Counties	−.002*	.001	−.019	.011	−.005	.009	−.005	.007	−.004	.006	−.003	.006
Traditional Destinations	.001	.002	−.003	.014	−.004	.010	−.013	.008	.006	.008	−.002	.009
Small Destinations	−.002	.002	−.014	.037	.019	.030	−.018	.025	−.010	.018	−.034	.021
New Destinations	−.004**	.001	−.054**	.016	−.034*	.014	−.014	.011	−.017	.009	−.011	.010

All Counties $N = 2{,}861$. Traditional Destinations $N = 176$. Small Destinations $N = 1{,}016$. New Destinations $N = 1{,}669$.

*$p < .05$; **$p < .01$; ***$p < .001$.

The results shown in table 6a using natural log transformation mirror those shown for our main analysis, indicating that increases in immigration are associated with significantly lower homicide and overdose death rates overall and across substance type. In terms of effect sizes, the exponentiated coefficients indicate that county overdose rates were reduced by 4.5 percent for every one-percentage-point increase in the foreign-born population. Similarly, a one-percentage-point increase in the foreign-born population was associated with a 3.0 percent decrease in death rates from natural opioids, heroin, and cocaine. Taken together, these supplemental models show sizable protective effects of immigration and suggest that a 10-percentage-point increase in a county's foreign-born population could contribute to as much as 40.0 to 50.0 percent lower overdose death rates overall and for nearly each of the substances examined here, net of controls.

As discussed, rhetoric surrounding immigration and drug crime has most often targeted Latinx (and typically Mexican) immigrants. Thus, we replicated our original models using "% Hispanic Foreign-Born Population" in each county as an alternative measure of Hispanic immigration. Results of this analysis are shown in table 6b (all control variables included but not shown). Findings using Hispanic immigration measures were similar to those obtained in the main analysis using "% Foreign-Born Population" and again show that (Hispanic) immigration had either neutral effects or, when significant, was linked to lower overdose and homicide death rates. Moreover, the protective effects of Hispanic immigration were particularly strong in new destination counties.

We also estimated models using alternative measures of county health problems (different versions of the health problem index) and additional controls for county social, economic, and health context. However, these variables did not have significant effects on homicide or overdose rates, and the immigration–overdose relationships did not vary when they were added.[14] As described in the Method section, we also replicated the analysis of destination types using several alternative categorizations for immigrant destinations (Lichter & Johnson, 2009; Ramey, 2013; Singer, 2004). The results obtained using alternative destination type categorizations were substantively similar to those shown here.

Last, prior research has noted the potential for selection effects and endogeneity in immigration–crime relationships. It is possible that immigrants select into safer counties with lower homicide and overdose rates rather than immigration driving down drug and homicide problems in local communities. To address this possibility, we followed the practices of prior research and replicated the models for all counties using an instrumental variable (IV) analysis (Light & Miller, 2018; Lyons et al., 2013; MacDonald et al., 2013). Specifically, we used the 1990 immigrant population as an instrument to remove endogeneity from measures of immigrant concentration, homicide, and overdose deaths covering 2000 through 2015. Results obtained using IV and two-stage least-Squares models generally showed null effects of immigration on overdose and homicide, again suggesting that immigration has not elevated drug overdose or homicide rates (results available upon request).[15]

[14] We examined the effects of several additional variables, including county employment rates in construction and agriculture, presence of hospitals and primary care physicians, and population density. The alternative variables noted here did not have consistent effects on mortality rates and did not impact the immigration–mortality relationships (results available upon request).

[15] Prior levels of immigration (i.e., 1990 in this case) are commonly used as IVs based on the assumption that they are associated with later levels of immigration but not with residuals in the multivariate models (Lyons et al., 2013). Preliminary analyses supported this assumption for the current models. Notably, we were unable to conduct multiple imputation for missingness in the IV analysis, which produced smaller sample sizes and may account for some differences in effects compared with the main models.

5 | DISCUSSION

5.1 | More Immigrants, Less Death

The last decades of the twentieth century and the early decades of the twenty-first century have been marked by near-record levels of immigration to the United States, ushering in a second "great migration." During this same period, the United States also experienced an unprecedented epidemic of drug overdose deaths that has led to the loss of nearly 1 million American lives and has accounted for more than five times the number of deaths from homicide occurring each year (CDC, 2020). Political rhetoric and public perception have often linked immigration with drugs, suggesting that rising tides of immigration have fueled drug use, crime, and related social problems across U.S. communities (Greenberg & Qiu, 2016; Jacobson, 2010). By contrast, a large body of literature has revealed that immigration is not associated with violence and, if anything, may insulate communities from crime. However, this line of research has given little attention to drug use and the overdose crisis. This omission is noteworthy and surprising considering that both immigration and drug overdose trends rose rapidly during the early twenty-first century that, in turn, fueled political claims and public fears about immigration as a driver of drug problems in U.S. communities. As reviewed, these claims have gained considerable traction as the overdose crisis has escalated, leading many to see immigration and drug problems as synonymous. Among conservative media and political outlets, immigration has not simply been portrayed as one of many possible influences on overdose. Instead, it has been cast as the leading actor in the drug overdose epidemic and has led to renewed calls to build border walls and restrict immigration to combat the drug crisis that has plagued U.S. communities (Arthur, 2022; Grassley, 2021; Portman & Katko, 2021; Rappeport, 2015). As such, the relationship between immigration and overdose deaths demands empirical scrutiny.

The current study sought to address this gap in the research by providing one of the few existing longitudinal analyses examining relationships between community patterns of immigration and drug overdose death rates throughout the early decades of the twenty-first century, while examining specific substances and different immigrant settlement patterns. Our findings indicated that, in stark contrast to public fears and political rhetoric, growth in immigrant populations has not been linked to increased homicide or overdose death rates. Net of controls, immigration was generally associated with lower overdose death rates (or had null effects). This was also the case in new destinations, where growing immigrant populations were associated with declining total overdose rates. Taken together, these results show that across more than 50 different models (including the destination type analysis and supplemental models examining log-transformed mortality rates and Hispanic immigration), there was not one significant effect linking immigration to increased homicide or drug overdose deaths. Instead, our results indicate that, all things equal, places with growing immigrant populations saw far lower overdose and homicide mortality rates, net of other factors. Moreover, these protective effects are sizable based on supplemental (log-transformed) models predicting that a 10-percentage point increase in immigration may be associated with nearly 50 percent lower county overdose rates.

5.2 | Implications

These findings hold several key implications for theory and research related to immigration, crime, communities, and public health. First, the results described here provide support for

"immigrant revitalization" perspectives, which suggest that immigration insulates communities from crime and social problems. Although prior research has consistently supported this perspective in analyses of crime and violence, research has produced less evidence examining whether these revitalizing effects also exist for community drug problems. The current findings provide clear support for this theory and suggest that immigrant revitalization effects are not exclusive to violence and extend to macro-level patterns of drug overdose rates. Theoretically, this is important because it adds to the growing evidence indicating that immigrant revitalization has general effects that apply to multiple forms of crime, health, and related social problems. Criminologists and criminological theories have long sought to identify "general" explanations and invariance in sources of crime, death, and community social problems (Gottfredson & Hirschi, 1990; Shaw & McKay, 1942; Sutherland, 1927). Social disorganization, learning, and control theories (among others) have highlighted their broad applicability and theoretical power by showing that they could account for a wide variety of crime and social problems. The current findings suggest that immigrant revitalization may have such general effects. Specifically, these findings, combined with a growing body of public health research (Canino et al., 2008; Carr et al., 2012; Vega et al., 2011; Vaughn et al., 2014), suggest that the insulating effects of immigration are not limited to crime and may instead have protective effects that apply to broad sets of community patterns of crime, health, and mortality.

Second, our findings offer important implications about differences in immigration effects on crime and overdose deaths across destination types. As noted, research has at times found that small and new immigrant destinations do not experience the "revitalizing" or protective effects of immigration often observed in traditional destinations, with some studies even showing that immigration may be linked to greater crime in new destinations (Harris & Feldmeyer, 2013; Ramey, 2013; Xie & Baumer, 2018; see review in Ousey & Kubrin, 2018). However, our findings show the opposite. When we disaggregated our analysis by destination type, immigration had protective effects on homicide and overdose mortality in new destinations (and small destinations for homicide). This finding has important implications for research examining immigration and crime across destination types and suggests that new destinations may not be as vulnerable to crime-generating influences as previously thought. At least for fatal drug use, new destinations appear to benefit from immigration and may be similarly or even more protected from overdose deaths than places with more established histories of migration.

Third, these findings also have important practical and political implications for efforts aimed at combating the overdose crisis. Although the root sources of the overdose epidemic are complex and multifaceted, from a pragmatic standpoint, our findings indicate that efforts to reign in the overdose crisis and reduce fatal drug use should focus on factors other than immigration control. The results presented here clearly suggest that reducing immigration flows is unlikely to "make a dent," so to speak, in decreasing overdose deaths across U.S. locales. Likewise, from a political standpoint, our findings suggest that strategies aimed at combating overdose problems that center on restricting migration, building walls, and creating barriers to immigration into U.S. communities may be misguided. Given our consistent finding that immigration has either neutral or protective effects on overdose mortality, the overdose problem is unlikely to be solved (and could even be worsened) by allocating time, attention, and resources to combating immigration.

5.3 | Future research

Additional research is needed to test some of the assumptions described above and address caveats of the current study to provide a more thorough understanding of immigration effects

on community patterns of drug use. Notably, the current study does not directly examine immigration enforcement practices or policies. Thus, we avoid making direct claims about the ways in which immigration restrictions and policies may have influenced the overdose crisis. To explore these relationships, future research is needed that explicitly analyzes how overdose patterns have changed amidst shifts in immigration policies, including changes in border enforcement or the establishment of sanctuary cities. Immigration to the United States is also highly diverse in terms of country of origin, race/ethnicity, socioeconomic background, and the migration status of new arrivals (documentation status, refugee groups, etc.). As such, future research should extend these analyses to examine immigration–overdose relationships across different migrant groups, race/ethnicities, and backgrounds, but especially for those groups that have been the frequent targets of rhetoric linking immigration and drugs (e.g., undocumented Mexican migrants).

Although the MCOD mortality data provide some of the best available information on aggregate-level drug problems with national coverage over time, they also have several limitations. For example, these data only capture fatal drug use and cannot measure nonfatal patterns of drug use. Thus, it is possible that places with immigrant concentrations experience high levels of nonlethal overdose or drug use. The MCOD mortality data also do not allow analysis of study units at micro-geographic levels, such as census tracts, block groups, or neighborhoods. The county-level analysis conducted here has several advantages previously described, but additional research exploring the relationships between immigration and drug use at more local levels is needed. Notably, such analyses are also important for exploring the mechanisms that may be at work in these relationships—for example, examining the ways that immigration shapes social control, social capital, and attachment to protective institutions that may insulate communities from drug abuse, crime, and related social problems. Likewise, such studies (especially over time) would also be useful for examining possible reciprocal effects in immigration and overdose relationships. Additional research on immigration and overdose is needed that incorporates community measures of (both legal and illegal) drug manufacturing, distribution, and sales. As noted, there is also a need to further explore the different ways in which immigration is associated with overdose deaths across destination type. In contrast to prior studies, our findings suggest that immigration has particularly salient protective effects on drug mortality in new destination counties. Thus, we encourage researchers to explore the mechanisms that may be driving these effects.

Last, research is needed that examines the effects of the COVID-19 pandemic on homicide, overdose, and their relationships with immigration. Both homicide and overdose mortality were largely overshadowed by the onset of the COVID-19 pandemic. At the same time, research indicates that overdose deaths (and homicide to a lesser degree) have dramatically worsened throughout the pandemic (CDC, 2020; Rosenfeld et al., 2021). Notably, there is no obvious theoretical reason to expect that the relationships between immigration and overdose mortality observed here would have changed amidst the pandemic. However, further examination of these relationships that also accounts for the broader impact of the pandemic is warranted.

Although there is clearly a need for much more research, the current study provides several important advances in research on immigration and drug use. Our findings directly contradict the pervasive fears and political rhetoric suggesting that immigration has fueled drug problems across U.S. communities. We see no evidence linking immigration to rising overdose death rates, and instead we find that immigration has most often been associated with lower levels of overdose and homicide mortality. Thus, it appears that, if anything, immigration is more likely to have been part of the solution than the source of the overdose crisis of the early twenty-first century.

ORCID

Ben Feldmeyer https://orcid.org/0000-0001-6703-1045

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AUTHOR BIOGRAPHIES

Ben Feldmeyer is an associate professor in the School of Criminal Justice at the University of Cincinnati. His research focuses on macro-level patterns of crime and punishment across demographic groups (race/ethnicity, gender, age) and community contexts.

Diana Sun is an assistant professor in the School of Criminology and Criminal Justice at Florida Atlantic University. Her research interests include the effects of incarceration on white-collar offenders, macro-level patterns of crime, and issues related to race/ethnicity.

Casey T. Harris is an associate professor in the Department of Sociology and Criminology at the University of Arkansas. His research focuses on stratification and crime, community contexts, health, and immigration.

Francis T. Cullen is a distinguished research professor and senior research fellow in the School of Criminal Justice at the University of Cincinnati. His research interests include correctional policy, social support theory, and the criminology of Donald Trump.

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CRIMINOLOGY

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REASSESSING THE BREADTH OF THE PROTECTIVE BENEFIT OF IMMIGRANT NEIGHBORHOODS: A MULTILEVEL ANALYSIS OF VIOLENCE RISK BY RACE, ETHNICITY, AND LABOR MARKET STRATIFICATION*

MIN XIE [1] and ERIC P. BAUMER[2]

[1]Department of Criminology and Criminal Justice, University of Maryland—College Park
[2]Department of Sociology and Criminology, Pennsylvania State University

KEYWORDS: immigration, victimization, race, Hispanics or Latinos, National Crime Victimization Survey (NCVS)

Researchers in the United States have increasingly recognized that immigration reduces crime, but it remains unresolved whether this applies to people of different racial–ethnic and economic backgrounds. By using the 2008–2012 area-identified National Crime Victimization Survey (NCVS), we evaluate the effect of neighborhood immigrant concentration on individual violence risk across race/ethnicity and labor market stratification factors in areas with different histories of immigration. The results of our analysis reveal three key patterns. First, we find a consistent protective role of immigrant concentration that is not weakened by low education, low income, unemployment, or labor market competition. Therefore, even economically disadvantaged people enjoy the crime-reduction benefit of immigration. Second, we find support for threshold models that predict a nonlinear, stronger protective role of immigrant concentration on violence at higher levels of immigrant concentration. The protective function of immigration also is higher in areas of longer histories of immigration. Third, compared with Blacks and Whites, Latinos receive a greater violence-reduction benefit of immigrant concentration possibly because they live in closer proximity with immigrants and share common sociocultural features. Nevertheless, immigrant concentration yields a diminishing return in reducing Latino victimization as immigrants approach a near-majority of neighborhood residents. The implications of these results are discussed.

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Direct correspondence to Min Xie, Department of Criminology and Criminal Justice, University of Maryland, College Park, MD 20742 (e-mail: mxie@umd.edu).

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The findings reported in contemporary research provide compelling evidence that communities with larger immigrant populations exhibit less overall crime, especially in political, social, and economic "contexts of reception" that are favorable to immigrant incorporation (Desmond and Kubrin, 2009; Lee, Martinez, and Rosenfeld, 2001; MacDonald, Hipp, and Gill, 2013; Ousey and Kubrin, 2009; Stowell and Martinez, 2009; Vélez, 2009). Indeed, as MacDonald and Sampson (2012: 9) noted, areas of immigrant concentration are "some of safest places around" (see also Sampson, 2008).

Despite mounting empirical evidence, some politicians and a large portion of the public remain skeptical of the crime-reducing benefits of immigrant concentration (see Beinart, 2017; Pew Research Center, 2015). This skepticism may reflect deep-seated stereotypes about immigrants that are reinforced through selective and sensational media coverage (Rumbaut, 2008). Moreover, skepticism may stem in part from doubts about the empirical evidence supporting the noted inverse association between immigration and crime. The existing research has been focused either on homicide, which represents a tiny proportion of the crimes committed in the United States, or on police-based measures of nonlethal crime and arrest rates, which may yield inaccurate results because of the differences in police notification and arrest patterns in areas with large immigrant populations.[1] These data limitations weaken the impact of research findings that refute the general notion that immigration is associated with elevated crime rates. An analysis addressing these issues is timely given the current national and local dialogue concerning proposed changes to American immigration policy.

In this article, we enrich the empirical basis of claims about the potentially protective benefit of immigration by circumventing the police-based data source with multilevel data from the National Crime Victimization Survey (NCVS) to assess the relationship between neighborhood immigrant concentration and nonlethal violence. The NCVS data improve our ability to measure crimes that are unknown to the police, and thus, we provide this study to complement research comprising police-based crime rates. Substantively, we focus on whether neighborhood immigrant concentration is associated with reductions in violent victimization risk for all individuals who reside in such communities or, rather, whether that protective benefit depends on one's racial and ethnic status or exposure to different labor market realities.

Our theoretical orientation is grounded in immigrant revitalization theory (Lee and Martinez, 2002; Martinez and Lee, 2000a), which implies that the crime-reduction benefit of communities with a larger proportion of immigrants should be enjoyed by all residents. Nevertheless, through an emerging "threshold" model of immigrant social revitalization (Browning, Dirlam, and Boettner, 2016) and the literature on economic revitalization related to immigration (Reed and Danziger, 2007; Shihadeh and Barranco, 2010a), evidence has been introduced that race, ethnicity, and labor market conditions may modify the protective effects of immigrant concentration (see also Painter-Davis, 2013). Furthermore,

1. As Zatz and Smith (2012) noted, areas with high levels of immigrant concentration may be characterized by low rates of police notification, which may yield police-based crime estimates in immigrant communities that are biased downward. Additionally, areas with many immigrants may be under- or overpoliced compared with other places (see also Davies and Fagan, 2012; Kirk et al., 2012). Thus, studies of immigration and crime with arrest rates used as an indicator of crime rates may yield results that do not accurately reflect the true relationship between immigration and crime.

researchers who have highlighted the importance of spatial stratification and "contexts of reception" have indicated that these patterns may differ across "immigrant destination types" (Harris and Feldmeyer, 2013; Lyons, Vélez, and Santoro, 2013; Marrow, 2011; Painter-Davis, 2016; Shihadeh and Barranco, 2013).

We use data from the 2008–2012 NCVS that are linked to a wide variety of community-level indicators to assess the effects of neighborhood immigrant concentration on violent victimization risk among non-Latino Whites, non-Latino Blacks, and Latinos. Paralleling the integrated theoretical framework that guides our study, we assess both linear and nonlinear functional forms of the relationship between neighborhood immigrant concentration and violence risk. Additionally, we evaluate whether this relationship is moderated, either by individual educational attainment, employment status, and income or by county-level indicators of "immigrant destination type" and labor market competition between immigrants and natives. We go beyond previous assessments by testing for theoretically informed threshold (nonlinear) models using nationally representative data, using survey-based victimization data rather than police-based data, and directly evaluating the potential for individual-level heterogeneity in the observed effects.

In the next section, we provide an overview of the theoretical framework that guides our study and situate our analysis within existing research on community immigrant concentration and crime. We then present the data used for the study and discuss how the data can complement and extend the evidence derived from other sources, such as police data and homicide records. After presenting the results, we discuss their implications for theories of immigration and crime and outline some promising future research directions.

THEORETICAL BACKGROUND

Immigration has been linked to community crime levels through a variety of mechanisms, including its impact on population composition (e.g., age structure, family structure, and criminal propensity), economic conditions, and levels of formal and informal social control (Kubrin and Ishizawa, 2012; Martinez and Lee, 2000b; Shihadeh and Barranco, 2010a). In classic theoretical discussions, the possible adverse impacts of immigrant concentration on crime through these community attributes were emphasized (Shaw and McKay, 1942). In contrast, in contemporary scholarship, how immigrant concentration in communities can positively impact these features under suitable conditions, ultimately revitalizing communities and reducing crime, has been emphasized (Feldmeyer, 2009; Kubrin and Ishizawa, 2012; Lee and Martinez, 2002). When viewed together, what has emerged over the past century is a framework in which the effect of community immigrant concentration on crime is highly contingent on several moderating factors.

By borrowing from broader discussions about the consequences of immigration for communities (e.g., see Carr, Lichter, and Kefalas, 2012; Iceland, 2009), scholars in the criminological literature have emphasized the conditioning role of different social, political, and cultural contexts reflected in traditional versus more recently emergent immigrant destinations (Lyons, Vélez, and Santoro, 2013; Painter-Davis, 2016). Nevertheless, scholars in the social science literature have also pointed to two other potentially important moderators of the consequences of immigration: "threshold" effects and labor market stratification (e.g., Barranco and Shihadeh, 2015; Browning, Dirlam, and Boettner, 2016). Urban sociologists have recently highlighted the relevance of a "threshold" of immigrant concentration (Browning, Dirlam, and Boettner, 2016) that may

enhance community social organization and foster positive outcomes for community residents. Additionally, scholars in economics and criminology have drawn attention to the degree to which labor market segmentation may stratify the potential benefits of a large concentration of immigrants (e.g., Barranco and Shihadeh, 2015; Borjas, Grogger, and Hanson, 2010; Shihadeh and Barranco, 2010a; Waters, Kasinitz, and Asad, 2014). We next elaborate on the implications of these arguments for potential racial and ethnic differences in the impact of neighborhood immigrant concentration on violence. We also discuss how the implied processes may unfold differently in traditional and newer immigrant destinations, emphasizing how "destination type" may be critical for shaping the experiences of immigrants and the consequences of immigration for different groups (Painter-Davis, 2016).

THRESHOLD EFFECTS OF IMMIGRANT CONCENTRATION AND SOCIAL REVITALIZATION

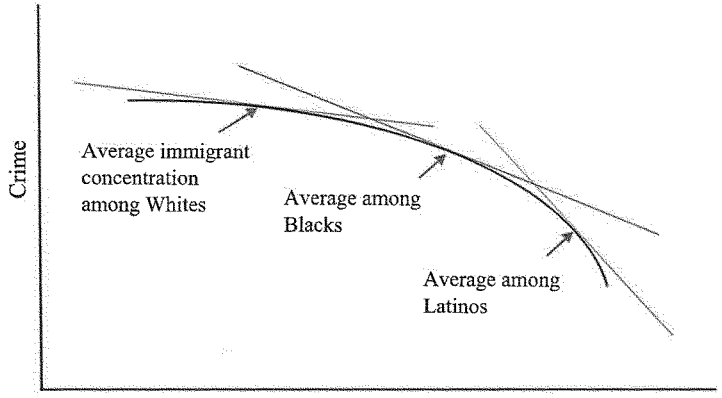
Contemporary scholars have argued that a large concentration of immigrants often enhances economic opportunities, increases family stability, and promotes more cohesive and functional social networks (Feldemeyer et al., 2017; Kubrin and Desmond, 2015; Portes and Rumbaut, 2006). In linking this "immigrant revitalization" perspective to theories of structural strain and social disorganization, many have suggested that immigrant concentration will be associated with lower crime rates (Kubrin and Ishizawa, 2012; Lee and Martinez, 2002; Painter-Davis, 2016; Sampson, 2008; Vélez, 2009). Consistent with this expectation, researchers on immigration and crime consistently have found a significant inverse association between indicators of immigrant concentration and crime (Ousey and Kubrin, 2018).

Nearly all scholars have interpreted and tested the immigrant revitalization perspective as a linear process, but Browning, Dirlam, and Boettner (2016) departed from this logic by suggesting the importance of what we label "threshold effects." Specifically, they argued that when immigrant populations surpass a threshold to reach high levels, the conditions that foster community-wide revitalization are likely to emerge: strong social networks and social support, greater social trust, and highly productive relationships within local institutions. In contrast, they suggested that when immigrants compose a small-to-modest share of the population, the anticipated positive community-wide impacts that have been associated with immigration may not accrue. As they put it, when "immigrant populations achieve sufficient presence within a neighborhood, increases in their numbers reinforce the benefits of immigrant concentration," but at lower levels, increases in immigrants may be "insufficient to yield social support, institutional, and public space benefits for neighborhood social organization" (Browning, Dirlam, and Boettner, 2016: 784).

A key empirical implication of this threshold model of immigrant revitalization is that the relationship between community immigrant concentration and crime may be *nonlinear*, characterized by a weak association at low levels of immigrant concentration that becomes negative and grows at an accelerating rate as the immigrant population share grows. This insight has significant relevance for assessing differences among Whites, Blacks, and Latinos in exposure to the beneficial features of immigrant neighborhood contexts (Harris and Feldmeyer, 2013).[2]

2. All references to Whites and Blacks in the article refer to non-Latinos.

Figure 1. Hypothetical Nonlinear Relationship Between Immigrant Concentration and Crime



Currently in the United States, individuals of different racial and ethnic groups vary greatly in the degree to which they share neighborhoods with immigrants. Although immigrant destinations have shifted in important ways over the past few decades, immigrants tend to be geographically concentrated in or near urban areas and near communities with more affordable housing and higher proportions of non-White populations (Chiswick and Miller, 2004; Howell and Timberlake, 2014; Logan, Zhang, and Alba, 2002). Because of this spatial pattern, Whites are less likely than Blacks, and Blacks are less likely than Latinos, to reside in neighborhoods with a large concentration of immigrants (Portes and Rumbaut, 2006). Moreover, even when Whites and Blacks reside in neighborhoods of high immigrant concentration, they tend to be more spatially segregated from the foreign born than are Latinos (the majority of whom are native born) as a result of differences in socioeconomic status, language, and social customs (Hall, 2013; Iceland, 2009). This is especially true for Whites (Waters and Pineau, 2015).

Integrating these residential patterns with the idea of a threshold model of immigrant revitalization illustrates the potential for racial and ethnic differences in the impact of neighborhood immigrant concentration on victimization risk. Consider figure 1, which illustrates in a hypothetical way the expected pattern, assuming a threshold model of immigrant revitalization. As the figure shows, within this perspective, the relationship between neighborhood immigrant concentration and crime will be weaker for Whites than for Blacks, who tend to live in neighborhoods with higher levels of immigrant concentration, and weaker for both Whites and Blacks than for Latinos, who are even more likely to reside in areas of larger immigrant concentration. We examine these possibilities by assessing both linear and nonlinear associations between neighborhood immigrant concentration and violence for Whites, Blacks, and Latinos, and by testing whether the observed relationships vary across the three groups.

IMMIGRANT CONCENTRATION, ECONOMIC REVITALIZATION, AND LABOR MARKET STRATIFICATION

Beyond its potential benefits for community social organization, immigrant concentration also has been tied to economic revitalization (Portes and Zhou, 1993; Vigdor, 2013). The overall economic impact of immigration is difficult to measure, but reviews by the National Academy of Sciences have concluded that immigration has been "integral to the nation's economic growth" (Blau and Mackie, 2016: 5; see also Smith and Edmonston, 1997). Among other benefits, larger immigrant populations have been linked to innovation, job creation, greater economic productivity, and increased housing wealth (Peri, 2012; Portes and Zhou, 1993; Vigdor, 2013; Waldinger, 1989). Additionally, there is compelling evidence of significant economic revitalization in recent years not only in traditional immigration destinations but also in many other areas that previously have not received many immigrants (Sampson, 2008).

In drawing from a variety of theoretical perspectives (e.g., classic economic theory, structural strain, and social disorganization), many scholars have suggested that economic revitalization represents another mechanism through which immigrant concentration may lower crime rates (see Kubrin and Ishizawa, 2012). Furthermore, in much of the literature, researchers have posited that economic growth associated with larger immigrant populations yields community-wide, crime-reduction benefits. Sampson (2008) described this idea in terms of "externalities" that stem from immigrant concentration or, in other words, community-wide reductions in crime that extend not only to immigrants but also to natives who live in such areas.

In contrast to the idea of community-wide externalities, some have suggested that the economic revitalization that often accompanies high immigrant concentration may not be realized equally by all groups within an area. Reed and Danziger (2007) noted that although large immigrant populations may positively complement natives in the labor force, they could also exert negative labor market pressures upon natives, especially in the low-skilled job sector (see also Raphael and Ronconi, 2007; Shihadeh and Barranco, 2010a).

The empirical evidence reveals that immigration has little discernible negative impact on the economic circumstances of Whites, but the evidence is mixed about the impact of immigration for non-White natives (Blau and Mackie, 2016). On the one hand, some scholars have pointed out that immigrants often complement, rather than substitute for, Black workers because immigrants tend to have different preferences and skills (e.g., native Black construction workers are more likely to get union jobs, whereas immigrant construction workers are more likely to obtain jobs through informal networks; see Bailey, 1987). Furthermore, some research findings have shown that immigration can boost Black wages and employment rates, especially when immigration contributes to the development of a larger public sector that disproportionately employs Blacks (Waters, Kasinitz, and Asad, 2014). On the other hand, some researchers have reported that a larger share of immigrants in an area sometimes suppresses employment prospects and wages slightly for low-educated Blacks (Borjas, Grogger, and Hanson, 2010; Reed and Danziger, 2007; Waldinger, 1997). Therefore, Black workers sometimes experience job competition or displacement in hotels, restaurants, meatpacking, manufacturing, and other low-skilled sectors in contexts of greater immigrant concentration (e.g., Fennelly, 2008; Hernández-León and Zúñiga, 2000; Kandel and Parrado, 2005; Waldinger, 1997). If racial groups

are excluded from the beneficial impacts of economic revitalization that accrue from immigrant concentration, or if their economic positioning is compromised, they may not experience the anticipated crime-protective benefits of living in communities with large immigrant populations (Shihadeh and Barranco, 2010a, 2010b).

Overall, the findings reported in the literature on immigrant concentration and economic revitalization lead us to anticipate that the crime protective benefits of immigrant concentration may not extend to Blacks, who are more likely than other ethnoracial groups to compete with immigrants for low-skilled jobs (Reed and Danziger, 2007; Waldinger, 1997). This suggests that the theoretically expected inverse association between immigrant concentration and crime may be weaker for Blacks. We assess this possibility by testing for between-group differences in the impact of immigrant concentration on violence risk.

The economic revitalization perspective also suggests two ways in which the impact of neighborhood immigrant concentration may vary *within groups* as a function of economic status and labor market competition. First, for members of all ethnoracial groups, the protective role of immigrant concentration may be weaker among those with limited education, low income, and weak labor market attachment (Borjas, Grogger, and Hanson, 2010). We assess the prediction by evaluating whether the effect of neighborhood immigrant concentration on violent victimization for each group is moderated by individual-level measures of educational attainment, household income, and employment status. Second, the protective role of neighborhood immigrant concentration may be weaker for ethnoracial group members who reside in contexts where immigrants compete more favorably with natives for jobs. Some evidence from homicide studies demonstrates that this is the case for both urban Blacks (Shihadeh and Barranco, 2010a) and rural Whites (Shihadeh and Barranco, 2010b). In these studies, homicide rates for both groups were elevated in labor markets with increased competition for low-skilled jobs. To date, this work has not been extended beyond aggregate studies of Black/White homicide rates to account simultaneously for the influence of market conditions as well as that of individual labor characteristics (such as education, income, and employment status). We accomplish this by using multilevel data from the NCVS to evaluate whether the impact of neighborhood immigrant concentration on violent victimization is conditioned by county-level indicators of labor market competition between immigrants and natives, while including the effects of individual factors.

CONDITIONAL ETHNORACIAL DIFFERENCES BY DESTINATION TYPE

Prior researchers have emphasized the potentially important moderating role of "contexts of reception," often operationalized in terms of whether immigration occurs in traditional or newer (emerging) destinations (Portes and Rumbaut, 2006). For much of the past century, American immigrant neighborhoods were concentrated in a small number of large urban areas within a few "gateway states" (California, New York, Texas, Illinois, Florida, and New Jersey). Beginning in the 1990s, however, traditional gateway states declined in importance, and some new destinations, particularly those in the South and Midwest, emerged as the fastest growing receiving areas for immigrants (Massey, 2008). This historical narrative of immigrant destinations has relevance for our study. In particular, study results suggest that immigrant destinations with shorter histories of immigrant settlement may lack the kind of social, economic, and cultural support that is available

in traditional destinations that may limit the prospect of community revitalization and weaken the protective role of immigrant concentration (e.g., Barranco, 2013; Harris and Feldmeyer, 2013; Ramey, 2013; Shihadeh and Winters, 2010).

More important for our study, the differences in social and economic organization between traditional and newer immigrant destinations have implications for ethnoracial differences in the impact of neighborhood immigrant concentration on violence (Painter-Davis, 2013). To understand these implications, consider again the threshold model of immigrant revitalization. The realities of this model are likely to unfold differently in traditional and newer immigrant destinations. Newer immigrant destinations are less likely than traditional destinations to have reached the population threshold some have suggested may be needed to revitalize social organization (Browning, Dirlam, and Boettner, 2016). Additionally, even if immigrants compose a sufficiently large segment of the population to revitalize new destinations, other social and cultural features of these areas may foster higher levels of immigrant segregation and limit community-wide revitalization to a greater extent than is the case in traditional destinations (Hall and Crowder, 2014; Lichter et al., 2010). Therefore, increases in immigrant concentration may be less strongly associated with crime reductions in newer destinations. Furthermore, and more pertinent to our study, because native Whites and Blacks will be less likely to reside in neighborhoods of heavy immigrant concentration in emerging areas, any crime-protective benefit they receive may be limited to traditional destinations.

Painter-Davis (2013, 2016) pointed out also that immigration may impact labor markets differently in established and new destination types. Specifically, he suggested that economic competition between Latino immigrants and natives in low-skilled job sectors is likely to be especially intense in newer immigrant destinations (see also Marrow, 2011; Barranco and Shihadeh, 2015). Thus, the predictions outlined earlier about labor stratification and ethnoracial differences in the impact of immigrant concentration on violence may be moderated by destination type. In particular, we expect Blacks to benefit less from immigrant concentration in newer than in traditional destinations because they are more likely to face competition from immigrants in labor markets in newer destinations. Additionally, we expect members of all ethnoracial groups who have limited education, low income, and weak labor market attachment to benefit less from immigrant concentration when they reside in newer destinations rather than in traditional ones, again because of heightened competition for low-skilled jobs in newer destinations. Finally, such labor market competition should be exaggerated in newer destinations in which immigrants have been more successful in gaining a foothold in local labor markets (e.g., where they occupy a larger share of low-skilled jobs).

EXTENDING PRIOR RESEARCH

The empirical predictions we outlined have received little attention in prior research. Only recently have studies been designed to compare the impact of immigrant concentration across multiple groups (e.g., Barranco and Shihadeh, 2015; Harris and Feldmeyer, 2013; Painter-Davis, 2016; Ramey, 2013; Shihadeh and Barranco, 2010b; Stowell, Martinez, and Cancino, 2012). The findings from this small body of research reveal potentially important racial and ethnic group differences in the impact of immigration on crime, but the specific patterns vary across studies (cf., among others, Martinez, Stowell, and Lee, 2010; Painter-Davis, 2016). It is unclear what to make of these

differences, but it could be because different units of analysis, different measures of crime, and samples that encompass different areas of the country were adopted in these studies. More generally, additional research is needed because much of this prior work has used measures of crime from the police that may reflect differences in both criminal behavior and police responses to crime, and the research uniformly has been limited to aggregate-level designs that do not permit a direct assessment of how individual attributes, including race and ethnicity, may moderate the impact of immigrant concentration. In our study, we advance existing knowledge on potential ethnoracial differences in the impact of immigrant concentration in three key ways.

First, we assess the association between neighborhood immigrant concentration and crime using nationally representative survey data on nonlethal personal violence experienced by Whites, Blacks, and Latinos. This strategy extends existing research by moving beyond homicide victimization and avoiding the use of police-based crime data, which may reflect differences in social control across communities instead of, or in addition to, differences in crime.

Second, guided by the threshold model of revitalization, we test for possible nonlinear relationships between neighborhood immigrant concentration and nonlethal violent victimization risk. In only one study that we are aware of was the possibility of threshold effects considered (Harris and Feldmeyer, 2013), and it was focused on explaining city-level variation in police-recorded arrest rates, which as noted might reflect very different underlying processes.

Third, we examine whether the impact of immigrant concentration on nonlethal violence is conditioned by the degree to which immigrants and natives compete for low-skilled jobs in the labor market and whether individual markers of stratification—education, income, and employment status—moderate the relationship between neighborhood immigrant concentration and exposure to violence for Whites, Blacks, and Latinos. Although some scholars have assessed the role of county-level or other aggregate-level indicators of economic competition between immigrants and natives (e.g., Shihadeh and Barranco, 2010a), none has considered the potential moderating role of these individual-level attributes of stratification.

As we describe next, we can extend prior research in these directions by employing a multilevel analysis in which we integrate neighborhood-level data on immigrant concentration and individual-level data on personal violence victimization risk for Whites, Blacks, and Latinos. The data also permit an assessment of the association between immigrant concentration and violence moderated by immigrant destination type.

DATA AND METHOD

DATA SOURCES

The study used data from several sources. The primary data were from the area-identified NCVS from 2008 through 2012. The NCVS is a large, nationally representative survey conducted by the U.S. Census Bureau for the Bureau of Justice Statistics (BJS). The survey collects data on nonlethal victimizations against persons 12 years of age or older from a sample of U.S. households using a stratified, multistage cluster design. The area-identified file used in this study is a restricted version of the NCVS that contains geographic identifiers for the census tracts and counties of respondents (the geocodes

are omitted from the public-use files to protect the confidentiality of respondents). We used the geocodes to link the NCVS victimization records with characteristics of the neighborhoods (census tracts) and counties in which respondents resided. The American Community Survey (ACS) and the decennial Census were the source of information on neighborhood immigrant concentration and other relevant neighborhood and county sociostructural conditions, as described later.

The final sample for our analysis included 569,611 person-interviews: 426,083 person-interviews for Whites; 61,602 person-interviews for Blacks; and 81,926 person-interviews for Latinos.[3] Individuals of multiple races were not included in the analysis because these individuals were coded as "other" race/ethnicity.

MEASURES

Dependent Variable

The dependent variable was a dichotomous measure of whether a respondent was a victim of violence (rape, sexual assault, robbery, aggravated assault, and simple assault) during a 6-month reference period before the interview. We focused on violence because it is of most urgent concern to America's crime problem (Zimring and Hawkins, 1997), and we examined violence committed within the respondents' neighborhoods (defined as having occurred within 5 miles of the respondents' home) because our interest was in assessing how personal victimization risk is affected by exposure to neighborhoods with different levels of immigrant concentration.[4] We used a binary outcome variable (0 = no, 1 = yes) rather than victimization counts because few respondents reported multiple victimizations within the 6-month recall period.

Independent Variables

Neighborhood Immigrant Concentration. We used multiple measures to assess the extent of *total* and *Latino* (vs. *non-Latino*) *immigrant concentration* in a neighborhood (census tract) to account for the diversity of the U.S. immigrant population. The results of the 2008–2012 American Community Survey (ACS) revealed that approximately 47 percent of immigrants in the United States are Latinos, whereas Asian immigrants, non-Latino White immigrants, and other non-Latino immigrants make up the other 25 percent, 19 percent, and 9 percent, respectively. Latino and non-Latino immigrants often share neighborhoods. The 2008–2012 ACS responses showed for example that today's immigrants reside in census tracts that are on average 37 percent Latino immigrants and 63 percent non-Latino immigrants. Similarly, in the NCVS sample, we found that many

3. The NCVS had approximately 11 percent of respondents with no census tract codes because they lived in newly constructed houses for which census tract codes were not assigned at the time of the survey. We used zip codes to substitute for these missing census tract codes, and in a sensitivity analysis, we found that the results were the same despite whether the missing cases were included.

4. The NCVS has several distance measures: "at/in/near" home, ≤1 mile, ≤5 miles, ≤50 miles, or >50 miles. When comparing these with census tracts (mean = 42 miles^2 and mode = 2 miles^2), a distance of somewhere between 1 and 5 miles would capture crimes in a neighborhood for most census tracts, although some tracts are much larger. We compared results using multiple cutoffs: 1 mile, 5 miles, and all distances (excluding incidents that occurred when the respondents resided at a different address or outside the United States). The analysis yielded similar conclusions, so we reported the results of a 5-mile radius only.

neighborhoods contain a mixture of Latino and non-Latino immigrants. Appendix A shows, for instance, that White and Black non-Latino NCVS respondents resided in neighborhoods whose immigrant population was on average 29 percent and 37 percent Latino immigrants, respectively. Latino NCVS respondents, in comparison, resided in neighborhoods with a larger share of Latino immigrants (62 percent on average).

Our key variable, *total immigrant concentration*, was the percentage of the neighborhood population that is foreign born. We focused on this measure because it describes the total concentration of immigrants (not just those who arrived recently or had a specific ethnicity), which captures more fully the potential social and economic capital associated with immigration, and enables us to estimate the total effect of neighborhood immigrant concentration on crime. To evaluate the nonlinearity of the immigration–crime relationship, as implied by the threshold model of immigrant revitalization, we included higher order (squared and cubed) values of this measure of immigrant concentration.

Furthermore, we replicated the analysis after substituting two alternative indicators of immigrant concentration that distinguish the ethnic background of immigrants (the percentage of neighborhood population that is *Latino immigrant*, and the percentage of neighborhood population that is *non-Latino immigrant*) to evaluate whether the results are comparable with those obtained using the indicator of *total* immigrant concentration. Because Latino and non-Latino immigrants often share neighborhoods, the two measures of immigrant concentration are highly correlated with the level of total immigrant concentration ($r = .80$ and .71, respectively). The effects of these alternative measures are discussed in the Results section of this article.

Individual-Level Stratification. To test whether the immigration–crime relationship is moderated by individual markers of stratification (education, employment status, and income), we included these individual traits in multiplicative models. Educational status was measured by years of education (an alternative set of binary measures was considered as a robustness check: less than high school, high school, some college, and college and more). Employment status was included as a dichotomous measure of whether the respondents were employed during the last 6 months.[5] The respondents' household income was measured by a scale ranging from 1 to 14.[6]

County Labor Market Competition. To assess whether the immigration–crime relationship is moderated by the degree to which immigrants and natives compete for low-skilled jobs, we used county-level labor market data from the U.S. Census Bureau. Counties are commonly used to depict the geographic boundaries of labor markets (Bellair, Roscigno, and McNulty, 2003). Counties are also defined for the entire country across all geographical categories, from rural to suburban to urban, going beyond the boundaries of other geographic units (e.g., cities and metropolitan areas) to represent both urban and rural

5. We coded a person as employed only if the job lasted "two consecutive weeks or more" (which was the majority of employed persons), although our results were not sensitive to this requirement. In another sensitivity analysis, we restricted our sample to respondents 18 to 65 years of age who were the key members of the labor force, and the analysis produced similar results.

6. To address the issue of missing income, the BJS used a hot deck imputation method to impute NCVS household income data (Berzofsky et al., 2014). Other variables had no or very little missing data (<1 percent).

America. Immigrants to the United States are increasingly residing outside urban centers, in small towns and rural areas (Massey, 2008). Thus, our use of counties ensures that the study is inclusive of immigrants both inside and outside of urban centers.

Because no single indicator exists in the United States to capture the full substantive meaning of immigrant–native labor competition, we used a set of complementary indicators to capture the most detailed county labor market information disaggregated by citizenship and race/ethnicity that is available. First, in adapting the method of Shihadeh and Barranco (2010a), we used the logarithm of the ratio of *the odds of noncitizens being in low-skill jobs* to *the odds of citizens being in low-skill jobs* to measure the extent of competition between citizens and noncitizens in low-skilled markets.[7] A value of 1 on this ratio indicates parity between citizens and noncitizens in the low-skilled job market. Values greater than 1 indicate that citizens experience greater competition from noncitizens in this sector, whereas values lower than 1 indicate less direct competition from noncitizens. Based on the theoretical discussion outlined earlier, we expect that the anticipated protective role of neighborhood immigrant concentration on crime may be dampened (i.e., smaller) in counties with higher values on the measure, where citizens face greater competition from noncitizens and the advantages of economic revitalization are lessened. We expect this to be true for Whites, Blacks, and Latinos because, in all of these groups, the proportion of native-born residents is greater than the proportion of foreign-born residents (Krogstad and Lopez, 2014).

Second, as race/ethnicity has been used (often incorrectly) to infer individuals' immigration status (e.g., Latinos are more likely than Whites and Blacks to be regarded as immigrants), we also used a comparable set of race/ethnicity-specific indicators—the logarithm of the ratio of *the odds of Latinos being in low-skill jobs* to *the odds of Blacks (or Whites, or non-Latinos of all races) being in low-skill jobs*—to measure each group's competition from Latinos in low-skilled markets (also see Shihadeh and Barranco, 2010a). As with the more global measure described earlier, these variables were expected to moderate the immigration–crime relationship: the higher the value (i.e., the greater the competition between Latinos and non-Latinos for low-skilled jobs), the less likely the protective benefits of immigrant concentration will be experienced.

Third, as the Census Bureau only released data on low-skilled workers for counties with at least 100,000 people, we remedied the data limitation by using related measures of group competition that compare county-level unemployment rates by race/ethnicity, which are available for all counties (see, e.g., McClain and Karnig, 1990). Specifically, we calculated the ratios of the Black (or White, or total non-Latino) unemployment rate to the Latino unemployment rate. Scores greater than 1 indicate that the non-Latino groups have higher unemployment rates than do Latinos, and thus, the non-Latino groups have a weaker position in the labor market than do Latinos (i.e., that Latinos are competing more favorably for jobs). As with the other measures, we expect the protective role of neighborhood immigrant concentration to be dampened in counties where the ratios of non-Latino to Latino unemployment rates are higher because, again,

7. The U.S. Census Bureau publishes county labor market statistics by industry and citizenship but not by more detailed information on immigration status (e.g., naturalized citizen, permanent resident, temporary resident, and undocumented). Although citizenship is not a perfect measure of immigration, it is the best measure available and a good indicator of the extent to which immigrants are incorporated into receiving communities (Bloemraad, Korteweg, and Yurdakul, 2008).

the non-Latino groups may observe or perceive greater competition from Latinos as a result of immigration.

Immigrant Destination Type. We used counties and the typology suggested by Suro and Singer (2002) to define destination type, which we expect to moderate the impact of neighborhood immigrant concentration.[8] Counties define not only labor markets but also immigrant destination areas because, in addition to employment opportunities, they offer housing, education, and many other resources that are important to residents (Hanson, 2005). *Traditional immigrant areas* were counties in which the 1980 immigrant population exceeded the then–national average of 6.2 percent. As shown in appendix B, these included major immigration gateways such as Los Angeles, Chicago, New York City, and Miami. *New immigrant areas* were counties where the immigrant base population was less than 6.2 percent in 1980, but the growth of the immigrant population from 1980 to 2010 was larger than the national average growth rate of 175 percent. *Small immigrant areas* were counties where both the base and the growth of the immigrant population were less than the national averages. Appendix B shows that new immigrant areas were disproportionately clustered in the Southeast and West, whereas small immigrant areas were disproportionately clustered in the North and parts of the South.[9]

Control Variables

The study included several other individual, neighborhood, county, and census regional factors emphasized in the literature as correlates of victimization risk (see the descriptive statistics in appendix A). Specifically, we controlled for respondents' age, gender, marital status, homeownership (owned or rented), and years of residence because of the documented relationships between these variables and violent victimization (e.g., Dugan and Apel, 2003; Lauritsen and White, 2001).

At the neighborhood level, the analysis controlled for contextual risks of victimization as suggested by crime opportunity and social disorganization theories, including neighborhood population density (logged population per square mile), age structure (percent ages 18–34), family disruption (percent divorced/separated), a binary indicator of central city neighborhood location, and measures of residential instability (i.e., percent households moved into unit less than 10 years ago, and percent vacant housing) and concentrated socioeconomic disadvantage (i.e., a composite index combining the poverty rate, unemployment rate, percent female-headed households with children, median household income adjusted for inflation [sign reversed], percent households with public assistance income, and percent population 25 years and older without a high school

8. Suro and Singer (2002) used the typology to study the concentration of Latinos, whereas we used it to study the concentration of immigrants. An advantage of this typology is that it includes a type of destination called "small" immigrant areas beyond "traditional" and "new" immigrant areas, which is a more comprehensive indicator of the areas' differences in immigrant concentration than a simpler binary typology (traditional vs. nontraditional). We show in our study that the distinction between new and small areas is important for explaining the victimization of Blacks and Latinos as reported in the results.

9. We also considered alternative typologies based on more recent growth of the counties' immigrant population (i.e., from 1990 to 2010 rather than from 1980 to 2010) to define destination types. This approach yielded results very similar to those reported in this article.

diploma). Additionally, because of our interest in race/ethnicity, we further used the percentage of Black and Latino population in the neighborhood to control for potential racial/ethnic differences in violence (Krivo and Peterson, 2000).

We also included several county-level control variables. The counties' general labor market conditions, which help to shape the economic well-being of communities and the residents' victimization risk, were measured by the counties' unemployment rate and the characteristics of county-level industries (i.e., the percentage of industries in economic-base sectors as opposed to local-serving sectors, and the percentage of industries that are traditionally regarded as key employers of Latinos including construction, manufacturing, business and other professional services, and wholesale and retail trade), which are factors that influence the economic opportunities of ethnoracial groups (see, e.g., Xie et al., 2017). Moreover, we accounted for the racial/ethnic differences in violence using the extent of residential segregation in the county, as measured by indices of dissimilarity for Blacks (or Latinos, or all non-Whites) from Whites.[10] Residential segregation, particularly the segregation of Blacks, has long been regarded as a cause of racial disparities in violence (Massey and Denton, 1993). In more recent years, researchers began to pay more attention to the effects of Latino segregation on violence (e.g., Feldmeyer, 2010; Xie, 2010), and hence, we included a measure of Latino segregation in the study. Because the NCVS does not distinguish nationalities among Latinos, we included the proportion of Latinos in the county that are Mexicans, Puerto Ricans, Salvadorans, and Cubans to account for differences among these groups in victimization risks caused by differences in education, occupation, cultural values, migration histories, legal statuses, and so on (DiPietro and Bursik, 2012; Stowell and Martinez, 2009). Finally, census regional categories (South, West, Midwest, and Northeast) were added to adjust for regional differences in violence.

DATA ANALYSIS

Because the dependent variable was a binary measure, we evaluated the impact of neighborhood immigrant concentration on victimization by race/ethnicity through a series of logistic regression models. Briefly, we first pooled all respondents together and tested the interactions between neighborhood immigrant concentration and race/ethnicity to evaluate the need for race/ethnicity-specific models. We then built models separately for Whites, Blacks, and Latinos, in which we evaluated the functional form (linear or nonlinear) of the immigration–crime relationship for each group and the moderating roles of individual stratification factors, labor market competition, and immigrant destination type.

To account for the stratified multistage cluster design of the NCVS, we incorporated NCVS design variables and survey weights into the estimation of the models to adjust for clustering of the data (Muthén and Satorra, 1995). Note that in preliminary data analysis, we explored multilevel modeling and found that when the data were disaggregated by

10. Residential segregation below the county level (i.e., immigrant–native segregation within neighborhoods) also may yield differences across racial/ethnic groups in the relationship between neighborhood immigrant concentration and crime. Unfortunately, we cannot directly examine the level of within-neighborhood segregation because the ACS does not provide information on the spatial distribution of immigrants and natives within neighborhoods, as that would require data on these two groups for very small geographic areas (such as block-level data) nation-wide.

race and ethnicity, there were up to 10 percent of census tracts with only one person-interview and 40 percent of census tracts with fewer than five person-interviews for the analysis. These low levels of clustering within census tracts, including the existence of singleton tracts (i.e., a census tract containing only one person-interview), can yield biased parameters for testing random slope variances at the tract level in multilevel analyses (Snijders and Bosker, 1993). To adjust for this, we estimated survey logit models that incorporated the effect of clustering and stratification as well as the effect of sampling weights when computing the model estimates.[11]

In the analysis, all neighborhood and county variables were lagged 1 year to capture the context of violent victimization for the year prior to the violence and thus keep the temporal ordering or measurement in accord with the causal ordering implicit in the hypotheses. We tested the models for multicollinearity. All models had variance inflation factors less than 4.

RESULTS

THE NEED FOR RACE/ETHNICITY-SPECIFIC MODELS

Whites, Blacks, and Latinos differ in their level of neighborhood immigrant concentration, as shown by the summary statistics in appendix A. Whites live in neighborhoods with an average percent foreign born of 9 percent, which is notably lower than the parallel figure for Blacks (13 percent) and Latinos (26 percent). Consistent with this pattern, Whites are less likely than Blacks and Latinos to reside in traditional immigrant areas (28 percent vs. 36 percent vs. 64 percent, respectively). Additionally, Latinos have less education but higher employment rates than Blacks and Whites. These differences in labor market status and ecological conditions are incorporated in our regression analysis of victimization risks, as we describe next.

In table 1 (pooling all groups), the first model provides baseline estimates by including the main measure of neighborhood immigrant concentration (percent foreign born) and the neighborhood, county, and regional control variables. This model parallels what has been estimated in most existing neighborhood-level studies of immigration and crime based on aggregate data (i.e., it does not account for individual differences in violence risk factors), with the caveat that in our analysis, we also incorporated violent incidents not reported to the police. This model shows a significant negative coefficient for *percent foreign born*, and in addition, the coefficient for *traditional immigrant area* is independently negative and statistically significant. Thus, in this baseline model, the NCVS data confirm a key observation of many previous studies: Neighborhood immigrant concentration seems to be protective against violent victimization when all groups are pooled together, even when the "dark figure" of unreported crime is added. The other neighborhood variables perform the way expected from theory: Population density, concentrated disadvantage, the proportion of young adults, indicators of family disruption, residential instability, and location within a central city are all positively related to victimization.

11. In the pooled analysis, however (where Whites, Blacks, and Latinos were pooled together), the data structure was less sparse, and therefore, we estimated multilevel models using a three-level structure (respondents nested in census tracts nested in counties). We found that the results from the multilevel analysis were very similar to the results reported in this article.

Table 1. Logistic Regression Models of Violent Victimization, All Races

	Model 1		Model 2		Model 3		Model 4	
Characteristics	***b***	**(SE)**	***b***	**(SE)**	***b***	**(SE)**	***b***	**(SE)**
Main variables								
% foreign born	−.009**	(.003)	−.009**	(.003)	−.006*	(.003)	.007	(.008)
× Black					−.004	(.004)	.004	(.015)
× Latino					−.007*	(.003)	−.030*	(.013)
× (% foreign born)							−.001*	(.000)
× (% foreign born) × (Black)							−.000	(.000)
× (% foreign born) × (Latino)							.001*	(.000)
Control variables								
Neighborhood-level variables								
Population density	.075**	(.023)	.083***	(.023)	.081**	(.023)	.078**	(.023)
SES disadvantage	.268***	(.038)	.167***	(.041)	.170***	(.041)	.183***	(.042)
% ages 18–34	.009**	(.003)	−.003	(.003)	−.003	(.003)	−.004	(.003)
% Black	−.001	(.001)	.000	(.002)	−.000	(.002)	.000	(.002)
% Latino	.001	(.002)	.002	(.002)	.002	(.002)	.002	(.002)
% divorced/separated	.025***	(.005)	.018***	(.005)	.018***	(.005)	.018***	(.005)
% moved into units <10 years	.007***	(.002)	.003	(.002)	.003	(.002)	.002	(.002)
% vacant housing	.005	(.004)	.010**	(.004)	.010**	(.004)	.010**	(.004)
Central city neighborhood	.164**	(.054)	.155**	(.054)	.151**	(.054)	.145**	(.054)
County labor & housing markets								
% industry in base sectors	.002	(.006)	.002	(.007)	.002	(.007)	.002	(.007)
% key Latino-employing industries	−.007	(.007)	−.008	(.007)	−.007	(.007)	−.007	(.007)
% unemployed	.004	(.013)	.001	(.013)	.001	(.013)	−.000	(.014)
% Puerto Ricans	−.001	(.003)	−.002	(.003)	−.002	(.003)	−.002	(.003)
% Salvadorans	.010	(.005)	.012*	(.005)	.012*	(.005)	.011*	(.005)
% Cubans	−.013**	(.004)	−.010*	(.004)	−.009*	(.004)	−.011*	(.004)
White unemployment / Latinos'	−.026	(.021)	−.024	(.023)	−.024	(.022)	−.022	(.022)
White segregation from non-Whites	−.003	(.228)	.190	(.239)	.187	(.239)	.201	(.239)
Traditional immigrant area	−.199*	(.082)	−.140	(.082)	−.154	(.082)	−.178*	(.084)
New immigrant area	−.082	(.075)	−.046	(.072)	−.053	(.072)	−.075	(.074)
Census region variables								
South	−.092	(.128)	−.128	(.138)	−.132	(.139)	−.120	(.140)
Midwest	.119	(.128)	.048	(.140)	.051	(.141)	.061	(.142)
West	.139	(.130)	.114	(.144)	.109	(.146)	.110	(.146)
Individual-level variables								
Black			−.345***	(.081)	−.300**	(.092)	−.347**	(.119)
Latino			−.369***	(.074)	−.250*	(.111)	−.075	(.149)
Age			−.024***	(.002)	−.024***	(.002)	−.024***	(.002)
Male			.114*	(.048)	.113*	(.048)	.114*	(.048)
Divorced			.921***	(.060)	.920***	(.060)	.919***	(.060)
Separated			1.222***	(.086)	1.221***	(.086)	1.221***	(.086)
Never married			.415***	(.055)	.413***	(.055)	.411***	(.055)
Education in years			−.010	(.008)	−.011	(.008)	−.011	(.008)
Employed			−.158***	(.042)	−.157***	(.042)	−.157***	(.042)
Household income			−.054***	(.006)	−.054***	(.006)	−.054***	(.006)
Homeowner			−.229***	(.053)	−.229***	(.053)	−.229***	(.053)
Years of residence			−.010**	(.003)	−.010**	(.003)	−.010**	(.003)
F test	35.930***		51.090***		48.780***		45.040***	
No. of person-interviews	569,611		569,611		569,611		569,611	

ABBREVIATIONS: No. = number; SE = standard error.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

In model 2, the relationship between percent foreign born and violent victimization remains the same when the respondents' individual-level risk factors are added. This finding extends aggregate-level neighborhood studies by showing a negative association of neighborhood immigrant concentration with crime, net of differences across neighborhoods in attributes of the respondents at risk, including their race/ethnicity, age, gender, marital status, education, employment status, household income, homeownership, and residential duration.

Our primary focus in this article, however, is on models 3 and 4, in which we tested the two-way interactions between respondents' race/ethnicity and neighborhood percent foreign born. In model 3, we observe *negative* coefficients for both interaction terms representing differences in the association between percent foreign born and violence for Blacks and Latinos, compared with Whites. The size of the coefficient is larger and statically significant for Latinos, whereas the coefficient is smaller and insignificant for Blacks. These results suggest that Latinos see a larger protective role of neighborhood immigrant concentration in crime than do Whites and Blacks, whereas for Blacks, although the gap between Blacks and Whites is in the expected direction, the difference is not large enough to be statistically significant. These results support the use of race/ethnicity-specific models, at least for Latinos.[12]

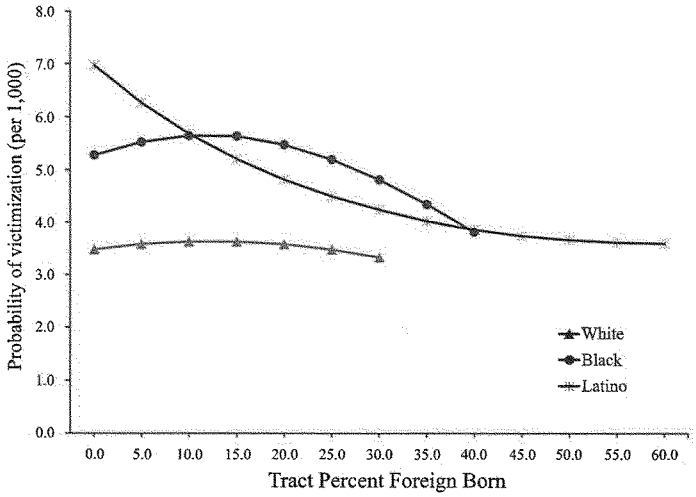
Furthermore, in model 4, we continue to observe race/ethnic differences when we allow the function of the immigration–crime relationship to be nonlinear using higher order terms of percent foreign born. Specifically, the results indicate a significant coefficient for *percent foreign born squared* for Whites (−.001, the reference group) and for Latinos (.001). In contrast, the difference between Blacks and Whites is not significant for either percent foreign born or for percent foreign born squared.[13] To help interpret the results, the findings are illustrated by figure 2, which shows the estimated probabilities of violent victimization for the average White (or Black, or Latino) at different levels of neighborhood immigrant concentration (percent foreign born).

The average White NCVS respondent resides in a neighborhood that is ~9 percent foreign born (see appendix A), but his or her neighborhood environment varies considerably, with most White NCVS respondents living in neighborhoods that fall between 0 percent and 30 percent foreign born. The form of the immigration–victimization relationship for Whites is best fit by a quadratic function. As illustrated in figure 2, in the lower range of neighborhood immigrant concentration (e.g., 0 to ~15 percent), the slope of the relationship is essentially flat for Whites, after which the slope becomes negative. This indicates that the risk of victimization for Whites is reduced when immigrant concentration increases from modest to high levels. For example, for an average White person, the difference in victimization risk between no immigrants and 30 percent immigrants (the maximum shown in figure 2) is a 5 percent reduction in the probability of victimization.

12. We found similar results when substituting % *Latino foreign born* and % *non-Latino foreign born* for the indicator of total immigrant concentration (% *foreign born*). The findings are not surprising given that the two measures are highly correlated with the measure of total immigrant concentration (r =.80 and .71, respectively). In light of the high degree of similarity in results, we only reported those for total immigrant concentration.

13. The cubed term of percent foreign born and its interactions with Blacks and Latinos provided no additional explanatory power.

Figure 2. Effects of Percent Foreign Born in a Census Tract on Violent Victimization, by Race/Ethnicity



NOTES: To show values in the usual range, *percent foreign born* was set to mean ±2 standard deviations for each group (i.e., 0% to 30% for Whites, 0% to 40% for Blacks, and 0% to 60% for Latinos, respectively). The values of other variables were set to race/ethnicity-specific means.

Blacks tend to reside in neighborhoods with a level of immigrant concentration higher than that of Whites—the average percent foreign born in neighborhoods occupied by Blacks is 13 percent—but this ranges from 0 to 40 percent for most Blacks. Like Whites, the form of the immigration–victimization relationship for Blacks is best fit by a quadratic function. In the lower range of immigrant concentration (e.g., 0 to about 15 percent), the slope of the relationship is positive, but the effect is weak and statistically indistinguishable from zero; in the higher range of immigrant concentration for Blacks (e.g., >15 percent), the slope becomes negative and is nontrivial in size. For an average Black person, the difference in victimization risk between no immigrants and 40 percent immigrants (the maximum shown in figure 2) is a 28 percent reduction in the probability of victimization. Notice that, for both Blacks and Whites, the patterns in figure 2 are mainly consistent with the hypothesized nonlinear relationship as depicted by figure 1.

We also observe a significant nonlinear relationship between neighborhood immigrant concentration and personal violent victimization for Latinos. Nevertheless, the nature of this relationship differs from the findings observed for Whites and Blacks, and it yields an unexpected pattern. Compared with Whites and Blacks, Latinos tend to live in neighborhoods that contain a much larger share of foreign-born persons. The average neighborhood for Latinos is 26 percent foreign born, but this ranges from 0 to 60 percent for most Latinos. Figure 2 shows that, for almost the first two thirds of this range (i.e., 0 to ~40 percent), the slope of the immigration–victimization relationship is significantly negative, and we observe a large, 44 percent, reduction in the probability of victimization

Table 2. Logistic Regression Models of Violent Victimization, by Race and Ethnicity

	Model 1		Model 2 Traditional areas		Model 3 New areas		Model 4 Small areas	
Characteristics	***b***	**(SE)**	***b***	**(SE)**	***b***	**(SE)**	***b***	**(SE)**
Panel A: Whites								
Main variables								
% foreign born	−.009*	(.004)	.023	(.016)	−.006	(.006)	−.009	(.014)
% foreign born squared			−.001*	(.000)	—	—	—	—
Control variables (see online table S.1)								
No. of person-interviews	426,083		116,117		167,291		142,675	
Panel B: Blacks								
Main variables								
% foreign born	−.022***	(.005)	−.022**	(.007)	−.028*	(.014)	.008	(.023)
% foreign born squared			—	—	—	—	—	—
Control variables (see online table S.2)								
No. of person-interviews	61,602		21,494		25,473		14,635	
Panel C: Latinos								
Main variables								
% foreign born	−.003	(.005)	−.036*	(.017)	−.020*	(.010)	−.039	(.040)
% foreign born squared			.001*	(.000)	.001*	(.000)	—	—
Control variables (see online table S.3)								
No. of person-interviews	81,926		51,604		23,095		7,227	

ABBREVIATIONS: No. = number; SE = standard error.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

as neighborhood immigrant concentration increases from 0 to 40 percent. For subsequent increases in the level of immigrant concentration beyond approximately 40 percent, however, the slope begins to ebb and gradually flattens. Thus, although neighborhood immigrant concentration is inversely associated with violent victimization risk for Latinos, this crime-reduction benefit peaks as the foreign-born population becomes the majority, after which additional immigrant concentration appears inconsequential for further crime reduction. This pattern is not observed for Blacks and Whites perhaps because it is rare for those groups to live in neighborhoods with such high levels of immigrant concentration, and, hence, the NCVS has very few of such observations in the sample. Before we discuss the significance of this observation and possible explanations behind it, we extend the models to incorporate information on immigrant settlement areas and labor market conditions.

RACIAL/ETHNIC PATTERNS BY IMMIGRANT DESTINATION TYPE

For all three racial/ethnic groups, the county's immigration history is found to be strongly related to the nature of the immigration–victimization relationship, as we report in table 2 (one panel for each group, with the full results reported in tables S.1 to S.3 of the online supporting information[14]). For all groups, we first estimated a baseline model

14. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2018.56.issue-2/issuetoc.

(model 1) that includes only aggregate-level variables (no individual-level variables), like we did in the pooled analysis. We then reported the estimated relationship between neighborhood immigrant concentration and victimization risk—both linear and nonlinear when applicable—in each destination type (traditional, new, and small) in models 2, 3, and 4, with all of the aggregate- and individual-level control variables also included. These models make it easier to show how the three types of destination areas compare with one another.[15]

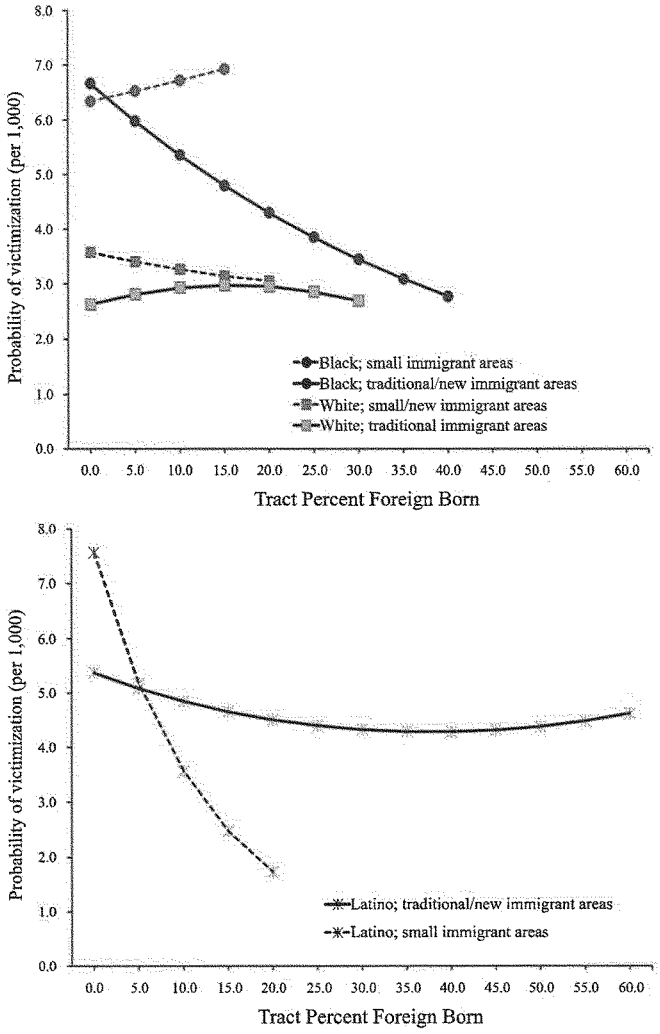
In table 2, all baseline models (model 1) show a negative coefficient for *percent foreign born*, but this relationship is contextualized by the county's immigration history, and it often takes a nonlinear form, as indicated by the variable *percent foreign born squared*. We illustrate the nature of these relationships in figure 3. The results show that the curvilinear relationship observed for Whites overall (see figure 2) is a more accurate description of Whites in traditional immigrant areas (see the solid line for Whites in figure 3), where the protective role of immigration starts to take effect and grows stronger at higher levels of immigrant concentration (e.g., >15 percent approximately). The effect is statistically insignificant in small and new immigrant areas, which show no significant difference from each other and thus are combined into one dotted line.

For Blacks, we also see divergent results across destination types. The protective role of immigrant concentration for Blacks is strong in both traditional and new immigrant areas (see the solid line, which combines these two types of areas because they were indistinguishable in the analysis). In contrast, in small immigrant areas, the relationship between neighborhood immigrant concentration and violence risk is positive, although the association is statistically insignificant.

For Latinos, several findings are noteworthy from figure 3. First, because more than 90 percent of Latinos live in traditional or new immigrant areas, the pattern observed in those areas is similar to the nation-wide pattern for Latinos (compare with figure 2). In fact, the correspondence between traditional and new immigrant destinations is so close (statistically indistinguishable) that we represent both areas with a single (solid) line in figure 3. In contrast, in small immigrant areas, represented by the dotted line, we notice that the risk of violence is particularly high when the neighborhoods have few or no immigrants, but the victimization risk drops rapidly as immigrant concentration grows. The slope, however, is statistically insignificant ($p = .34$), and we should be cautious in interpreting the results because, with less than 10 percent of Latinos living in small immigrant areas, the NCVS has a small number of cases available for analysis, which increases the standard error of the estimated slope. From the graphs, we see that for Latinos, the protective benefit of immigrant concentration may be more universal, with all three types of immigrant destinations showing signs of crime reduction related to immigrant concentration, particularly in the range from 0 to ~40 percent foreign born. Taken together, these findings show that Latinos are different from Whites and Blacks in that 1) they

15. In unreported supplementary analyses, we also used three-way interactions (% foreign born × % foreign born × immigrant destination type) to evaluate whether the three immigrant destination areas are significantly different in their nature of the immigration–crime relationship. The findings were similar to those reported in table 2. Essentially, we found that for all three race/ethnic groups, it is best to contrast differing patterns of the immigration–crime relationship across two (rather than three) types of immigrant destinations, although the types of areas contrasted vary across race/ethnic groups, as we describe next.

Figure 3. Effects of Percent Foreign Born in a Census Tract on Violent Victimization, by Race/Ethnicity and Immigrant Destination Type



NOTES: To show values in the usual range, *percent foreign born* was set to mean ±2 standard deviations for each racial/ethnic group by type of immigrant destinations. The values of other variables were set to race/ethnicity-specific means.

enjoy overall a greater benefit of immigrant concentration on reducing crime, 2) this effect occurs even at a low level of immigrant concentration, and 3) the effect gradually diminishes at a high level of immigrant concentration rarely experienced by Blacks or Whites (e.g., near 40 percent or even higher).

THE INCONSEQUENTIAL EFFECTS OF ECONOMIC STRATIFICATION

On top of models in table 2, we examined interactions of percent foreign born with the individual-level indicators of stratification and the county-level labor competition variables. We found no support for the idea that persons with weaker attachments to the labor market (or persons living in counties with higher labor market competition) were less likely to experience a violence-reduction benefit of living in neighborhoods with a higher concentration of immigrants. This was true no matter which labor-market variables were used (individual education, employment, or income, or county-level market conditions) and how we set up the models (e.g., whether we added each labor-market variable separately or in combination, whether the models were estimated within each destination type or with all areas combined, and whether we used percent foreign born, percent Latino or non-Latino foreign born, or higher order values of these immigrant concentration measures). As none of the hypothesized moderating effects were found, we do not include them in tabular form (these results are available by request).

Although in some research, a finding of "no effects" may be uninteresting, in our study, the finding of homogenous effects of immigrant concentration across widely differing individual stratification and county labor market conditions is an important observation. It stands in contrast to evidence from aggregate-level research that Blacks and Whites in some samples experience higher homicide rates when Latinos fare better than Blacks and Whites in low-skilled job markets (e.g., Shihadeh and Barranco, 2010a, 2010b). To evaluate further the implications of our analysis compared with the evidence that has been marshaled for homicide, we separately analyzed large urban counties and nonurban counties, and we analyzed low-skilled markets in the nine different industries separately, including manufacturing and other low-skilled jobs. The results were consistent across these specifications. We found no evidence that a person's education, employment status or income, or the various indicators of group-based labor market competition moderate the relationship between immigrant concentration and nonlethal violent victimization. This does not mean that group competition is absent, or that if we used homicide data, similar results would emerge. Nevertheless, at least for nonlethal violence, the data suggest that the impact of immigration is similar irrespective of one's economic and educational standing and the degree of competition between natives and immigrants in county labor markets.

DISCUSSION

Scholars in the United States have increasingly recognized that immigration plays an important role in reducing crime (MacDonald and Sampson, 2012). The results of our research confirm this finding through the use of victim survey data that include crimes not reported to the police. Beyond this, we advance knowledge about whether the relationship between neighborhood immigrant concentration and violent victimization risk is moderated by race/ethnicity, individual stratification factors, and exposure to labor market competition. Prior researchers had not fully addressed these issues mainly because of

a heavy reliance on aggregate-level, police-recorded data on crime that provide limited information on race, ethnicity, and markers of stratification. We build on existing theory and research and use data from the NCVS to address these gaps in the literature.

We find a significant protective role of immigrant concentration on victimization for Whites, Blacks, and Latinos that is not dampened by a person's low education level, low income, and unemployment or by measures of labor market competitions between citizens and noncitizens or between Latinos and non-Latinos. This evidence suggests to us that even those who are vulnerable to competition in labor markets enjoy the crime-reduction benefit of immigrant concentration. This finding corroborates the argument of immigrant revitalization that implies neighborhood-wide, crime-reduction "externalities" (Sampson, 2008). It should dispel, or at least diminish, the skepticism of many politicians and citizens about the crime protective benefits of immigration.

Against this backdrop of consistent protective benefits, it is important to recognize some important racial–ethnic differences: Latinos on average (i.e., when we pool the data of Latinos from across the country) enjoy more of the crime-protective benefits of immigration than do Whites and Blacks possibly because Latinos are more likely than Whites and Blacks to live in close proximity with immigrants and share with them common socioeconomic status, language, and social customs (Hall, 2013; Iceland, 2009; Lamphere, 1992). By testing for "threshold effects" (Browning, Dirlam, and Boettner, 2016), we show that the immigration–crime relationship is nonlinear for all three groups, characterized in general by a weak negative association at low levels of immigrant concentration that increases at an accelerating rate as the immigrant population share grows. This pattern, coupled with evidence that the relationship is modified by a county's immigration history, suggests that we need to consider possible consequences of immigrant concentration on victimization risk separately at low, moderate, and high levels of immigrant concentration, while taking into account the broader context of reception for immigrant adaptation.

When we look at the results more specifically, we see that Blacks and Whites enjoy a protective role of immigrant concentration on violent victimization when they live in more established immigrant areas with a level of neighborhood immigrant concentration that is close to or larger than the current national average (13 percent in the 2010–2014 ACS). For counties with much lower levels of immigrant concentration (small immigrant areas or new immigrant areas populated largely by Whites), immigration is yet to show a protective role for victimization among these groups, but neither does it have a measurable crime-stimulating effect. Whether these areas will observe a crime-reduction effect of immigration in the future if immigrant concentration increases is a question requiring longitudinal observation. For now, these findings are useful to encourage local communities to evaluate their attitude toward immigration and create a more immigrant-friendly environment to facilitate the adaptation of immigrants into the receiving communities.

We also find the anticipated protective role of immigrant concentration for Latinos. Nevertheless, in drawing from the threshold argument, we did not expect this relationship to manifest for Latinos in areas with low levels of immigrant concentration (e.g., see figures 2 and 3), and we especially did not expect the effect to level off at a higher level of immigrant concentration (e.g., near 40 percent or higher). In an earlier study, Desmond and Kubrin (2009: 588) suggested that immigrant concentration may have stronger protective effects on youth violence for Latinos compared with non-Latinos because "the

effects of immigrant concentration may be heightened among racial and ethnic groups that share the same language, as well as similar customs and religious beliefs." As currently a great share of immigrants in the United States are Latinos, Latinos may benefit from immigrant concentration at lower levels, compared with Whites and Blacks, which may at least partly explain our findings for Latinos at the lower range of immigrant concentration.

It is less clear why, for Latinos residing in neighborhoods where immigrants approach most of the neighborhood population (e.g., >40 percent), the NCVS data suggest that additional increases in the concentration of immigrants (e.g., an increase from 40 to 60 percent foreign born) yield less return in crime reduction. It is important to reiterate that our data have illustrated the great potential for immigrant concentration to reduce violence for all groups, including Latinos. Indeed, at very high levels of immigrant concentration in traditional/new immigrant areas (e.g., 60 percent), the victimization risk for Latinos (4.6 victimizations per 1,000) is lower than the extent of victimization observed among those who reside in neighborhoods with no immigrants (5.4 per 1,000). Yet, against that backdrop, the data reveal a reduction in the marginal returns from additional immigrant concentration beyond the majority point, a possibility that has not been discussed in the literature.

Future studies should be aimed at exploring this finding to see whether it holds in other periods and national contexts. If so, it could serve as an indication that the crime-protective benefits that arise from immigrant concentration reflect more than just the impact of immigrants on communities but also the extent to which immigrants and natives can form coalitions for purposes of marshalling the needed resources for crime prevention and control. For instance, when a neighborhood is integrated so that both immigrants and natives share the space and the neighborhoods are considered attractive by both, the benefits of immigration (e.g., economic revitalization, enhanced collective efficacy, and access to public social control) may be especially likely to flourish. When a neighborhood approaches immigrant-dominant, however, additional gains in such resources may be increasingly difficult to achieve because such neighborhoods may have limited access to the political capital that serves as a key facilitator (see Lyons, Vélez, and Santoro, 2013). These difficulties may attenuate the net benefit of having more immigrants, causing a diminishing return in crime reduction. To learn more about the majority-immigrant neighborhoods, we need to collect more data on their social, economic, and political structures to understand the challenges that these neighborhoods may face. Our findings, at minimum, suggest that we need to keep monitoring the level of victimization risk in immigrant-dominant neighborhoods and, in doing so, stimulate conversation with members of such communities to understand both the benefits and the potential limits of immigration for crime reduction.

To summarize, our research findings support the key tenet of immigrant revitalization theory that immigration protects communities from violence, but they also show that the typical assumption of linearity and uniform effects across ethnoracial groups is too simplistic. Our results suggest that the immigration–violence relationship takes different forms at different levels of immigrant concentration and in different contexts of receptions, and these nuances need to be more fully integrated into future theoretical and empirical scholarship on immigration and crime.

We encourage future research that is aimed at expanding our study in several directions and at overcoming some of the limitations of our study. Two such expansions seem

especially important. First, it would be valuable to consider the impact of a broader conceptualization of neighborhood immigrant concentration. Our main interest in this article was to assess the capacity of the total immigrant population in the United States for reducing violence, and this influenced our decision to focus on percent foreign born. We found similar results when substituting two other measures—Latino and non-Latino immigrant concentration, respectively—which suggests that the protective role of immigration is not driven by any single ethnic immigrant group. Nonetheless, to expand knowledge, it would be useful to measure immigrant concentration in alternative ways to see whether the immigration–victimization relationship is dependent on the time of immigrant arrival, their countries of origin, or their admission statuses. These measures, and particularly the use of multiple measures, can be useful in the further analysis of the NCVS to advance our understanding of the immigration–crime relationship.

Second, an important next step for research on the impact of community immigrant concentration on victimization risk is to account for the individual immigrant status of respondents. The omission of such an indicator is a limitation of our study. The NCVS traditionally has not collected data on individual immigration status (e.g., citizen, permanent resident, temporary resident, and undocumented), and consequently, this leaves open the important question of whether immigrant status affects victimization risk, an issue to which our research cannot speak. This limitation has been hard to remedy because no national data exist to identify immigrants, their victimization, and the kinds of neighborhoods in which they reside. Recently, however, a measure of citizenship has been added to the NCVS. Although citizenship is not a direct indicator of immigrant status, it captures the extent to which immigrants are incorporated into receiving communities (Bloemraad, Korteweg, and Yurdakul, 2008). When available to researchers, integrating this indicator into multilevel assessments of immigration and victimization risk will be possible, and this could add substantially to the current knowledge base.

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Min Xie is an associate professor of criminology and criminal justice at the University of Maryland—College Park. Her research interests include theories of criminal victimization; race, ethnicity, and gender issues; multilevel and longitudinal models; and spatial data analysis.

Eric P. Baumer is a professor of sociology and criminology at Pennsylvania State University. His research explores demographic, temporal, and spatial patterns of illicit activity; the mobilization of law; and the application of criminal justice sanctions.

Appendix A. Descriptive Statistics of Study Variables

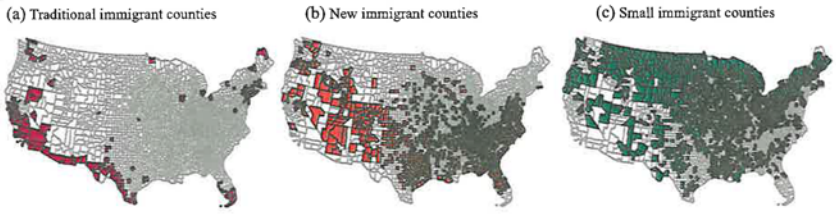
	Whites		Blacks		Latinos	
Characteristics	**Mean**	**(SD)**	**Mean**	**(SD)**	**Mean**	**(SD)**
Main variables						
Violent victimization	.005		.007		.006	
% foreign born	8.981	(9.653)	12.616	(11.954)	25.618	(15.439)
% Latino foreign born	3.121	(5.370)	5.319	(7.156)	17.862	(14.573)
% non-Latino foreign born	5.860	(7.049)	7.297	(8.448)	7.756	(8.048)
% tract foreign born that is Latino	29.195	(27.161)	36.917	(28.110)	62.431	(28.105)
Education in years	13.477	(2.533)	12.602	(2.200)	11.284	(3.127)
Employed	.621		.582		.638	
Household income	11.308	(3.221)	9.503	(3.594)	9.964	(3.385)
Log ratio of noncitizen to citizen odds in low-skill jobs	.883	(.447)	.952	(.345)	1.108	(.264)
Log ratio of Latino to non-Latino odds in low-skill jobs	.826	(.369)	.903	(.284)	.764	(.255)
Non-Latino to Latino unemployment	.872	(1.420)	1.742	(1.874)	.843	(.487)
Traditional immigrant area	.280		.360		.636	
New immigrant area	.388		.399		.278	
Small immigrant area	.332		.240		.086	
Control variables						
Neighborhood-level variables						
Population density	6.866	(1.854)	7.930	(1.485)	8.256	(1.596)
SES disadvantage	−.179	(.607)	.650	(.842)	.452	(.807)
% ages 18–34	22.024	(8.993)	25.125	(6.968)	25.841	(6.847)
% Black	7.506	(11.048)	46.092	(27.628)	10.859	(13.345)
% Latino	9.210	(11.898)	13.252	(14.581)	43.494	(26.383)
% divorced/separated	12.296	(4.275)	15.374	(4.182)	13.104	(3.914)
% moved into units <10 years	56.648	(12.416)	60.290	(12.391)	62.813	(11.947)
% vacant housing	9.424	(7.444)	11.335	(6.103)	8.773	(5.631)
Central city neighborhood	.259		.509		.463	

(*Continued*)

Characteristics	Whites		Blacks		Latinos	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
County labor & housing markets						
% industry in base sectors	24.249	(5.412)	22.506	(4.686)	22.719	(5.081)
% key Latino-employing industries	50.307	(4.753)	49.112	(4.346)	49.821	(4.381)
% unemployed	7.699	(2.022)	8.772	(1.957)	8.268	(1.847)
% Mexicans	54.560	(24.757)	50.327	(23.280)	60.852	(27.651)
% Puerto Ricans	15.182	(15.556)	15.618	(14.523)	9.572	(13.053)
% Salvadorans	2.618	(3.921)	3.811	(5.688)	3.193	(4.066)
% Cubans	2.884	(3.715)	3.586	(5.349)	3.937	(9.400)
Residential segregation	.358	(.128)	.543	(.143)	.454	(.117)
Census region variables						
South	.318		.544		.358	
Midwest	.272		.201		.105	
West	.195		.086		.383	
Individual-level variables						
Age	47.613	(17.784)	42.523	(15.554)	38.328	(14.857)
Male	.489		.450		.500	
Divorced	.110		.110		.074	
Separated	.015		.044		.037	
Never married	.249		.457		.372	
Homeowner	.769		.499		.516	
Years of residence	12.554	(12.204)	9.586	(10.163)	7.603	(8.058)
No. of person-interviews	426,083		61,602		81,926	

NOTE: Standard deviations are only reported for continuous variables.
ABBREVIATIONS: SD = standard deviation; SES = socioeconomic status.

Appendix B. Counties of Different Immigrant Destination Types



SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Table S.1. Logistic Regression Models of Violent Victimization Against Whites
Table S.2. Logistic Regression Models of Violent Victimization Against Blacks
Table S.3. Logistic Regression Models of Violent Victimization Against Latinos

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Adverse Childhood Experiences Among Immigrants to the United States

Michael G. Vaughn, PhD,[1]
Christopher P. Salas-Wright, PhD,[2]
Jin Huang, PhD,[1] Zhengmin Qian, MD, PhD,[1]
Lauren D. Terzis, MSW,[1]
and Jesse J. Helton, PhD[1]

Abstract

A growing number of studies have examined the "immigrant paradox" with respect to health behaviors in the United States. However, little research attention has been afforded to the study of adverse childhood experiences (ACE; neglect, physical and sexual abuse, and witnessing violence) among immigrants in the United States. The present study, using Waves I and II data from the National Epidemiologic Survey of Alcohol and Related Conditions (NESARC), aims to address these gaps by comparing forms of ACE of first- and second-generation immigrants with native-born American adults in the United States. We also examined the latent structure of ACE among immigrants and conducted analyses to assess the psychiatric correlates of identified latent classes. With the exception of neglect, the prevalence of ACE was markedly higher among native-born Americans and second-generation immigrants compared with first-generation immigrants. Four latent classes were identified—limited adverse experience (n = 3,497), emotional and

[1]Saint Louis University, MO, USA
[2]The University of Texas at Austin, TX, USA

Corresponding Author:
Michael G. Vaughn, Saint Louis University, Tegeler Hall, 3550 Lindell Boulevard, St. Louis, MO 63103, USA.
Email: mvaughn9@slu.edu

physical abuse (n = 1,262), family violence (n = 358), and global adversity (n = 246). The latter three classes evinced greater likelihood of being diagnosed with a mood, anxiety, personality, and substance use disorder, and to report violent and non-violent antisocial behavior. Consistent with prior research examining the associations between the immigrant paradox and health outcomes, results suggest that first-generation immigrants to the United States are less likely to have experienced physical and sexual abuse and witness domestic violence. However, likely due to cultural circumstances, first-generation immigrants were more likely to report experiences that are deemed neglectful by Western standards.

Keywords

child maltreatment, physical and sexual abuse, family violence, immigrants, immigrant paradox, mental health, substance use, violence and crime

Introduction

Adverse childhood experiences (ACE) such as physical and sexual abuse, neglect, and witnessing family violence are associated with a wide swath of deleterious outcomes in adulthood (Anda et al., 2006; Briere & Jordan, 2009; Dube, Felitti, Dong, Giles, & Anda, 2003; Kalmakis & Chandler, 2013). Behavioral problems (Gorey & Leslie, 1997; Moylan et al., 2010), low self-esteem and depression (Anda et al., 1999; Chapman et al., 2004; Dube et al., 2001; Gorey & Leslie, 1997; Moylan et al., 2010), substance use (Anda et al., 1999; Dube et al., 2001; Gorey & Leslie, 1997), and criminal behaviors (Gorey & Leslie, 1997; Moylan et al., 2010) are just some of the outcomes that have been found to be associated with ACE. Emerging evidence suggests that substantial heterogeneity exists among individuals with respect to the various types of ACE experiences (Dunn et al., 2011; Vaughn, Salas-Wright, Underwood, & Gochez-Kerr, 2014); however, studies have also found that children who are exposed to one type of childhood abuse or household dysfunction are also likely to be exposed to at least another one (Felitti et al., 1998).

Despite accumulated research derived from U.S. samples, relatively little is known about ACE among immigrant children. Immigrant parents are often placed in stressful situations created by low socioeconomic status (SES; low-income employment and less education), and cultural and language barriers once moved to the United States. Immigrant parents are at an increased risk of experiencing stress associated with low SES, and in turn are at an increased

risk of maltreating their child (Alink, Euser, van IJzendoorn, & Bakermans-Kranenburg, 2013). Culture may play a role for first-generation immigrants, as parenting styles from their country of origin may clash with values in the United States (Strasburger, 2013). Many immigrant parents, including Latino parents, are more likely to engage in "authoritarian style" child rearing practices (Fontes, 2002), which is characterized by high controlling behaviors and low in behaviors that are responsive to a child's needs (Sleddens, Gerards, Thijs, Vries, & Kremers, 2011). Similarly, Asian immigrants to the United States often encounter criticism due to their harsh discipline style that is not congruent with the dominant U.S. culture (Larsen, Kim-Goh, & Nguyen, 2008). Limited research on immigrants and child maltreatment suggests that child rearing practices that lead to adverse outcomes may exist among parents who have been in the United States for a shorter period of time, are of younger age, and are less educated (Rhee, Chang, Berthold, & Mar, 2012).

Studies have suggested that differing cultural values within immigrant populations can attribute to domestic violence. Factors such as polygamy, traditional roles of the husband and wife, infidelity, extended family and their power over the married couple, and the "universal institution of bride price" can contribute to domestic violence in immigrant families (Magwa, 2013). Immigrant women and their children may not realize the abuse they are enduring is not common in the United States, because it may be considered normal in their home country.

Incidents of ACE may go unreported in immigrant communities in the United States, as women and their children may not realize that the abusive behavior they are enduring is considered illegal in the United States. In addition, immigrants often choose not to disclose information to the police due to fear of deportation and losing their children (Clark, 2007). Furthermore, research suggests that U.S. police officers often view violence and neglect in the family as behaviors that are inherently part of the cultures of many immigrants (Menjívar & Salcido, 2002). This could deter immigrant women and children from seeking and reporting abuse to officials, which could explain the lack of knowledge on ACE among immigrant children.

Current Study Aim

Prior research has established a paradox where immigrants are healthier than native-born Americans despite greater disadvantage. A recent spate of studies has extended the immigrant paradox to crime and antisocial behavior (Butcher & Piehl, 2006; Hagan & Palloni, 1999; Lee, Martinez, & Rosenfeld, 2001; Martinez & Lee, 2000; Stowell, Messner, McGeever, & Raffalovich, 2009; Vaughn, Salas-Wright, DeLisi, & Maynard, 2014; Vaughn, Salas-Wright,

Maynard et al., 2014), substance use (Prado et al., 2009; Salas-Wright, Vaughn, Clark, Terzis, & Córdova, 2014), and mental health (Escobar, 1998; Escobar, Nervi, & Gara, 2000; Salas-Wright, Kagotho, & Vaughn, 2014). However, little research has accrued that has compared immigrants with native-born Americans with respect to sexual and physical abuse, neglect, and exposure to family violence (e.g., witnessing father or adult male threatening or hitting your mother). The present investigation fills this gap by examining these forms of ACE among first- and second-generation immigrants and native-born Americans using an 18-item retrospective measure. Furthermore, we identify and describe latent subgroups of immigrants based on sexual and physical abuse, neglect, and witnessing family violence and *Diagnostic and Statistical Manual of Mental Disorders* (*DSM*) substance use and mental health disorders. In so doing, we take advantage of a large population-based sample to fully capture and specify the empirical status of ACE among immigrants to the United States.

Method

Sample and Procedures

Study findings are based on data from Wave I (2001-2002) and Wave II (2004-2005) of the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC). The NESARC is a nationally representative sample of non-institutionalized U.S. residents aged 18 years and older. The survey gathered background data and extensive information about substance use and comorbid psychiatric disorders and information about childhood events, family, and general background characteristics from individuals living in households and group settings such as shelters, college dormitories, and group homes in all 50 states and the District of Columbia. NESARC utilized a multistage cluster sampling design, oversampling young adults, Hispanics, and African Americans in the interest of obtaining reliable statistical estimation in these subpopulations, and to ensure appropriate representation of racial/ethnic subgroups. Multistage cluster sampling design is a commonly used design when attempting to provide nationally representative estimates. This is because interviewing all participants is not feasible, so larger units (i.e., clusters) are identified and randomly selected from. With respect to the NESARC, 709 primary sampling units (PSUs) provided by the Census Supplementary Survey was selected (Stage 1). Within the sample PSUs, households were systematically selected (Stage 2). An individual aged 18 or older was randomly selected from each household. The response rate for Wave I data was 81% and for Wave II 86.7% ($N = 34,653$) with a cumulative

response rate of 70.0% for both waves. Data were weighted at the individual and household levels to adjust for oversampling and non-response on demographic variables (i.e., age, race/ethnicity, sex, region, and place of residence). Data were also adjusted to be representative (based on region, age, race, and ethnicity) of the U.S. adult population as assessed during the 2000 Census. Adjustments were also made by NESARC investigators to account for any sociodemographic or psychiatric differences resulting from Wave 2 non-response. This process is described in greater detail elsewhere (see Grant et al., 2008; Grant, Kaplan, & Stinson, 2007). Study participants provided fully informed consent. The U.S. Census Bureau and the U.S. Office of Management and Budget approved the research protocol and informed consent procedures. Sociodemographic information on U.S.-born as well as first- and second-generation immigrants in the Waves I and II NESARC samples is available elsewhere (Salas-Wright et al., 2014).

Data were collected through face-to-face structured psychiatric interviews conducted by U.S. Census workers trained by the National Institute on Alcohol Abuse and Alcoholism and U.S. Census Bureau. Interviewers administered the *Alcohol Use Disorder and Associated Disabilities Interview Schedule–Diagnostic and Statistical Manual of Mental Disorders* (4th ed.; *DSM-IV*; American Psychiatric Association [APA], 1994) version (AUDADIS-IV), which has been shown to have good-to-excellent reliability in the diagnosis of mental disorders in the general population (Grant, Hartford, Dawson, Chou, & Pickering, 1995; Hasin, Carpenter, McCloud, & Grant, 1997). Participants had the option of completing the NESARC interview in English, Spanish, or one of four Asian languages (i.e., Mandarin, Cantonese, Korean, and Vietnamese). More details about the NESARC design and procedures are available elsewhere (Grant & Dawson, 2006).

Measures

Childhood adverse experience. At Wave II interviews, study participants were asked about 18 measures of childhood adverse experiences in the domains of neglect, emotional and physical abuse, family violence, and sexual abuse. These items were drawn from previously validated measures including the Childhood Trauma Questionnaire (Bernstein et al., 1994) and Conflict Tactics Scale (Straus, 1979). Consistent with previous research, items were dichotomously coded (0 = *no*, 1 = *yes*) depending on whether the respondent reported having ever experienced a specific adversity as a child (McLaughlin, Conron, Koenen, & Gilman, 2010; Vaughn et al., 2011). Table 1 provides a comprehensive list of each of these items.

Table 1. Adjusted Risk Ratios for Adverse Childhood Experiences Among Non-Immigrants and Second-Generation Immigrant Adults With First-Generation Immigrants as the Reference Group.

	Native-Born Americans (n = 24,461)		Second-Generation Immigrants (n = 4,826)	
	ARR	(95% CI)	ARR	(95% CI)
Neglect				
Any neglect	**0.95**	**[0.90, 0.99]**	**0.87**	**[0.83, 0.90]**
Parent/caregiver . . .				
Made to do chores that were too difficult or dangerous for someone your age	0.99	[0.94, 1.04]	**0.81**	**[0.77, 0.85]**
Left you alone or unsupervised when you were too young to be alone	**0.91**	**[0.87, 0.95]**	**0.89**	**[0.85, 0.92]**
Went without things you needed like clothes, shoes, or school supplies because a parent or other adult living in your home spent the money on themselves	**0.89**	**[0.84, 0.94]**	**0.82**	**[0.77, 0.86]**
Made you go hungry or did not prepare regular meals	1.02	[0.94, 1.10]	**0.91**	**[0.84, 0.99]**
Ignored or failed to get you medical treatment when you were sick or hurt	0.93	[0.87, 1.00]	**0.80**	**[0.74, 0.87]**
Abuse				
Any physical/emotional abuse	**1.50**	**[1.45, 1.56]**	**1.41**	**[1.35, 1.47]**
Parent/other adult living in your home . . .				
Swore at you, insulted you, or said hurtful things	**1.61**	**[1.55, 1.66]**	**1.40**	**[1.35, 1.46]**
Threatened to hit you or throw something at you, but didn't do it	**1.37**	**[1.32, 1.42]**	**1.38**	**[1.33, 1.44]**
Made you afraid that you would be physically hurt/injured	**1.37**	**[1.32, 1.43]**	**1.40**	**[1.35, 1.46]**
Pushed, shoved, slapped, or hit you	**1.41**	**[1.36, 1.47]**	**1.37**	**[1.32, 1.42]**
Hit you so hard that you had marks or bruises or were injured	**1.41**	**[1.34, 1.48]**	**1.33**	**[1.27, 1.40]**

Table 1. (continued)

	Native-Born Americans (n = 24,461)		Second-Generation Immigrants (n = 4,826)	
	ARR	(95% CI)	ARR	(95% CI)
Domestic violence				
Any domestic violence	**1.44**	**[1.37, 1.52]**	**1.48**	**[1.42, 1.54]**
Father/other adult male . . .				
Pushed, grabbed, slapped, or threw something at your mother	**1.47**	**[1.40, 1.56]**	**1.51**	**[1.44, 1.58]**
Kicked, bit, hit your mother with a fist or with something hard	**1.53**	**[1.43, 1.63]**	**1.53**	**[1.44, 1.62]**
Repeatedly hit your mother for at least a few minutes	**1.44**	**[1.33, 1.57]**	**1.35**	**[1.26, 1.45]**
Threatened your mother with a knife or gun or used a knife or gun to hurt her	**1.51**	**[1.41, 1.63]**	**1.32**	**[1.21, 1.45]**
Sexual abuse				
Any sexual abuse	**1.41**	**[1.34, 1.48]**	**1.31**	**[1.23, 1.40]**
Adult/other person . . .				
Fondled you in a sexual way when you didn't want them to or when you were too young to know what was happening	**1.51**	**[1.43, 1.60]**	**1.38**	**[1.28, 1.49]**
Had you touch their body in a sexual way that you didn't want to or were too young to know what was happening	**1.26**	**[1.18, 1.34]**	**1.35**	**[1.26, 1.45]**
Attempted to have sexual intercourse with you when you didn't want them to or were too young to know what was happening	**1.37**	**[1.29, 1.45]**	**1.22**	**[1.13, 1.32]**
Actually had sexual intercourse with you when you didn't want them to or were too young to know what was happening	**1.29**	**[1.20, 1.38]**	**1.13**	**[1.04, 1.23]**

Note. Reference group: First-generation immigrants (n = 5,363). Adjusted odds ratios adjusted for age, gender, race/ethnicity, household income, education level, marital status, region of the United States, and urbanicity. Odds ratios in bold are significant at $p < .05$ or lower. ARR = adjusted risk ratios; CI = confidence intervals.

Immigrant status. At Wave I, respondents were asked whether they and their parents were born in the United States. Three mutually exclusive categories were created: Respondents who reported having been born outside the United States (n = 5,363) were classified as first-generation immigrants. Respondents who reported that they had been born in the United States but at least one parent had been born outside the United States (n = 4,826) were classified as second-generation immigrants. Respondents who reported that they and their parents were born in the United States were considered native-born Americans (n = 24,461).

Mental disorders. Using the AUDADIS-IV administered during Waves I and II, we examined lifetime *DSM-IV* mood (i.e., bipolar disorder, major depression, dysthymia), anxiety (i.e., generalized anxiety disorder, panic disorder, social phobia, specific phobia, and posttraumatic stress disorder), and personality disorders (i.e., antisocial, avoidant, borderline, narcissistic, obsessive–compulsive, paranoid, schizoid, and schizotypal). We also examined alcohol, cannabis, and other illicit drug (i.e., heroin, hallucinogens, cocaine/crack, marijuana, stimulants, painkillers, tranquilizers, or sedatives) use disorders (abuse/dependence) by means of the AUDADIS-IV.

Sociodemographic controls. The following sociodemographic variables assessed at Wave I were included as controls (in the multinomial regression) and indicator covariates (in the latent class analysis [LCA]): age, gender, race/ethnicity, household income, education level, marital status, region of the United States, and urbanicity.

Data Analysis

Analyses were executed in several successive steps. We first examined the prevalence of childhood adverse experiences among native-born Americans and across immigrant generations (n = 34,650). Next, we examined the relationship between childhood adverse experience and mental and behavioral health among immigrants (n = 5,363). These steps are described in greater detail below.

We examined the prevalence of each of the 18 childhood adverse experiences among first-generation immigrants, second-generation immigrants, and native-born Americans. Next, we executed a series of multinomial regression analyses to contrast the prevalence of childhood adverse experiences among first-generation immigrants with those of second-generation immigrants and native-born Americans. We present adjusted risk ratios (ARR), which are conceptually similar to odds ratios in that they refer to the

likelihood of membership in a specified class versus that of a reference class (Zhang & Yu, 1998).

Given the large sample of immigrants and likely heterogeneity, we conducted an LCA to identify subgroups of immigrants on the basis of the 18 child adverse experience indicator variables and sociodemographic indicator covariates. Specifically, a sequence of latent class models was identified between one and five classes using Latent GOLD® 4.5 (Vermunt & Magidson, 2008) software. Five statistical criteria were used to identify the best fitting model: the Bayesian Information Criterion (BIC), Akaike's Information Criterion (AIC), Consistent Akaike's Information Criterion (CAIC), Log Likelihood, and entropy. In interpreting these criteria, lower BIC, AIC, and CAIC values and higher log likelihood and entropy values reflect better model fit. Substantive criteria (i.e., parsimony, interpretability) were also considered. After identifying the best model and assigning cases to nominal groups, we examined the association between class membership and the prevalence of mental disorders and antisocial behavior using multinomial regression. For all statistical analyses, weighted prevalence estimates and standard errors were computed using Stata 13.1 SE software (StataCorp, 2013).

Results

Adverse Childhood Experience Among Immigrants and Non-Immigrants

Figure 1 presents the prevalence of self-reported experiences of adverse childhood events among native-born Americans and first- and second-generation immigrants. Table 1 displays the ARRs for ACEs among native-born Americans and second-generation immigrant adults with first-generation immigrants as the reference group. Controlling for sociodemographic factors, native-born Americans (ARR = 0.95, 95% confidence interval [CI] = [0.90, 0.99]) and second-generation immigrants (ARR = 0.87, 95% CI = [0.83, 0.90]) were significantly less likely to report having experienced any form of neglect as compared with first-generation immigrants. Notably, with respect to second-generation immigrants, this relationship held for all manifestations of neglect examined; however, among native-born Americans, effects were only observed for "left unsupervised" (ARR = 0.91, 95% CI = [0.87, 0.95]) and "go without things" (ARR = 0.89, 95% CI = [0.84, 0.94]).

Controlling for the same array of sociodemographic factors, compared with first-generation immigrants, native-born Americans and second-generation immigrants were significantly more likely to report exposure to emotional and physical abuse (native-born American: ARR = 1.50, 95% CI =

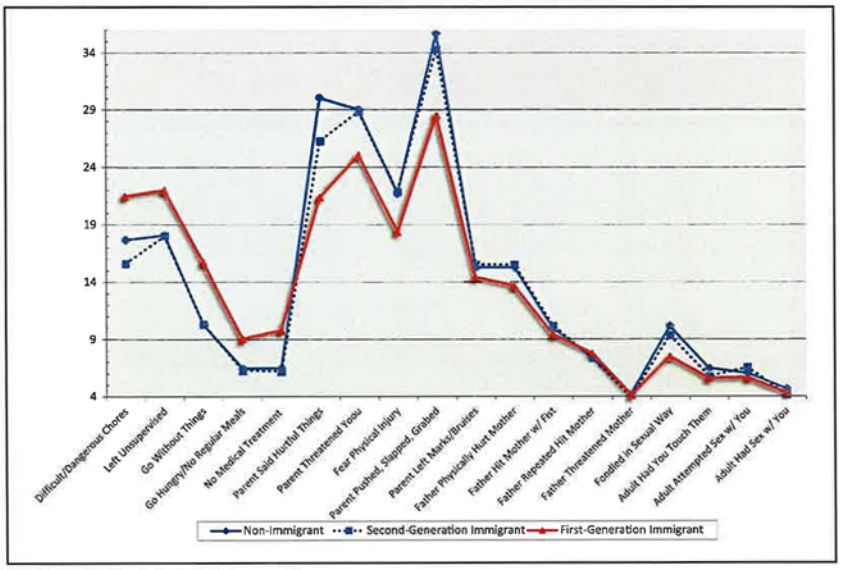

Figure 1. Prevalence of self-reported experience of adverse childhood events.

[1.45, 1.56]; second-generation immigrants: ARR = 1.41, 95% CI = [1.35, 1.47]), family violence (native-born American: ARR = 1.44, 95% CI = [1.37, 1.52]; second-generation immigrants: ARR = 1.48, 95% CI = [1.42, 1.54]), and sexual abuse (native-born American: ARR = 1.41, 95% CI = [1.34, 1.48]; second-generation immigrants: ARR = 1.31, 95% CI = [1.23, 1.40]). As evidenced by the overlapping 95% CIs, the magnitude of the relationship between immigrant status and these ACEs is not significantly different between native-born Americans and second-generation immigrants as compared with first-generation immigrants. In addition, it should be noted that, with varying effect sizes, this relationship held for all manifestations of ACEs examined in these domains.

Latent Subgroups of Immigrants Based on Adverse Childhood Experiences

An evaluation of the fit indices and substantive criteria suggests that a four-class solution was the optimal modeling of the data. There was an accelerated flattening of the differences in the log likelihood, BIC, AIC, and CAIC values suggesting that the addition of a fifth class would not be parsimonious. Moreover, inspection of entropy values suggests that the four-class solution

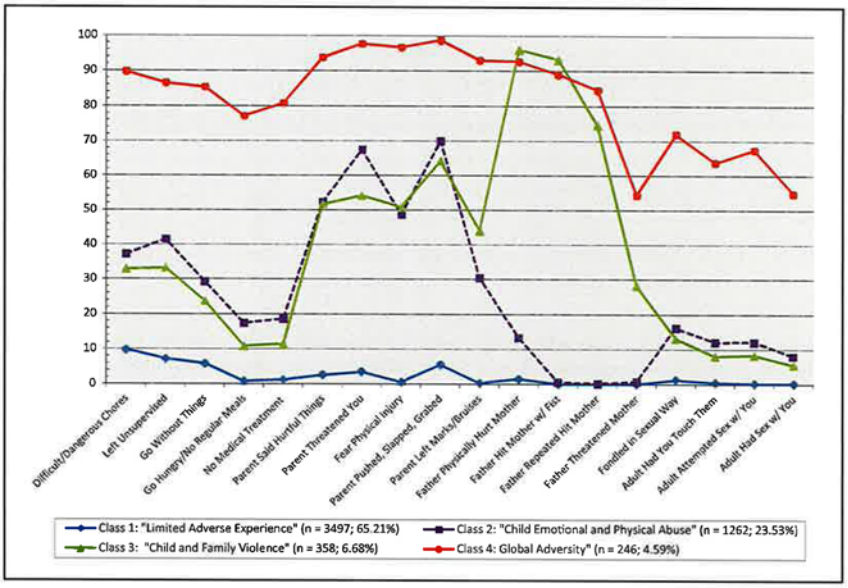

Figure 2. Adverse childhood events across latent classes.

has acceptable model fit. These statistical criteria also cohere with the substantive interpretability of the four-class solution, which offers a conceptually clear modeling of the heterogeneity of the data. As illustrated in Figure 2, the four classes can be readily distinguished from one another on the basis of ACEs in the domains of neglect, emotional and physical abuse, family violence, and sexual abuse.

The four-class solution is composed of the following classes: Class 1: "Limited Adverse Experience" (n = 3,497; 65.21%), Class 2: "Child Emotional and Physical Abuse" (n = 1,262; 23.53%), Class 3: "Child and Family Violence" (n = 358; 6.68%), and Class 4: "Global Adversity" (n = 246; 4.59%). Class 1, which includes nearly two out of every three respondents in the immigrant subsample, is characterized by universally low levels of ACEs across all domains. Class 2, accounting for nearly one in four respondents in the immigrant subsample, is characterized by moderate levels of neglect and elevated levels of emotional and physical abuse in combination with relatively low levels of family violence and sexual abuse. Class 3, which is notably smaller than the first two classes, is characterized by similar levels of neglect, emotional and physical abuse, and sexual abuse as those identified in Class 2; however, this class is also characterized by exceedingly

elevated levels of family violence. Finally, Class 4, the smallest of all four classes, is characterized by elevated levels of all ACEs examined in the study.

Sociodemographic Characteristics of the Latent Classes

Table 2 presents the sociodemographic characteristics of the four latent classes and for the entire sample of immigrants. Significant bivariate differences were observed for age ($F = 19.06$, $p < .001$), gender ($\chi^2 = 72.75$, $p < .001$), race/ethnicity ($\chi^2 = 32.14$, $p < .001$), household income ($\chi^2 = 22.20$, $p < .001$), and education ($\chi^2 = 49.35$, $p < .001$). Class 1 (Limited Adverse Experience) is relatively evenly distributed in terms of gender (female = 52.99%, male = 47.01%) and is composed of the highest proportion of non-Hispanic White respondents (23.62%). Class 2 (Child Emotional and Physical Abuse) has the highest proportion of male respondents (59.54%) as well as the highest proportion of respondents earning more than $70,000 per year (26.68%) and possessing a post-secondary degree (36.63%). Class 3 (Child and Family Violence) is the youngest of all classes ($M = 43.58$, $SD = 13.54$) and is composed of the highest proportion of Hispanic respondents (54.33%). Class 4 (Global Adversity) is the oldest of all classes ($M = 48.11$, $SD = 15.23$) and is composed of the highest proportion of female respondents (61.43%) as well as respondents of "other" ethnicity (26.98%), residing in households earning less than $20,000 per year (28.71%), and reporting less than a high school diploma (44.25%).

Supplementary Analyses

We also examined various immigration-related factors in relation to membership in the four latent classes. Controlling for the same list of sociodemographic confounds, significant differences were observed with respect to the developmental age at the time of immigrant arrival. Compared with the reference class (Class 1: Limited Adverse Experience), members of Class 2 (Child Emotional and Physical Abuse; ARR = 1.43, 95% CI = [1.35, 1.51]) and Class 3 (Child and Family Violence; ARR = 2.49, 95% CI = [2.32, 2.68]) were significantly more likely to have immigrated during childhood (i.e., age 12 or younger). Although effects were relatively small, members of Class 2 (Child Emotional and Physical Abuse) were also significantly more likely to have arrived during their teenage years (ARR = 1.10, 95% CI = [1.02, 1.18]). No significant differences in the developmental age at the time of immigrant arrival were observed for Class 4 (Global Adversity). In addition, no significant differences were observed with respect to total duration—commonly used as a proxy for acculturation—as an immigrant in the United States.

Table 2. Sociodemographic Characteristics by Latent Class.

	Class 1: Limited Adverse Experience	Class 2: Child Emotional and Physical Abuse	Class 3: Child and Family Violence	Class 4: Global Adversity		
	(*n* = 3,497; 65.21%)	(*n* = 1,262; 23.53%)	(*n* = 358; 6.68%)	(*n* = 246; 4.59%)	Full Immigrant Sample	*F*/$\chi 2$ *Significance*
	M (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	
Sociodemographic factors						
Age	47.62 (16.59)	44.26 (14.39)	43.58 (13.54)	48.11 (15.23)	46.58 (15.93)	19.06***
Gender						
Female	2,073 (52.99)	626 (40.46)	214 (54.68)	163 (61.43)	3,076 (50.48	72.75***
Male	1,424 (47.01)	636 (59.54)	144 (45.32)	83 (38.57)	2,287 (49.52)	
Race/ethnicity						
Non-Hispanic White	613 (23.62)	226 (20.94)	45 (17.32)	31 (15.94)	915 (22.31)	32.14***
African American	292 (6.90)	155 (9.82)	34 (8.37)	22 (9.35)	503 (7.78)	
Asian/Other	476 (24.30)	169 (22.51)	41 (19.98)	40 (26.98)	717 (23.74)	
Hispanic	2,125 (45.18)	712 (46.73)	238 (54.33)	153 (47.73)	3,228 (46.18)	
Global region						
Africa	69 (2.20)	26 (1.70)	6 (1.33)	4 (2.32)	105 (2.04)	18.71***
Latin America	2,094 (49.98)	685 (51.94)	238 (61.23)	152 (52.47)	3,169 (51.19)	
Europe	518 (20.85)	205 (20.74)	40 (14.74)	29 (15.20)	792 (20.24)	
Asia	459 (26.96)	165 (25.62)	41 (22.71)	39 (30.00)	704 (26.53)	

(continued)

Table 2. (continued)

	Class 1: Limited Adverse Experience (*n* = 3,497; 65.21%)	Class 2: Child Emotional and Physical Abuse (*n* = 1,262; 23.53%)	Class 3: Child and Family Violence (*n* = 358; 6.68%)	Class 4: Global Adversity (*n* = 246; 4.59%)	Full Immigrant Sample	*F*/χ2 Significance
	M (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	*M* (*SD*)/*n* (%)	
Household income (in US$)						
<20,000	975 (22.76)	291 (21.15)	95 (23.18)	70 (28.71)	1,431 (22.65)	22.20***
20,000-34,999	819 (22.67)	267 (20.43)	101 (23.94)	61 (23.12)	1,148 (22.23)	
35,000-69,999	1,059 (32.07)	400 (31.75)	104 (35.02)	73 (26.11)	830 (31.91)	
>70,000	644 (22.51)	304 (26.68)	58 (17.86)	42 (22.07)	1,688 (23.20)	
Education						
Less than HS	1,135 (29.33)	335 (25.37)	124 (33.29)	103 (44.25)	1,697 (29.25)	49.35***
HS graduate	783 (21.89)	253 (19.64)	74 (19.94)	38 (14.78)	1,148 (20.95)	
Some post-secondary	508 (14.66)	227 (18.36)	52 (14.81)	43 (16.38)	830 (15.61)	
Post-secondary degree	1,071 (34.13)	447 (36.63)	108 (31.96)	62 (24.59)	1,688 (34.20)	

Note. All percentages are reported as column percentages; HS = high school.
*$p < .05$. **$p < .01$. ***$p < .001$.

Beyond duration, we were unable to examine differences in acculturation as the NESARC does not include acculturation-related items that can be used across multiple regions of origin.

We also examined the region of origin across the four latent classes. Controlling for sociodemographic confounds we found that, with Class 1 (Limited Adverse Experience) as the reference class, immigrants from Africa were significantly less likely to be members of Class 2 (Child Emotional and Physical Abuse; ARR = 0.66, 95% CI = [0.59, 0.74]) and Class 3 (Child and Family Violence; ARR = 0.50, 95% CI = [0.28, 0.90]). African immigrants were, however, significantly more likely to be members of Class 4 (Global Adversity; ARR = 1.35, 95% CI = [1.26, 1.44]). A similar pattern was observed for immigrants from Asia who were significantly less likely to be members of Class 2 (ARR = 0.93, 95% CI = [0.89, 0.97]) and Class 3 (ARR = 0.79, 95% CI = [0.71, 0.88]), but significantly more likely to be members of Class 4 (Global Adversity; ARR = 1.40, 95% CI = [1.29, 1.52]). European immigrants were significantly more likely to be members of Class 2 (Child Emotional and Physical Abuse; ARR = 1.08, 95% CI = [1.02, 1.14]) and significantly less likely to be members of Classes 3 (Child and Family Violence; ARR = 0.72, 95% CI = [0.53, 0.97]) and 4 (Global Adversity; ARR = 0.80, 95% CI = [0.72, 0.89]).

DSM *Disorders and Antisocial Behavior by Latent Class*

Table 3 presents the ARRs for the latent classes in contrast to the reference class (i.e., Limited Adverse Experience). Members of Classes 2 to 4 were significantly more likely to meet criteria for a *DSM* mental or substance use disorder and to report involvement in violence and non-violent crime. With the exception of other illicit drug use, the largest effects were consistently observed among members of Class 3 (Child and Family Abuse). Particularly large effects were observed for cannabis use disorder among members of Class 3 (ARR = 7.93, 95% CI = [5.89, 10.66]) as well as other illicit drug use among members of Class 4 (Global Adversity; ARR = 6.60, 95% CI = [5.52, 7.89]).

Discussion

Heightened interest in extending research on the immigrant paradox has revealed notable findings indicating that despite greater disadvantage, immigrants are less likely to be diagnosed with mental health disorders, and engage in less crime and violence, and substance abuse (Butcher & Piehl, 2006; Escobar, 1998; Escobar et al., 2000; Hagan & Palloni, 1999; Lee et al., 2001; Martinez & Lee, 2000; Prado et al., 2009; Salas-Wright, Kagotho, & Vaughn,

Table 3. Adjusted Risk Ratios of *DSM* Disorders and Antisocial Behavior by Latent Class.

	Class 2: Child Emotional and Physical Abuse		Class 3: Child and Family Violence		Class 4: Global Adversity	
	(*n* = 1,262; 23.53%)		(*n* = 358; 6.68%)		(*n* = 246; 4.59%)	
	ARR	(95% CI)	ARR	(95% CI)	ARR	(95% CI)
DSM mental disorders						
Mood	**2.24**	**[2.10, 2.39]**	**3.10**	**[2.78, 3.45]**	**2.22**	**[2.01, 2.44]**
Anxiety	**2.27**	**[2.14, 2.41]**	**3.81**	**[3.48, 4.16]**	**2.73**	**[2.54, 2.92]**
Personality	**2.50**	**[2.36, 2.66]**	**3.96**	**[3.57, 4.38]**	**2.96**	**[2.75, 3.19]**
DSM substance use disorders						
Alcohol	**2.28**	**[2.17, 2.40]**	**2.45**	**[2.16, 2.77]**	**1.87**	**[1.63, 2.15]**
Cannabis	**2.51**	**[1.88, 3.36]**	**7.93**	**[5.89, 10.66]**	**4.77**	**[3.18, 7.16]**
Other illicit drug	**2.96**	**[2.66, 3.30]**	**4.58**	**[2.54, 8.28]**	**6.60**	**[5.52, 7.89]**
Antisocial behavior						
Violence	**2.76**	**[2.47, 3.09]**	**3.85**	**[3.38, 4.38]**	**3.50**	**[3.24, 3.78]**
Non-violent crime	**2.30**	**[2.18, 2.43]**	**3.50**	**[3.10, 3.94]**	**2.40**	**[2.04, 2.82]**

Note. Reference class is Class 1: "Limited Adverse Experience" (*n* = 3,497; 65.21%). Adjusted risk ratios (ARR) adjusted for age, gender, race/ethnicity, household income, marital status, region of the United States, and urbanicity. ARRs and 95% confidence intervals (CI) in bold are significant at $p < .05$. *DSM* = *Diagnostic and Statistical Manual of Mental Disorders.*

2014; Salas-Wright & Vaughn, 2014; Salas-Wright, Vaughn, Clark, Terzis & Córdova, 2014; Stowell et al., 2009; Vaughn, Salas-Wright, DeLisi, & Maynard, 2014). The present study builds on this recent tradition and to our knowledge, is the largest study of ACE among immigrants vis-à-vis native-born Americans.

Consistent with prior research on the immigrant paradox, we found that native-born Americans and second-generation immigrants were more likely to report physical and emotional abuse, witnessing domestic violence, and sexual abuse. One notable exception, however, was found for neglect. It seems quite plausible that behaviors deemed neglectful in the American cultural context such as doing chores that are difficult or dangerous, ignoring or failing to obtain medical treatment when sick or hurt, going hungry, and not having clothes, shoes, and school supplies are part and parcel of the deprivation of the lived experience in less advantaged cultural contexts and a motivating force for emigrating to the United States.

Additional noteworthy findings surfaced when we examined the presence of latent subgroups. Although approximately two thirds of immigrants reported very limited ACE, three other latent classes were identified including a global adversity latent class that was composed of a significantly larger proportion of females and persons with less than a high school education. Other research has shown that immigrant families may place less value on the well-being of females, particularly if poorly educated (Jewkes, 2002; Raj & Silverman, 2002). Of critical interest is that, compared with those immigrants experiencing limited adversity, the three ACE classes were substantially more likely to be diagnosed with a *DSM*-based mood, personality, or substance use disorder. Violent and non-violent crime was also significantly more likely. Although particularly large effects were found for cannabis and other drug use disorders, in large measure, it did not matter what type of ACE latent class an immigrant was a member of as it was associated with increased probability of poor mental and behavioral health.

Present study findings possess broad implications. First, and perhaps foremost, policies and practices that operate under an assumption (implicit or explicit) of increased risk of physical and sexual abuse and family violence among immigrants are likely to be flawed. Although few studies have investigated ACE among immigrants, the present population-based study clearly demonstrates this is not the case. However, neglect may be differentially perceived cross-culturally (or just a matter of pre-migration circumstances), and the American child welfare norms around neglect my come in conflict with the norms of some immigrants. Thus, future endeavors that target raising awareness about the lower levels of violence found in this and other studies among first-generation immigrants, which often contrast with depictions found in

popular culture, are important in changing viewpoints and mitigating some of the xenophobia that acts as a barrier to rational immigrant policies. Although the present investigation possesses a number of assets, several important limitations are worth highlighting. One major limitation is the data are less than ideal with respect to temporal ordering. Thus, any causal determinations are not possible. Another major limitation is that we necessarily rely on retrospective and uncorroborated self-reported measures of ACE, and this approach has been shown to possess several limitations (Widom, 1989a, 1989b, 1991). To rectify these aforementioned limitations, prospective life-course study designs are needed. An additional limitation is that the data did not include important situational or contextual information on the immigrant experience, which could be revealing with regard to cultural circumstances.

Immigrant families often find themselves in disadvantaged contexts. These contexts have been found to be associated with ACE in native-born U.S. samples. However, immigrants, despite the difficult circumstances in which they are often enmeshed, are less likely, except for neglect, to report ACE. Present study findings run counter to popular opinion and limited empirical research using non-representative samples on ACE among immigrants. As such, findings are consistent with recent extensions of the immigrant paradox to problem behaviors. The present study provides a useful baseline from which future studies can build on vis-à-vis ACE and immigrants.

Declaration of Conflicting Interests

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Author Biographies

Michael G. Vaughn, PhD, is a professor in the School of Social Work at Saint Louis University. He has contributed more than 250 scholarly publications across a wide range of areas. His research interests include youth violence, adolescent psychopathy, and drug use and abuse in relation to antisocial behavior over the life course. Current projects include a cell-to-society approach to the study of human behavior, youth violence prevention, school dropout, and drug use epidemiology.

Christopher P. Salas-Wright, PhD, is an assistant professor in the School of Social Work at the University of Texas at Austin. His research interests include youth problem behavior, religiosity and spirituality, and adolescent substance abuse and violence, particularly among Latino youth in the United States and in Latin America.

Jin Huang, PhD, is an assistant professor in the School of Social Work at Saint Louis University. His research interests include poverty and child development and quantitative methods.

Zhengmin Qian, MD, PhD, is a professor and the chair in the Department of Epidemiology, College of Public Health and Social Justice, Saint Louis University. His research interests include the effect of environmental sources of pollution on health and social epidemiology.

Lauren D. Terzis, MSW, is a doctoral student at Saint Louis University. Her research interests involve immigrants and problem behaviors.

Jesse J. Helton, PhD, is an assistant professor in the School of Social Work at Saint Louis University. His research interests include childhood adversity and health including obesity and studies of the child welfare system.

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On the association between undocumented immigration and crime in the United States

By Christian Gunadi[a]

[a]Department of Economics, University of California Riverside, 900 University Ave, Riverside, California 92521, USA; christian.gunadi@email.ucr.edu

Abstract

Approximately 11 million undocumented individuals live in the United States. At the same time, there are concerns that the presence of undocumented immigrants may contribute to an increase in crime rates. In this article, I examine the institutionalization rate of undocumented immigrants and quantify the change in crime rates attributable to undocumented immigration. The analysis yields a few main results. First, despite possessing characteristics usually associated with crime, undocumented immigrants are 33% less likely to be institutionalized compared to US natives. Second, there is no evidence that undocumented immigrants who have spent more time in the USA are more likely to be institutionalized compared to those who have been in the USA for a shorter time. There is evidence, however, that arriving at a younger age is associated with higher institutionalization rate. Finally, overall property and violent crime rates across US states are not statistically significantly increased by undocumented immigration.

JEL classifications: K37, K42.

1. Introduction

Crimes are costly for society. Cohen (1988) estimated that the annual cost of crime to the victims is approximately US$92.6 billion in the United States. Similarly, Miller *et al.* (1993) estimated that violent crime led to US$23 billion in lost productivity and approximately US$145 billion in reduced quality of life in the United States. At the same time, there are concerns that an increase in immigration may lead to a sizable increase in crime rates. When asked about the consequence of more immigrants coming into the United States, almost half of the respondents of Gallup polling in 2017 stated that immigrants make the crime situation worse. Therefore, immigration—especially undocumented immigration—may impose a cost to society through an increase in crime rates. In this article, I examine the institutionalization (incarceration) rate of undocumented immigrants and quantify the change in crime rates attributable to undocumented immigration.

The empirical literature on immigration and crime in the USA seems to broadly agree that immigrants are no more or even less likely to participate in criminal activity compared

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to US natives (Butcher and Piehl, 1998a, 1998b; Reid *et al.*, 2005; Moehling and Piehl, 2009; Stowell *et al.*, 2009). However, there are reasons to believe that undocumented immigrants would be more likely to commit crime compared to those who are legally in the United States or US natives.[1] Undocumented immigrants are overrepresented among low-skilled workers (see Fig. 1), which makes the returns from participating in informal labour markets—such as crime—relatively higher for them. Furthermore, undocumented immigrants are more likely to be younger men (see Figs 2 and 3), which is the group that is more frequently involved in criminal activity than others (Freeman, 1999). Therefore, an increase in the share of undocumented immigrants in the population may lead to a higher crime rate.

On the other hand, undocumented immigrants may have the most incentive to not commit a crime because contact with law enforcement even for a minor offence would increase the probability that their illegal status would be revealed. For example, Butcher and Piehl (2007) argue that one of the reasons why immigrants who arrived most recently are less likely to commit a crime is the higher share of undocumented immigrants in the recent immigrant cohorts. It is possible, therefore, that an increase in the share of undocumented immigrants in the population would lead to lower crime rates.

I proceed as follows in analysing the connection between undocumented immigration and crime. First, I examine the institutionalization rate of undocumented immigrants compared to other US residents. A higher institutionalization rate would give an indication that undocumented immigrants are indeed more likely to commit a crime once they are in the United States compared to other US residents. Then, I examine whether undocumented immigration contributes to a sizable increase in crime rates. Although undocumented immigrants might be more (less) likely to commit a crime compared to those who are legally in the USA or natives, undocumented immigration may not necessarily increase (lower) crime rates. Previous studies have noted that immigration may increase labour market competition among other US residents, worsening their labour market outcomes and incentivizing them to commit a crime (Borjas *et al.*, 2010). At the same time, undocumented immigration may spur job creation and improve the labour market outcomes of US natives and legal residents (Chassamboulli and Peri, 2015), which in turn lowers crime (Gould *et al.*, 2002). Therefore, the question of whether undocumented immigration has an adverse effect on crime rates depends not only on the likelihood of undocumented immigrants committing a crime but also on the resulting externalities that may increase or reduce crime rates.

At the individual level, I find that undocumented immigrants are approximately 33% less likely to be institutionalized compared to US natives. After accounting for differences in background characteristics such as education and age, the institutionalization rate gap between US-born individuals and undocumented immigrants becomes even larger. I also find no evidence that undocumented immigrants who have spent more time in the USA are more likely to be institutionalized compared to those who have been in the USA for a shorter time. However, there is evidence that arriving at a younger age is associated with higher institutionalization rate.

1 It should be noted that illegal/improper entry into the United States carries criminal penalties and thus constitutes a crime. The subject of this article is therefore whether undocumented immigrants are more likely commit a crime once they are in the USA (i.e. other than the one that they already did by illegally crossing the border) and whether their presence contributes to the increase in crime rates.

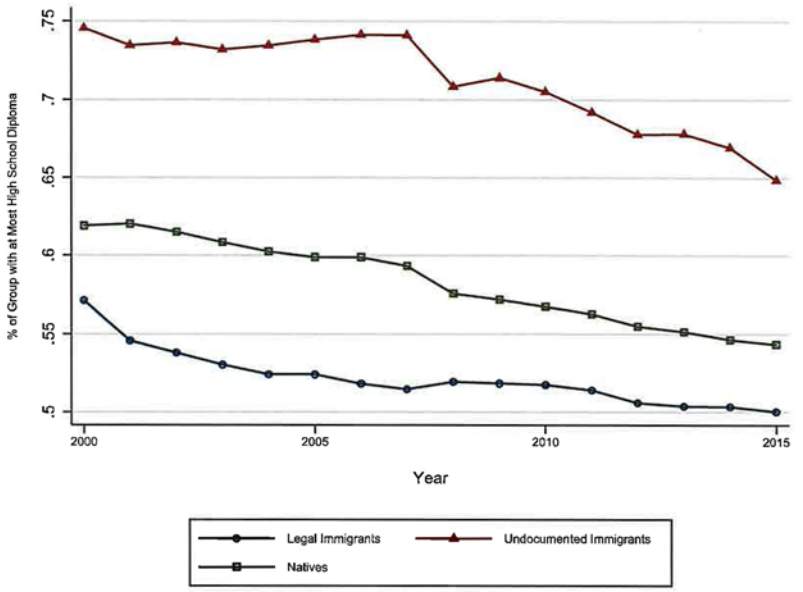

Fig. 1. Share of individuals with at most high school diploma by residency status.
Source: Author's calculation from IPUMS 5% 2000 Census and American Community Survey (ACS) 2001–2015. The graph shows the share of individuals with at most high school diploma by residency status over the time period in the analysis. Sampling weight is used in the analysis.

The analysis of whether undocumented immigration increases crime rates across US states is complicated by a few factors that may bias the effect of interest. For example, undocumented immigrants may endogenously select where they migrate to. Then, in response to the inflow of undocumented immigrants, the other US residents may choose to leave or migrate into the area. Furthermore, there may be a systematical underreporting of crime in the state where there is a large share of undocumented immigrants because they are less likely to report a crime because of fear of deportation.[2] To address these concerns, I use two instrumental variable approaches. First, following Altonji and Card (1991) and Card (2001), I use the historical regional patterns of settlement of undocumented immigrants from a source country to predict the subsequent flow of undocumented immigrants from the source country into a state. This approach, although widely used in immigration literature, has been argued to not satisfy exclusion restriction condition for a valid instrument because factors that pull migrants into a particular destination in the past may be correlated with the crime rate in the destination today (Chalfin, 2013). Therefore, in the spirit of Angrist and Kugler (2003) and Llull (2017) who used push factors (e.g. wars or economic conditions in the source country) to identify the wage effect of immigration, I create a similar instrument based on active conflict events in Mexico that become prevalent after 2000.

2 Indeed, a study by Comino *et al.* (2016) suggests that undocumented immigrants are almost four times less likely to report crimes compared to US natives.

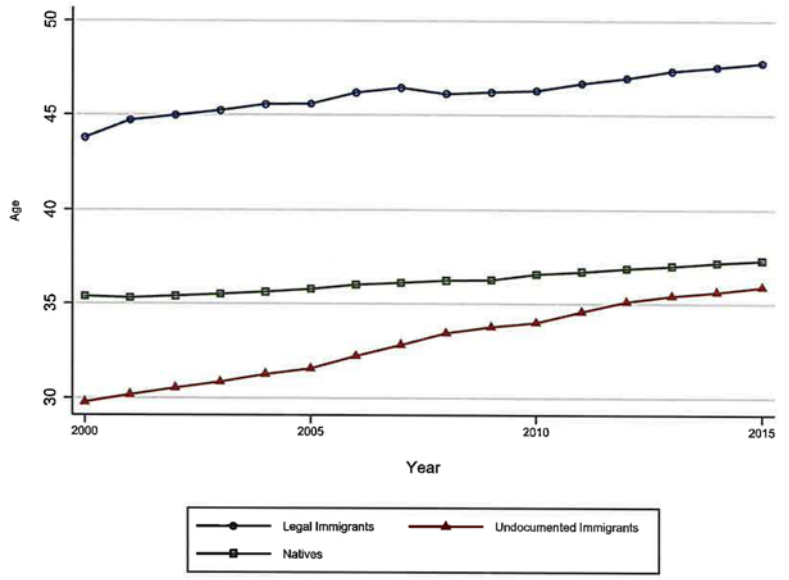

Fig. 2. Average age by residency status.
Source: Author's calculation from IPUMS 5% 2000 Census and American Community Survey (ACS) 2001–2015. The graph shows the average age of each residency status group over the time period in the analysis. Sampling weight is used in the analysis.

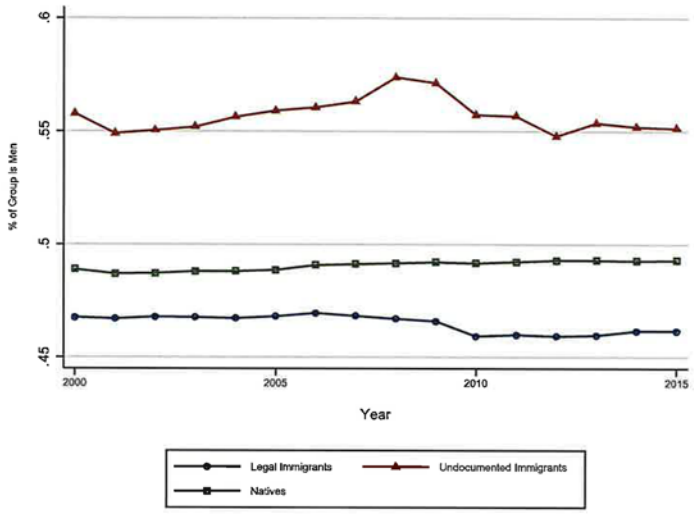

Fig. 3. Share of men by residency status.
Source: Author's calculation from IPUMS 5% 2000 Census and American Community Survey (ACS) 2001–2015. The graph shows the share of men in each residency status group over the time period in the analysis. Sampling weight is used in the analysis.

The results from both approaches show that the overall property and violent crime rates across US states are not significantly increased by undocumented immigration.

This article contributes to the literature that examines whether immigrants are more likely to commit a crime compared to US natives. So far, this literature has focused on the average immigrant, without differentiating their legal status (e.g. Butcher and Piehl, 1998b; Moehling and Piehl, 2009). However, it is unlikely that undocumented immigrants are equally likely as legal immigrants to participate in crime considering that their unauthorized status would limit their opportunities in the job market. Indeed, a few recent studies have documented that granting legal status to unauthorized immigrants lowers their probability of committing a crime in Europe (Mastrobuoni and Pinotti, 2015; Pinotti, 2017). A study closely related to this article is the CATO Institute Research and Policy Brief by Landgrave and Nowrasteh (2017), which also examines the difference in institutionalization rate between undocumented immigrants and other US residents. However, their illegal status imputation method that does not exclude some high-skilled foreign-born individuals employed in licensed occupations from the undocumented immigrant pool might lead to an overestimation in the gap of institutionalization rate between undocumented immigrants and natives.[3] Furthermore, their analysis did not test whether this gap is statistically significant, nor did it take into account the difference in background characteristics between undocumented immigrants and other US residents. This article extends the work of Landgrave and Nowrasteh (2017) by improving on the methodology and by examining whether assimilation, age at arrival, and arrival cohort play a role in determining the institutionalization rate of undocumented immigrants.

This article also contributes to the literature that examines the nexus of immigration and crime at an aggregate level. The literature has mainly focused on the overall immigration thus far (e.g. Butcher and Piehl, 1998a; Bianchi *et al.*, 2012; Bell *et al.*, 2013). However, the interest of recent policy debates has been mainly on undocumented immigration, which is the focus of this article. Recently, a few studies have documented that legalization policies lead to a reduction in the crime rates (Baker, 2015; Fasani, 2018). It is natural to generalize the findings of these studies as implying that an increase in the share of undocumented immigrants in the population would increase crime rates. However, it is worth noting that the effect of an increase in the share of undocumented immigrants in the population on crime rates may not necessarily be symmetric to the effect of lowering it through legalization policy. For example, following the passage of legalization policy in the USA (IRCA), applicants who were convicted of a felony or three misdemeanors during the 18-month probationary period would be removed from the programmes, exerting an incentive to not commit crime among the would-be legal immigrants during this period. It is plausible that this effect would be long-lasting if the applicants believe that their status could be revoked long after the probationary period. Therefore, although a decrease in the share of the undocumented population caused by legalization policies reduces crime rates, a rise in the share of the unauthorized population may not necessarily increase it.[4] It is also possible that undocumented immigrants who were legalized due to the IRCA have a

3 Landgrave and Nowrasteh (2017) found that undocumented immigrants are 44% less likely to be institutionalized than US natives.

4 In a different context to this study, recent works by Clemens *et al.* (2018) and Lee *et al.* (2017) have also argued that the wage and employment effects of an increase in the population of migrants are not as symmetric as reducing it through immigration restriction policy.

different propensity to commit crimes compared to those who arrived after the IRCA, suggesting the need to examine how crime rates respond to the movement of undocumented immigrants in other settings.

Another study closely related to this article is by Light and Miller (2018), who also examine the effect of undocumented immigration on violent crimes in the US context. Conducted independently and at the same time as this article, the authors found no evidence that undocumented immigration increases violent crime rates across the United States. This article complements the findings of Light and Miller (2018) in two ways. First, I analyse the association between undocumented immigration and crime using an instrument that does not rely on the past settlement of immigrants, which has been shown to be unlikely to meet the exclusion restriction requirement for a valid IV (Jaeger *et al.*, 2018). Second, I examine whether undocumented immigration is associated with an increase in property crime rates across the United States. I believe investigating this is important, especially because undocumented immigrants have fewer employment opportunities compared to legal immigrants, caused by their illegal status, making them more vulnerable to committing crimes for financial gains rather than 'crimes of passion' (violent crimes).[5]

The remainder of the article is organized as follows. Section 2 describes the procedure used to impute undocumented status in census data. Section 3 examines the institutionalization rate of undocumented immigrants compared to other US residents. Section 4 analyses the impact of undocumented immigration on crime rates in the United States. Section 5 concludes.

2. Imputing undocumented status in census data

The US Census Bureau does not ask the immigration status of the respondent. However, it is possible to impute the legal status of immigrants based on the characteristics of the respondent. For example, since a large share of undocumented immigrants come from Mexico and Central America, Orrenius and Zavodny (2016) defined those who are unauthorized as 'immigrants aged 20–54 who have at most completed high school, are from Mexico and Central America, and are not US citizens'. Similarly, Passel and Cohn (2014) from the Pew Research Center creates a methodology to impute undocumented status in American Community Survey (ACS) and CPS Annual Social and Economic Supplement data set. The ACS and CPS data set with imputed undocumented status from Passel and Cohn (2014) are not publicly available. However, Borjas (2017a, 2017b) was granted access to the 2012–2013 CPS ASEC files that Passel and Cohn (2014) constructed, and the author suggested that it may be possible to 'reverse engineer' the method that was used to create the imputed undocumented status based on a number of characteristic variables available in the census microdata. Following Borjas (2017a, 2017b), I classify a foreign-born person as a legal immigrant if any of the following conditions holds:

a. that person arrived before 1980;
b. that person is a citizen;
c. that person receives welfare benefits such as Social Security, SSI, Medicaid, Medicare, or military insurance;
d. that person is a veteran or is currently in the Armed Forces;
e. that person works in the government sector;

5 For example, Bell *et al.* (2013) found that the wave of asylum seekers in the UK increase the property crime rates. The authors attributed this result to the limited labour market opportunities among the asylum seekers.

f. that person resides in public housing or receive rental subsidies, or that person is a spouse of someone who resides in public housing or receive rental subsidies;[6]
g. that person was born in Cuba;
h. that person's occupation requires some form of licensing;
i. that person's spouse is a legal immigrant or citizen.

All other foreign-born persons who are not classified as legal immigrants are assumed to be undocumented. It is worth noting that this method may overestimate the population of undocumented immigrants because some of the legal immigrants are classified as undocumented. For example, a person on a temporary student visa or high-skilled (H1-B) visa holder who is employed in an occupation that does not require a license would be classified as an undocumented immigrant. Assuming that a highly educated person is less likely to commit a crime, then this misclassification would cause the estimates to be biased in the direction that shows that the crime rates are negatively associated with undocumented immigration, and undocumented immigrants are less likely to be institutionalized relative to other US residents. However, we can do a simple back of the envelope calculation to estimate an approximate number of how many legal immigrants are misclassified as undocumented. For example, Ruiz (2014) estimated that there are about 450,000 foreign students in the USA in 2008. At the same time, Kerr and Lincoln (2010) used the demographic model from Lowell (2000) and estimated that there is approximately 500,000H-1B visa holder in the USA between 2002 and 2008. As there are about 12.3 million individuals classified as undocumented from the ACS in 2008, assuming that the misclassification error is similar to its level in 2008, this would lead to approximately 7.7% of legal residents classified as undocumented in the sample.[7] Although this misclassification error might be arguably small to substantially affect the estimates, it should be noted that the estimates might be biased in the direction that shows that the crime rates are negatively associated with undocumented immigration, and undocumented immigrants are less likely to be institutionalized relative to other US residents. It is also possible that this method may classify some undocumented immigrants as legal migrants. For example, the share of Hispanics in the sample is approximately 50%, while according Pew Research Center estimate, more than half of undocumented immigrants are Hispanics.[8] The direction of the bias in the estimates caused by this misclassification error would likely depend on the propensity to commit crimes of undocumented Hispanics misclassified as legal migrants compared to that of the other US residents.[9] Figure 4 shows the share of a foreign-born person classified as undocumented in the sample from 2000 to 2015. The share of undocumented people is relatively stable at approximately 4% of the population, while the share of legal immigrants is slightly increased from 7% to 9%.

6 Because there is no information on whether someone resides in public housing or receives rental subsidies in IPUMS 5% 2000 Census or ACS, similar to Borjas (2017a), I did not apply this condition to impute undocumented status.

7 Note that in this simple calculation, I did not take into account some H1-B visa holders who are employed in licensed occupations. Therefore, it is likely that this estimate of misclassification error is lower.

8 http://www.pewresearch.org/fact-tank/2017/04/27/5-facts-about-illegal-immigration-in-the-u-s/.

9 Studies have found that the propensity to be involved in crimes among Hispanics falls between non-Hispanic whites and blacks, and almost all of this differential between Hispanics and non-Hispanic whites can be explained by the differences in their social and economic characteristics (Martinez, 1997; Phillips, 2002).

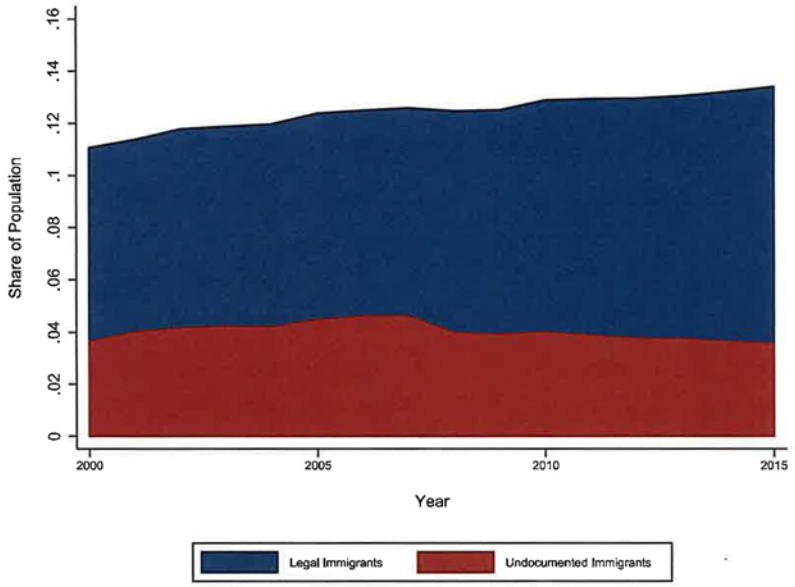

Fig. 4. Share of undocumented immigrants as % of total foreign-born.
Source: Author's calculation from IPUMS 5% Census 2000 and American Community Survey (ACS) 2001–2015. The graph shows the share of undocumented immigrant in the population over the time period in the analysis. Sampling weight is used in the analysis.

3. The incarceration rate of undocumented immigrants

3.1 Relative incarceration rate

Now, I examine the incarceration rate of undocumented immigrants compared to other US residents by using the ACS data from 2006 to 2015 available from IPUMS Ruggles *et al.* (2015). Although the ACS data allows me to identify a respondent if he/she lives in an institution (i.e. correctional facilities, mental hospitals, and homes for the aged), it does not separately identify if an individual is in a correctional facility. To address this, I follow Butcher and Piehl's (1998b) approach that limits the sample only to men between 18- and 40-years-old. By using the Census 1980, which was the last census from which we can separately identify individuals in different institutional settings, Butcher and Piehl (1998b) shows that the vast majority of institutionalized men between 18- and 40-years-old are indeed in correctional facilities, and their results are not substantially affected when the institutionalization rate is used in the analysis.[10]

10 Similarly, Borjas *et al.* (2010) use institutionalization rate as a proxy for incarceration rate to examine the increase in incarceration rate of African American following immigrant supply shock. There is a concern that the Census Bureau in many cases imputed the information on citizenship question for individuals who live in an institution, leading to a bias in the institutionalization rate of immigrants (Vaughan and Camarota, 2009). However, as noted by Vaughan and Camarota (2009), the Census Bureau imputed the citizenship question for individuals living in an institution only 6% of the time in ACS, suggesting the reliability of the data. It is also worth noting that this issue only applies to individuals living in an institution and the overall Census does not suffer from this problem (Vaughan and Camarota, 2009).

Table 1. Descriptive statistics

	All	US Natives	Legal Immigrant	Undocumented Immigrant
Share of All Individuals	–	0.83	0.09	0.09
Years in the US	–	–	15.28	9.47
Institutionalization Rate	0.03	0.03	0.01	0.02
Age	28.73	28.36	31.22	29.73
Race:				
Asian	0.06	0.02	0.27	0.16
White	0.71	0.76	0.44	0.48
Black	0.13	0.14	0.10	0.05
Hispanic Origin	0.16	0.11	0.29	0.50
Education:				
High School Dropout	0.15	0.12	0.20	0.42
High School Graduates	0.30	0.31	0.24	0.26
College	0.55	0.57	0.56	0.32
Observations	4,242,790	3,605,670	335,715	301,405

Source: Author's calculations from ACS 2006–2015.
Notes: The table reports the summary statistics of men aged 18 to 40 by their residency status. ACS sampling weight is used in the analysis.

Table 1 shows the descriptive statistics for the sample when it is restricted to only men aged 18 to 40. In this sample, the share of individuals classified as legal immigrants is approximately similar to those classified as undocumented, which is capturing the fact that undocumented immigrants are more likely to be younger men, while legal immigrants are more likely to be female and older (Figs 2 and 3). A large share of those who are undocumented are of Hispanic origin (50%)—compared to 29% for legal immigrants and 11% for US natives. Similarly to the full sample, undocumented immigrants in this restricted sample are overrepresented in high school dropouts category (42%) and underrepresented in college-educated category compared to other US residents.

Figure 5 shows the institutionalization rate by residency status of US population from 2006 to 2015. The institutionalization rate among all groups is relatively stable during this period. However, the institutionalization rate of undocumented immigrants (~2%) are smaller compared to US natives (~3%). This result implies that undocumented immigrants are approximately 33% less likely to be institutionalized compared to US natives, which might be surprising because undocumented immigrants have many characteristics that are usually associated with crime.[11] To formally test the difference of institutionalization rate between US natives and immigrants, I run the following simple regression:

11 There is a concern that undocumented immigrants might be deported prior to serving their sentences, which then leads to the underestimation of their institutionalization rate. However, the immigration statute (US Code Title 8 Chapter 12 Section 1231) does not allow aliens (i.e. legal or undocumented immigrants) who are sentenced to imprisonment to be removed (deported) from the country until he/she is released from imprisonment. An exception can be made only in a case in which an alien was convicted of a non-violent offence and the appropriate official (the attorney general or the chief of the state prison system) requests early removal after considering if such action is appropriate and in the best interest of the United States. At the same time, it is possible for institutionalization rate of

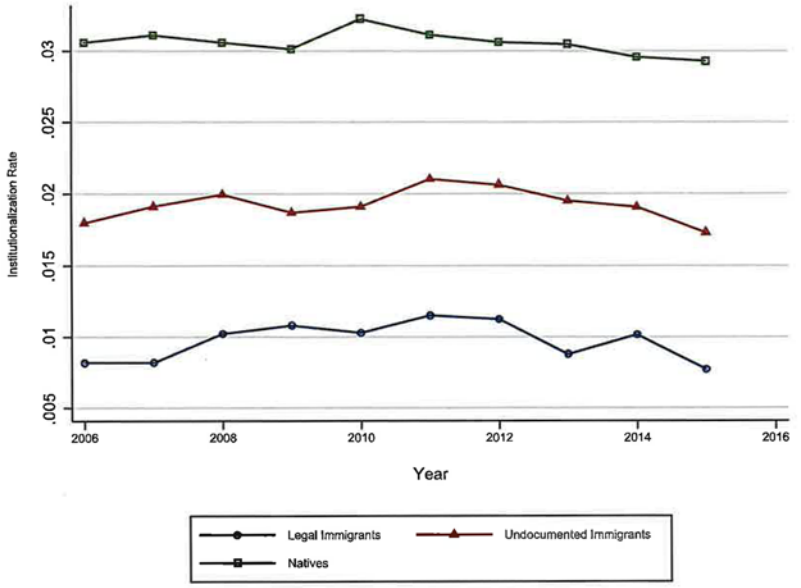

Fig. 5. Institutionalization rate by residency status.
Source: Author's calculations from American Community Survey 2006–2015. The graph shows institutionalization rate of men aged 18 to 40 by their residency status. Sampling weight is used in the analysis.

$$y_{ist} = \gamma_0 + \alpha_1 U_{ist} + \mu_1 D_{ist} + X_{ist}\beta + \delta_{st} + \varepsilon_{ist} \quad (1)$$

where y_{ist} is an indicator variable that is equal to one if individual i in state s at time t is institutionalized and zero otherwise. U_{ist} and D_{ist} indicate undocumented and documented status, respectively. x_{ist} is a vector of individual characteristics such as age and education. δ_{st} is state-year fixed effects to absorb the differences in policing/enforcement environment or other unobservables that may vary across states over time.

Table 2 shows the results of the regression. Column (1) shows that the institutionalization rate gap that was observed in Fig. 5 is indeed statistically significant. The rest of the columns shows that the gap in institutionalization rate between US natives and undocumented immigrants widens with additional controls. After all the differences in background characteristics are controlled for, the estimates show that undocumented immigrants' institutionalization rate is slightly lower even when compared to those who are in the US legally, although this difference is not statistically significant with a p-value of 0.49.

The finding that institutionalization rate of undocumented immigrants is similar to legal immigrants is interesting, especially because recent studies have documented that granting legal status to unauthorized immigrants lowers their probability of committing a crime in Europe (Mastrobuoni and Pinotti, 2015; Pinotti, 2017). A concern is that I did not control for

undocumented immigrants to be inflated because immigrants under deportation order spent more time incarcerated for a given sentence than similar natives (Butcher and Piehl, 2000). This may be caused by administrative delays or backlogs in the system that resulted in more immigrants waiting in prisons while awaiting their deportation process to be cleared.

Table 2. Estimates for institutionalization rate

	(1)	(2)	(3)	(4)	(5)
Undocumented Immigrant	−0.011	−0.013	−0.028	−0.026	−0.027
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Legal Immigrant	−0.021	−0.022	−0.023	−0.025	−0.026
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Controls:					
Age	No	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes
Race	No	No	No	Yes	Yes
State × Years FE	No	No	No	No	Yes
Observations	4,242,790				

Source: Author's calculations.
Notes: The table reports the difference in institutionalization rate relative to US natives among men aged 18 to 40. Clustered robust standard error at the state level in parentheses. *p*-value in the brackets. ACS sampling weight is used in the analysis.

Table 3. Institutionalization rate of undocumented immigrants relative to legal immigrants

	(1)	(2)
Undocumented Immigrant	0.006	0.009
	(0.002)	(0.002)
	[0.005]	[0.000]
Controls:		
Age	Yes	Yes
Education	Yes	Yes
Race	Yes	Yes
State × Years FE	Yes	Yes
Country of Origin	Yes	Yes
Years Since Migration	No	Yes
Observations	637,120	

Source: Author's calculations.
Notes: The table reports the difference in institutionalization rate relative to legal immigrants among men aged 18 to 40. Clustered robust standard error at the state level in parentheses. *p*-value in the brackets. ACS sampling weight is used in the analysis.

important variables that may play a role in determining the difference in institutionalization rate between legal and undocumented immigrants, such as years since migration and country of origin. To examine this, I re-ran the analysis using the sample of immigrants only and added years since migration and country of origin as additional controls. After controlling for years since migration and country of origin, the results showed that undocumented immigrants are more likely to be institutionalized compared to legal immigrants, suggesting that legal status is associated with a lower propensity to commit a crime among immigrants (Table 3).

3.2 Assimilation, age at arrival, and cohort effects

The number of years spent in the United States has been known to improve the earnings of immigrants as they obtain US-specific skills and information that natives have (Chiswick, 1978). Similarly, immigrants who arrive in the USA at a younger age are more likely to be successful in assimilating to the US labour market compared to those who arrive at an older age (Friedberg, 1992). Considering that individuals with poor labour market outcomes are more likely to commit a crime, it follows that we may expect that individuals who arrive at a younger age and have spent more time in the USA are less likely to be institutionalized. At the same time, it may take some time for immigrants to become accustomed to US norms and get involved in activities that result in institutionalization. It is possible, therefore, that immigrants who have been in the USA longer are very similar to US natives in their probability to be institutionalized.

To examine if assimilation and age at arrival have an effect on the relative institutionalization rate of undocumented immigrants, I exclude legal immigrants from the sample (so that the reference group is US natives) and estimate the following regression model in each of ACS cross sections:

$$y_{i\tau} = \gamma_{0,\tau} + \gamma_{1,\tau} X_{i\tau} + \gamma_{2,\tau} A_{i\tau} + \sum_{h} \gamma_{h,\tau} C_{h,i\tau} + \varepsilon_{i\tau} \tag{2}$$

where $y_{i\tau}$ is equal to one if individual i in cross section τ is institutionalized and zero otherwise. I consider 2007, 2011, and 2015 ACS cross-sections in the analysis. A represents age at arrival, and C_h is an indicator variable if an undocumented immigrant belongs to arrival cohort h (i.e. C_{90} is an indicator if year of immigration is on or before 1990, C_{91-94} is an indicator if year of immigration is from 1991 to 1994, C_{95-98} is an indicator if year of immigration is from 1995 to 1998, and so on). For US natives (the reference group), the age at arrival A and arrival cohort C_h are set to be equal to zero.[12] X is a set of control variables that includes education and current age. ε is the error term.

To examine whether undocumented immigrants are more likely to be institutionalized as they spend more time in the United States (i.e. the assimilation effect), we can take a look at the changes in the relative institutionalization rate of undocumented immigrants who belong to the same cohort between 2007 and 2015. In terms of the regression above, this can be computed as $\gamma_{h,2015} - \gamma_{h,2007}$. Furthermore, to see if there are changes in relative institutionalization rate across cohorts (e.g. recent cohorts might be more likely to be institutionalized compared to US natives compared to earlier cohorts), we can compare the relative institutionalization rate of a cohort at a given number of years since immigration to the relative institutionalization rate of earlier cohorts at that same number of years since migration. For example, we can compare 2011–2014 cohort in 2015 with 2003–2006 cohort in 2007 because they have been in the USA for a similar number of years (one to four years). In terms of the coefficients above, this cohort effect can be computed as $\gamma_{11-14,2015} - \gamma_{03-06,2007}$.

Table 4 shows the effect of assimilation and age at arrival effects on relative institutionalization rate of undocumented immigrants. The estimates suggest that undocumented immigrants who arrived a year younger are 0.1 percentage points more likely to be institutionalized. For illustrative purpose, consider the institutionalization rate among undocumented immigrants who

12 There are some cases in which age at arrival has a value of −1 or 0, which may imply that the individual had not had his/her first birthday when immigrating into the United States. For these individuals, I assign a value of one for their age at arrival.

Table 4. Assimilation and age at arrival effects (undocumented immigrants)

	2007 ACS	2011 ACS	2015 ACS	Assimilation Effects (2007–2015)
1991 to 1994 Cohort	−0.015	−0.019	−0.018	−0.003
	(0.010)	(0.009)	(0.007)	[0.765]
	[0.179]	[0.055]	[0.026]	
1995 to 1998 Cohort	−0.019	−0.021	−0.022	−0.003
	(0.011)	(0.009)	(0.008)	[0.770]
	[0.123]	[0.061]	[0.016]	
1999 to 2002 Cohort	−0.018	−0.017	−0.019	−0.001
	(0.011)	(0.010)	(0.008)	[0.934]
	[0.170]	[0.113]	[0.041]	
2003 to 2006 Cohort	−0.014	−0.016	−0.020	−0.005
	(0.012)	(0.010)	(0.009)	[0.697]
	[0.295]	[0.153]	[0.051]	
2007 to 2010 Cohort		−0.003	−0.002	
		(0.011)	(0.009)	
		[0.820]	[0.803]	
2011 to 2014 Cohort			0.013	
			(0.010)	
			[0.217]	
Age at Arrival	−0.001	−0.001	−0.001	
	(0.000)	(0.000)	(0.000)	
	[0.075]	[0.094]	[0.016]	

Source: Author's calculations.

Notes: The table reports the difference in institutionalization rate of undocumented immigrants for a given cohort relative to US natives among men aged 18 to 40. 'Assimilation Effects' column reports the within-cohort difference in relative institutionalization rate of undocumented immigrants in 2015 with its level in 2007. The regressions control for education and age. Clustered robust standard error at the cohort level in parentheses. p-value in the brackets. The p-value in assimilation effects column is based on the hypothesis testing that the difference in the estimate between the third and the first column is equal to zero. ACS sampling weight is used in the analysis.

arrive when they are nine-years-old. Since 3.2% of undocumented immigrants who arrive when they are nine-years-old are institutionalized in the sample, the finding suggests that if this group arrives in the USA a year older, holding all else constant, their institutionalization rate would be lower by approximately 3%. The assimilation effects are negative but statistically insignificant, suggesting that the institutionalization rate of undocumented immigrants is not converging to that of US natives as they spend more time in the United States. It should be noted that these estimates may underestimate the assimilation effect because deportation may remove undocumented immigrants who are more likely to be institutionalized over time. For example, undocumented immigrants from the 2003–2006 cohort who are institutionalized in 2007 might be removed from the country by 2015, and because those who are deported are more likely to commit a crime, the relative institutionalization rate decreased for that cohort in 2015. To see if deportation could explain the results, I follow Butcher and Piehl's (2007) approach that examines the assimilation effect for naturalized citizens. Considering that naturalized citizens are not subject to deportation after committing a crime, a widening gap in the institutionalization rate between US natives and naturalized citizens over time would provide evidence

which suggests that the findings in Table 4 are not driven by deportation. Similar to Butcher and Piehl (2007), the results show a decline in the relative institutionalization rate among naturalized citizens, even for different cohorts and time periods that are considered in this study (Table 5), suggesting that deportation is unlikely to play a major role in explaining the assimilation effect estimates observed among undocumented immigrants in Table 4.

Borjas (1985) noted that the qualities of immigrants across cohorts are different—with earlier cohorts being more likely to be more successful in the US labour market compared to recent cohorts. Since an individual's labour market performance is correlated with his/her likelihood to commit a crime, we may expect to see a higher relative institutionalization rate among undocumented immigrants in the recent cohorts. Table 6 shows no robust evidence that recent cohorts are more likely to be institutionalized compared to older cohorts. The results show that undocumented immigrants in 2011–2014 cohort are more likely to be institutionalized by 2.8 percentage points compared to the 2003–2006 cohort, but it is not statistically significant at the conventional levels. Qualitatively similar results are observed for other cohort comparisons.

The analysis so far examines the assimilation effect of undocumented immigrants using natives as the reference group. However, it is also possible to examine how the

Table 5. Assimilation and age at arrival effects (naturalized citizens)

	2007 ACS	2011 ACS	2015 ACS	Assimilation Effects (2007–2015)
1991 to 1994 Cohort	0.001	−0.011	−0.010	−0.011
	(0.004)	(0.002)	(0.002)	[0.034]
	[0.786]	[0.000]	[0.000]	
1995 to 1998 Cohort	0.003	−0.005	−0.007	−0.010
	(0.004)	(0.002)	(0.002)	[0.099]
	[0.529]	[0.040]	[0.005]	
1999 to 2002 Cohort	0.004	−0.004	−0.007	−0.011
	(0.005)	(0.003)	(0.003)	[0.113]
	[0.468]	[0.133]	[0.016]	
2003 to 2006 Cohort	0.015	−0.004	−0.003	−0.018
	(0.005)	(0.003)	(0.003)	[0.037]
	[0.032]	[0.259]	[0.405]	
2007 to 2010 Cohort		−0.001	0.000	
		(0.003)	(0.004)	
		[0.729]	[0.976]	
2011 to 2014 Cohort			0.000	
			(0.004)	
			[0.922]	
Age at Arrival	−0.001	−0.001	−0.001	
	(0.000)	(0.000)	(0.000)	
	[0.001]	[0.000]	[0.000]	

Source: Author's calculations.

Notes: The table reports the difference in institutionalization rate of naturalized citizens for a given cohort relative to US natives among men aged 18 to 40. 'Assimilation Effects' column reports the within-cohort difference in relative institutionalization rate of naturalized citizens in 2015 with its level in 2007. Clustered robust standard error at the cohort level in parentheses. The regressions control for education and age. p-value in the brackets. The p-value in assimilation effects column is based on the hypothesis testing that the difference in the estimate between the third and the first column is equal to zero. ACS sampling weight is used in the analysis.

Table 6. Cohort effects

	(1)
Arrival Cohort:	
2011–2014 (ACS 2015) vs 2003–2006 (ACS 2007)	0.028
	[0.103]
2007–2010 (ACS 2015) vs 1999–2002 (ACS 2007)	0.015
	[0.272]
2003–2006 (ACS 2015) vs 1995–1998 (ACS 2007)	0.000
	[0.980]
1999–2002 (ACS 2015) vs 1991–1994 (ACS 2007)	−0.004
	[0.721]

Source: Author's calculations.

Notes: The table reports across cohort difference in the relative institutionalization rate of undocumented immigrants among men aged 18 to 40. The regressions control for education and age. p-value in the brackets. The p-value is based on the hypothesis testing that the difference in the estimate between the recent cohort and the earlier cohort is equal to zero. ACS sampling weight is used in the analysis.

institutionalization rate of undocumented immigrant changes over time relative to that of legal immigrants. The result of this exercise is reported in Table 7. The evidence suggests that the institutionalization rate of the 2003–2006 cohort is converging toward that of legal immigrants. However, this is not the case for the earlier cohorts. Therefore, there is no robust evidence that the institutionalization rate of undocumented immigrants is converging toward that of legal migrants.

4 Undocumented immigration and crime rates

The results from the previous section show that undocumented immigrants are less likely to be institutionalized and thus provide a suggestive evidence that they are less likely to commit a crime compared to US natives. In this section, I examine whether an inflow of undocumented immigrants increases crime rates. As noted before, the impact of undocumented immigration on crime rates is ambiguous even though undocumented immigrants might be less likely to commit a crime at an individual level compared to US natives. For example, worsening labour market conditions have been shown to increase crime rates in the USA (Gould *et al.*, 2002). At the same time, immigration has been thought to worsen the labour market outcomes of natives (e.g. Borjas, 2003; Borjas *et al.*, 2010), although many studies argue that the labour market effect of immigration is relatively small (Card, 1990, 2005; Ottaviano and Peri, 2012). Recently, undocumented immigration has been suggested to spur job creation because the expected value of posting a vacancy is higher in a place where there is a larger share of undocumented immigrants (Chassamboulli and Peri, 2015). Therefore, the impact of undocumented immigration on crime rates depends not only on the likelihood to commit a crime among undocumented immigrants, but also on the resulted externalities that may induce or reduce crimes committed by other US residents.

To examine the impact of undocumented immigration on crime rates, I consider the following regression:

$$y_{st} = \gamma_1 + \beta_1 u_{st} + \delta_s + \delta_t + \delta_s t + e_{st} \tag{3}$$

where y_{st} is the crimes per 100,000 population in state s at time t. The violent and property crimes data are obtained from the FBI Uniform Crime Report. u_{st} is the share of

Table 7. Assimilation and age at arrival effect (legal immigrants as reference group)

	2007 ACS	2011 ACS	2015 ACS	Assimilation Effects (2007–2015)
1991 to 1994 Cohort	0.015	0.008	0.015	0.000
	(0.000)	(0.001)	(0.002)	[0.823]
	[0.000]	[0.000]	[0.000]	
1995 to 1998 Cohort	0.007	0.004	0.006	−0.001
	(0.000)	(0.001)	(0.001)	[0.492]
	[0.000]	[0.002]	[0.002]	
1999 to 2002 Cohort	0.005	0.004	0.004	−0.001
	(0.001)	(0.001)	(0.001)	[0.486]
	[0.000]	[0.003]	[0.020]	
2003 to 2006 Cohort	0.005	0.001	−0.001	−0.006
	(0.001)	(0.001)	(0.001)	[0.014]
	[0.004]	[0.340]	[0.659]	
2007 to 2010 Cohort		0.005	0.003	
		(0.001)	(0.001)	
		[0.005]	[0.037]	
2011 to 2014 Cohort			0.006	
			(0.001)	
			[0.000]	
Age at Arrival	−0.001	0.000	0.000	
	(0.000)	(0.000)	(0.000)	
	[0.005]	[0.001]	[0.000]	

Source: Author's calculations.

Notes: The table reports the difference in institutionalization rate of undocumented immigrants for a given cohort relative to legal immigrants among men aged 18 to 40. 'Assimilation Effects' column reports the within-cohort difference in relative institutionalization rate of undocumented immigrants in 2015 with its level in 2007. The regressions control for education and age. Clustered robust standard error at the cohort level in parentheses. p-value in the brackets. ACS sampling weight is used in the analysis.

undocumented immigrants in the population in state s at time t. It is obtained by aggregating undocumented individuals at the state level and then dividing it with total population in the state. δ_s and δ_t are state and year fixed effects, while δ_{st} is the state time trend to control for state-specific factors that vary (linearly) over time. For this analysis, I use IPUMS 5% 2000 Census and ACS data from 2001 to 2015, yielding 816 annual observations from 2000 to 2015. I conduct the analysis at the state level to minimize bias caused by measurement error and the response of other US residents to the inflow of undocumented immigrants.[13] The coefficient of interest is β_1, which measures the change in crime rates associated with a 1% point increase in the share of undocumented immigrants.[14]

13 For example, it may be harder for US natives to move to other states than to move to another city within a state following an inflow undocumented immigrants. Borjas (2006) and Aydemir and Borjas (2011) outline these problems of measurement error and native internal migration in the context of wage effect of immigration.

14 For ease of interpretation, the regression coefficient β_1 has been divided by 100, so that it shows the effect of one percentage point increase in the share of undocumented immigrants on crime rates.

As previously noted, estimating eq. (3) by a simple OLS regression may lead to biased estimate caused by the endogenous location of undocumented immigrants and systematic underreporting in a state where there is a higher share of undocumented immigrants. To minimize the bias, I follow Altonji and Card (1991) and Card (2001), who use the historical regional patterns of settlement of undocumented immigrants from a source country to predict the subsequent flow of undocumented immigrants from the source country into a state—which is usually known as 'network instrument'. This instrument relies on the tendency of new immigrants to settle in an area with a large share of immigrants from the same country (Bartel, 1989), possibly because of lower information costs associated with ethnic networks. Formally, the predicted share of undocumented immigrants can be written as:

$$\widehat{u_{st}} = \frac{\sum_{c=1}^{9} U_{ct} \times \frac{U_{cs,1990}}{U_{c,1990}}}{Population_{st}} \tag{4}$$

where U_{ct} is the total number of undocumented immigrants from country of origin c at time t.[15] $\frac{U_{cs,1990}}{U_{c,1990}}$ represents the share of undocumented immigrants from country c who were living in state s in 1990.

Although widely used in the literature, the network instrument has been argued to not satisfy exclusion restriction conditions for a valid instrument because factors that pull migrants into a particular destination in the past may be correlated with the crime rate in the destination today (Chalfin, 2013). Recently, the work by Jaeger *et al.* (2018) has argued that an instrument that relies on past settlement of immigrants is unlikely to be a valid IV in the context of wage effect of immigration because it conflates the short-term effect with the long-run adjustment effects (e.g. capital adjustment). In my case, if undocumented immigration leads to an adjustment that reduces crimes in the long-run, then the estimates obtained from using network instrument would be biased downward. In the light of these studies, there is a need to construct an instrument that does not rely on past settlement of immigrants. Therefore, in the spirit of Angrist and Kugler (2003) and Llull (2017) who used push factors (e.g. wars or economic conditions in the source country) to identify the wage effect of immigration, I create a similar instrument based on active conflict events in Mexico that become prevalent after 2000.[16] Figure 6 shows the casualties from active conflict events in Mexico in three time periods: 1990 to 1999, 2000 to 2009, and 2010 to 2015. Between 1990 and 1999, there were only two active conflicts events in the Mexican state of Chiapas, which took place as a protest to the NAFTA agreement in 1994, but continued in 1997 after peace talks stagnated. The conflicts spread to more Mexican states and became more intense after the Mexican government took a stronger stance against drug cartels beginning in the 2000s. The justification for the push instrument comes from the fact that the majority of undocumented immigrants come from Mexico (Rosenblum and Ruiz Soto, 2015), so that factors that push people to migrate away from Mexico (e.g. wars or conflicts) are expected to increase the share of undocumented immigrants in US states

15 Following Spenkuch (2013), I aggregate countries into nine groups: Northwestern Europe, Eastern Europe, Southern Europe, Asia, Mexico, South and Central America, Africa, Canada and all other countries.

16 The data for active conflict events come from UCDP/PRIO Armed Conflict Dataset (Gleditsch *et al.*, 2002). I define an active conflict event as an incident of lethal violence occurring at a given time and place that resulted in at least 25 deaths.

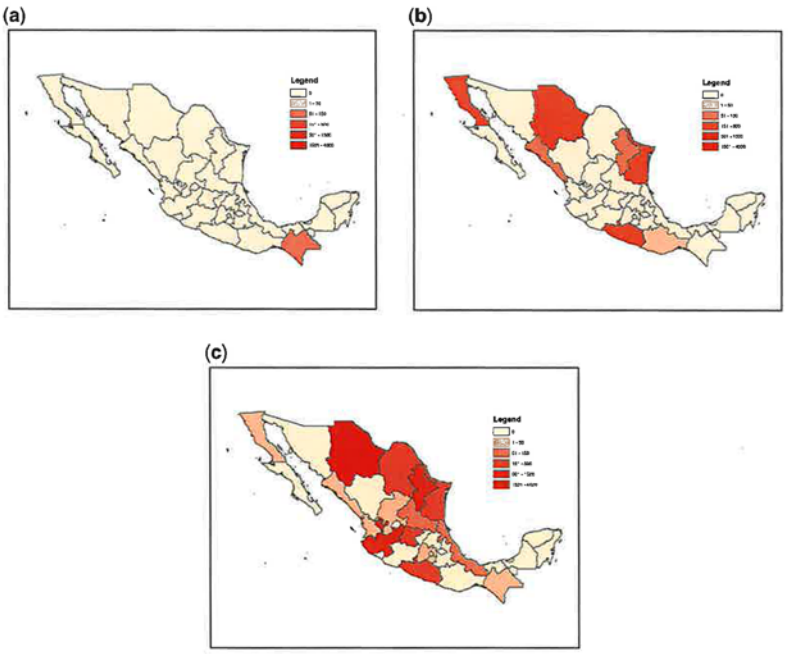

Fig. 6. Casualties from active conflict events in Mexico 1990–2014. (a) 1990–1999, (b) 2000–2009, (c) 2010–2014*
Source: Author's calculation from UCDP Georeferenced Event Data Set.

that are close to the place of conflict.[17] Formally, I formulate the push instrument as an index that assigns a higher value to the state that is predicted to receive a larger inflow of undocumented immigrants following conflicts in Mexico:

$$p_t^s = \sum_{m=1}^{32} \frac{conflict_{t-10,t}^m}{Max.conflict_{t-10.t}} \times \frac{Min.Dist.}{Dist_{sm}} \tag{5}$$

where p_t^s is the index value for state s at time t. $conflict_{t-10.t}^m$ is the casualties from all active conflict events between time t and a decade prior in Mexican state m $Max.conflict_{t-10.t}$ is the maximum number of casualties from all active conflicts between time t and a decade prior. Thus: $\frac{conflict_{t-10.t}^m}{Max.conflict_{t-10.t}}$ is a normalized conflict measure between time t and a decade prior in Mexican state m with a value between zero and one. $Dist_{sm}$ is the distance of

17 Distance has been known as a factor that mitigates the migration of people from their home country into the United States, probably due to larger moving costs and an individual's dislike of living far away from home. For example, Llull (2016) estimated that the stock of Mexican immigrants in the US would increase by a percentage three times larger than the percentage increase in the stock of Chinese immigrants following a $1,000 increase in US income *per capita*.

Table 8. First stage estimates

	Estimates	Robust F-Statistics	Observations
Network Instrument	0.441	66.12	816
	(0.054)		
	[0.000]		
Push Instrument	0.088	17.07	816
	(0.021)		
	[0.000]		

Source: Author's calculations.

Notes: State and year fixed effects along with state-specific time trends are included in the regressions. Regressions are weighted by state population. Clustered robust standard error in parentheses. *p*-value in the brackets.

Mexican state m with US state s.[18] Therefore, $\frac{Min.Dist.}{Dist_{sm}}$ is similar to inverse distance weight, but the value is normalized to be between zero and one by multiplying it with the closest distance between a US state and a Mexican state.[19] It follows that the variation in push instrument p_t^s across US states comes from the differences in distance from the place of conflict in Mexico. For ease of interpretation, I standardized the push instrument to have mean of zero and standard deviation of one.

Table 8 shows the first stage results of the two IV approaches. Both the network and push instrument are sufficiently strong with robust F-statistics of 66.12 and 17.07, respectively, which is well above the Staiger and Stock (1994) rule of thumb of 10. The interpretation of the push instrument is that a one standard deviation increase in the push index is expected to increase the share of undocumented immigrants by approximately 0.09 percentage points.

The results of the analysis are reported in Tables 9–11. Focusing on violent crime rate, I find no evidence that undocumented immigration increases the overall violent crime rate (Table 9). The estimates obtained from network instrument suggests that a 1% point increase in the share of undocumented immigrants in the population is associated with 5% decrease in overall violent crime rate, while the estimates obtained from push instrument are less precise, but nonetheless still show that undocumented immigration is not statistically significantly associated with an increase in violent crimes. In Table 10, I report the results of the analysis when violent crimes are disaggregated into murder, rape, robbery, and aggravated assault. Focusing on the estimates obtained through network instrument, I found that undocumented immigration is statistically significantly associated with an increase in murder rate but negatively associated with rape and aggravated assault. A 1% point increase in the share of undocumented immigrants in the population is associated with 5.9% increase in the murder rate, while it is associated with 7.1% and 8.5% decrease in rape and aggravated assault rates. None of the estimates obtained from the push instrument is statistically significant, except for robbery. A 1% point increase in the share of undocumented immigrants in the population is associated with 33.5 more robberies per

18 Distance between Mexican state m and US state s is measured by the distance between the most populated city in Mexican state m and the most populated city in US state s.

19 The closest distance between a US state and a Mexican state in the analysis is the distance between California and Baja California.

Table 9. The effect of undocumented immigration on overall crime rate

Type of Crime	OLS		IV Network Instrument		IV Push Instrument	
	Logs	Levels	Logs	Levels	Logs	Levels
Violent Crime	−0.006	−1.500	−0.050	−27.586	−0.015	0.210
	(0.011)	(6.264)	(0.011)	(4.695)	(0.083)	(40.514)
	[0.626]	[0.812]	[0.000]	[0.000]	[0.860]	[0.996]
Property Crime	0.007	34.437	−0.001	13.745	−0.086	−200.350
	(0.010)	(32.927)	(0.012)	(41.842)	(0.112)	(347.786)
	[0.440]	[0.301]	[0.965]	[0.743]	[0.440]	[0.565]
Observations	816		816		816	

Source: Author's calculations.

Notes: The table reports the result of least squares and IV regressions on overall crime rates. 'Logs' estimates report the effect of a 1% point increase in the share of undocumented immigrants on the log of number of crimes per 100,000 population. 'Levels' estimates report the effect of a 1% point increase in the share of undocumented immigrants on the number of crimes per 100,000 population. State and year fixed effects along with state-specific time trends are included in the regressions. Regressions are weighted by state population. Clustered standard error at the state level in parentheses. p-value in the brackets.

Table 10. The effect of undocumented immigration on violent crime rate

Type of Crime	OLS		IV Network Instrument		IV Push Instrument	
	Logs	Levels	Logs	Levels	Logs	Levels
Murder	0.031	0.236	0.059	0.367	0.011	0.053
	(0.018)	(0.119)	(0.018)	(0.100)	(0.081)	(0.509)
	[0.090]	[0.053]	[0.001]	[0.000]	[0.892]	[0.918]
Rape	−0.023	−0.658	−0.071	−2.297	0.064	0.992
	(0.014)	(0.470)	(0.012)	(0.333)	(0.123)	(2.856)
	[0.122]	[0.168]	[0.000]	[0.000]	[0.605]	[0.728]
Robbery	0.002	2.665	0.004	6.487	0.107	33.478
	(0.013)	(2.026)	(0.019)	(3.172)	(0.092)	(18.274)
	[0.877]	[0.194]	[0.830]	[0.041]	[0.246]	[0.067]
Aggravated Assault	−0.007	−3.874	−0.085	−32.866	−0.120	−34.567
	(0.018)	(6.299)	(0.017)	(4.942)	(0.137)	(42.630)
	[0.682]	[0.541]	[0.000]	[0.000]	[0.379]	[0.417]
Observations	816		816		816	

Source: Author's calculations.

Notes: The table reports the result of least squares and IV regressions on each type of violent crime. 'Logs' estimates report the effect of a 1% point increase in the share of undocumented immigrants on the log of number of crimes per 100,000 population. 'Levels' estimates report the effect of a 1% point increase in the share of undocumented immigrants on the number of crimes per 100,000 population. State and year fixed effects along with state-specific time trends are included in the regressions. Regressions are weighted by state population. Clustered standard error at the state level in parentheses. p-value in the brackets.

Table 11. The effect of undocumented immigration on property crime rate

Type of Crime	OLS		IV Network Instrument		IV Push Instrument	
	Logs	Levels	Logs	Levels	Logs	Levels
Burglary	0.006	9.868	−0.029	−20.240	−0.057	−4.650
	(0.011)	(8.898)	(0.011)	(10.196)	(0.175)	(141.003)
	[0.564]	[0.273]	[0.012]	[0.047]	[0.743]	[0.974]
Larceny	−0.008	−16.299	−0.018	−24.407	−0.108	−157.506
	(0.009)	(23.507)	(0.009)	(23.253)	(0.106)	(202.132)
	[0.364]	[0.491]	[0.045]	[0.294]	[0.310]	[0.436]
Motor Vehicle Theft	0.045	40.760	0.032	58.038	−0.059	−37.550
	(0.022)	(13.638)	(0.035)	(21.510)	(0.085)	(34.379)
	[0.047]	[0.004]	[0.359]	[0.007]	[0.485]	[0.275]
Observations	816		816		816	

Source: Author's calculations.

Notes: The table reports the result of least squares and IV regressions on each type of property crime. 'Logs' estimates report the effect of a 1% point increase in the share of undocumented immigrants on the log of number of crimes per 100,000 population. 'Levels' estimates report the effect of a 1% point increase in the share of undocumented immigrants on the number of crimes per 100,000 population. State and year fixed effects along with state-specific time trends are included in the regressions. Regressions are weighted by state population. Clustered standard error at the state level in parentheses. p-value in the brackets.

100,000 population. However, this result does not hold when the robbery rate is measured in logs. Taken as a whole, the overall evidence suggests a weak association between undocumented immigration and violent crimes, similar to the findings of Light and Miller (2018), who also examine the association between undocumented immigration and violent crimes using different methodology to this study.

Although there is a weak association between undocumented immigration and violent crimes, this result does not naturally extend to property crimes. Undocumented immigrants have more limited labour market opportunities compared to other US residents, mainly because of their illegal status. It is likely, therefore, that they are more vulnerable of committing crimes for financial gains, rather than 'crimes of passion' (violent crimes). To examine this, I repeat the analysis above on the property crime rates. Contrary to the expectation, the results of the analysis show no strong evidence linking undocumented immigration with property crimes. On the overall property crime rate, the result obtained from the push instrument suggest that a 1% point increase in the share of undocumented immigrants in the population lowers property crime rate by 8.6%, but this estimate is imprecisely estimated and is not significant at conventional levels (Table 9). The result from using past settlement of immigrants as instrument shows that undocumented immigration is not statistically significantly associated with overall property crime rate, corroborating the result obtained from the push instrument. Disaggregating property crimes into burglary, larceny, and motor vehicle theft, the results show that there is no robust evidence that undocumented immigration is statistically significantly associated with an increase in these crime rates (Table 11). If anything, the result obtained from using the network instrument as an IV suggests that a 1% point increase in the share of undocumented immigrants in the

population is associated with a decrease in burglary rates by 2.9% or 20 less burglaries per 100,000 population when the burglary rate is measured in level.

As a final note, there are some instances in which the estimates obtained from using the network instrument present contradictory findings as compared to the ones obtained through push instrument. For example, undocumented immigration is associated with a reduction in rape crime rate when network instrument is used in the analysis, while it is positive and statistically indistinguishable from zero when the push instrument is used. My preferred estimates are the ones obtained from using the push instrument, not only because it is constructed without relying on past settlement of immigrants, which has been shown to be unlikely to meet the exclusion restriction requirement for a valid IV, but also because it identifies the effect of an increase in the share of undocumented immigrants in the population that comes through the increase in unauthorized Mexican immigrants, which is the focus of recent policy debates. Nonetheless, the overall evidence from the analysis shows a weak link between undocumented immigration and crimes.

5 Conclusion

Crimes are costly for society. At the same time, there are concerns that an increase in undocumented immigration may lead to a sizable increase in the crime rates. In this article, I examine the institutionalization (incarceration) rate of undocumented immigrants and quantify the change in crime rates attributable to undocumented immigration.

The analysis yields a few main findings. First, despite possessing many characteristics usually associated with crime, undocumented immigrants are approximately 33% less likely to be institutionalized compared to US natives. Second, there is no evidence that undocumented immigrants who have spent more time in the USA are more likely to be institutionalized compared to those who have been in the USA for a shorter time. There is evidence, however, that arriving at a younger age is associated with a higher institutionalization rate. Finally, overall property and violent crime rates across US states are not statistically significantly increased by undocumented immigration.

Supplementary Material

Supplementary material is available on the OUP website. This comprises the replication files. The data used in the analysis is publicly available from IPUMS USA (https://usa.ipums.org/usa/) and FBI Uniform Crime Report (https://www.ucrdatatool.gov/). The UCR crime data for 2015 is available from https://ucr.fbi.gov/crime-in-the-u.s/2015/crime-in-the-u.s.-2015/tables/table-5. The UCDP/PRIO Armed Conflict Georeferenced Event Data Set is available from https://ucdp.uu.se/downloads/olddw.html.

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IMMIGRATION AND CRIME IN AN ERA OF TRANSFORMATION: A LONGITUDINAL ANALYSIS OF HOMICIDES IN SAN DIEGO NEIGHBORHOODS, 1980–2000*

RAMIRO MARTINEZ, JR.
Department of Criminal Justice
Florida International University

JACOB I. STOWELL
Department of Criminal Justice and Criminology
University of Massachusetts Lowell

MATTHEW T. LEE
Department of Sociology
University of Akron

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Emerging research associated with the "immigration revitalization" perspective suggests that immigration has been labeled inaccurately as a cause of crime in contemporary society. In fact, crime seems to be unexpectedly low in many communities that exhibit high levels of the following classic indicators of social disorganization: residential instability, ethnic heterogeneity, and immigration. But virtually all research conducted to date has been cross-sectional in nature and therefore unable to demonstrate how the relationship between immigration and crime might covary over time. This limitation is significant, especially because current versions of social disorganization theory posit a dynamic relationship between structural factors and crime that unfolds over time. The current study addresses this issue by exploring the effects of immigration on neighborhood-level homicide trends in the city of San Diego, California, using a combination of racially/ethnically disaggregated homicide victim data and community structural indicators collected for three decennial census periods. Consistent with the revitalization thesis, results show that the increased size of the foreign-born population reduces lethal violence over time. Specifically, we find that neighborhoods with a larger share of immigrants have fewer total, non-Latino White, and Latino homicide victims. More broadly, our findings suggest that social disorganization in heavily immigrant cities might be largely a function of economic deprivation rather than forms of "neighborhood" or "system" stability.

Almost 70 years ago, Shaw and McKay (1969 [1942]) vividly illustrated the residential settlement patterns of new immigrants. Through their research, they provided a detailed picture of the role immigration played in shaping juvenile crime. For example, Shaw and McKay observed how immigrants settled into high-crime communities in part because of inexpensive housing and restricted access to economic resources available to newcomers. Through absorption into the occupational structure of the city and assimilation into mainstream society, immigrants[1] gradually moved into areas of lower crime and higher economic affluence (Bursik, 2006). The larger significance of Shaw and McKay's observation was that most immigrant groups initially settled into socially disrupted, high-crime communities and often were perceived incorrectly as a disproportionately crime-prone population. They argued that any direct connection between particular groups of immigrants and crime was largely spurious (Bursik, 2006: 24). Instead, they identified the causes of crime as corresponding to neighborhood structural properties, such as residential turnover and ethnic heterogeneity, both of which might be brought about by immigration

1. The notable exception was the rural migration of Blacks/African Americans.

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as a social process but have nothing to do with the characteristics of immigrants themselves.

In other words, immigrants were not biologically predisposed to crime and their cultural traditions did not necessarily promote crime, as other criminologists of that era had argued. Decades later, most ecologically minded scholars agree that it is the structural characteristics identified by Shaw and McKay that produce socially disorganized communities by weakening intervening mechanisms of social control. Although politicians and public commentators continue to exploit stereotypes that essentialize crime as a property of immigrants themselves, or "the poor" more generally, the disorganization perspective focuses on *types of places* rather than on *types of people* (Lee, 2003: 11).

It turns out that both kinds of explanation might require revision, at least for cities transformed by recent waves of immigration. This change is suggested by arguments that the recent homicide surge in some cities reflects their low levels of immigration, whereas immigrant destination cities have not experienced this increase (see Dale, 2007). More generally, few serious scholars today see crime as an inherent characteristic of immigrant groups. Moreover, based on empirical research findings, it stands to reason that some neighborhood factors might not disrupt communities as the initial theoretical logic suggests.

This is the question being asked by researchers associated with the emerging "immigration revitalization" perspective (Lee and Martinez, 2002: 365; see also Lee and Martinez, 2009; Sampson, 2008; Sampson, Morenoff, and Raudenbush, 2005). These scholars are showing that immigration, ethnic heterogeneity, and residential instability often are associated with reduced crime rates. This trend is partly because crime rates today are often highest in areas of concentrated disadvantage, which is associated with the most segregated and racially/ethnically homogeneous neighborhoods in large cities (Sampson and Wilson, 1995). But positive social change is also a major factor. Researchers in a variety of other fields, such as health, mental health, and education, have spent years marveling at the "immigrant paradox" of high poverty combined with better-than-expected life outcomes (cf. Rumbaut, 1999). But the discipline of criminology is just beginning to appreciate the myriad ways in which immigration in the modern era paradoxically has strengthened institutions of social control, fostered economic development, and sparked a revival of previously high-crime, inner-city neighborhoods.

The present study is designed in part as a response to recent calls by scholars to address these specific limitations in the extant criminological literature (see Bursik, 2006; Martinez, 2002; Sampson and Bean, 2006; Stowell, 2007). But the point of the article is not simply to add a longitudinal analysis involving racially/ethnically disaggregated data to the existing

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body of cross-sectional research, as important as that might be for assessing the immigration revitalization perspective. Instead, we use our unique longitudinal analysis to advance the communities and crime field by exploring a series of hypotheses drawn from the immigration and social disorganization literatures that shed new light on the processes behind ecological variations in crime. We no longer can assume contemporary communities are fundamentally similar to those studied by Shaw and McKay (1969 [1942]) and other founders of the social disorganization approach working in the early and middle part of the twentieth century. In one sense, our findings serve as a partial corrective to one aspect of this perspective. We argue that although the processes of social disorganization remain important, some factors producing it have changed with respect to immigration.

The stream of immigrants currently reshaping the United States is no longer primarily of European origin. The racial/ethnic/immigrant composition of many neighborhoods has grown increasingly diverse, and the Mexican border has supplanted Ellis Island as the predominant entry point into the nation. Although this transformation is well documented by sociologists and historians, we still know very little about how immigration impacts social disorganization and, in turn, about levels of violent crime in a contemporary social context. Moreover, research has yet to quantify whether the impact of shifting levels of immigration over time disrupts border communities as many politicians and pundits speculate (for a review, see Martinez, 2002: 1–31). Admittedly, this matter is not simple, as even Shaw and McKay's (1969 [1942]) multifaceted theory of crime was developed inductively from their contextually specific data. As such, we recognize that some exceptions to the general theoretical premises might exist because "local conditions eclipse broad theoretical predictions" (Martinez, Lee, and Nielsen, 2004: 152). By acknowledging this principle, our attempt to assess the impact of immigration or other social processes of disorganization on crime must also consider the "outcomes in relation to a potentially changing local context" (Entwisle, 2007: 687; see also Nielsen, Lee, and Martinez, 2005 on the importance of local variations in the causes of crime). Therefore, our interpretations of the findings incorporate detailed knowledge of the research setting as well as an exploration of how these processes unfold over time.

To address these issues, the current study employs neighborhood-level data from the city of San Diego collected from 1980 to 2000. As a major immigrant destination, this research site has undergone an intense compositional transformation since 1980, making this border city an ideal *critical case* (Yin, 1994) for investigating the impact of the dynamic processes thought to be associated with social disorganization. The substantive focus of our analysis is on the consequence of changing levels of immigration on

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levels of community violence (as measured by homicide) over time. Using homicide data as a proxy for "crime" has its shortcomings. But we believe that the reliability of homicide data relative to other crimes, which are less comprehensively reported to police, coupled with the high level of public concern about this serious form of crime, justifies its selection as the dependent variable in this first neighborhood-level longitudinal, multivariate test of the immigration–crime relationship in the modern era.

LITERATURE REVIEW AND HYPOTHESES

SOCIAL DISORGANIZATION AND IMMIGRATION REVITALIZATION

One of Shaw and McKay's (1969 [1942]) contributions was directing attention to the consequences of distressed community conditions on levels of crime. They noted how variations in the rates of delinquency across nativity, nationality, or racial groups corresponded to the structural composition of communities. They also argued that when exposed to the same types of areas, boys of native parentage and boys of foreign parentage have similar rates of delinquency (Bursik, 2006). Because of this underlying logic, the direct effect of immigration was not a focus of the initial tests of the theory. Although social disorganization theory posits that "changes in the nationality composition of a population are not accompanied by appreciable changes in relative rates of delinquency," this assumed temporal influence has not yet been examined fully (Bursik, 2006: 26).

Existing studies have documented the importance of social structural factors in observed levels of criminal deviance. This finding is hardly controversial in the communities and crime literature (see Lee, Martinez, and Rosenfeld, 2001; Martinez, Stowell, and Cancino, 2008; Stowell, 2007). But social disorganization theory goes further, positing that crime is a "relatively constant" condition of a specific type of urban neighborhood, independent of the characteristics of its residents (Shaw and McKay, 1969 [1942]). In advancing this argument, disorganization theorists make two assumptions about neighborhood processes over time. The theory takes for granted the structural stability of such neighborhoods, which in turn suggests the relative intractability of their levels of criminal deviance over time. But the traditional assumption of ecological stability has been questioned in more recent studies of social disorganization (cf. Bursik, 1988; Bursik and Webb, 1982; Schuerman and Kobrin, 1986). The conclusion of this literature is that the stability, so carefully documented by Shaw and McKay in the first half of the twentieth century, has attenuated somewhat after World War II, a process Bursik (1984: 402) referred to as increased

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"ecological differentiation." Therefore, in this study, we test the following hypothesis:

Hypothesis 1 (H1): Ecological structures such as immigration, economic disadvantage, and residential stability will vary over time.

However, important work by Sampson and Morenoff (2006: 199) has demonstrated that, in the contemporary era, neighborhoods have remained "remarkably stable" in their *relative position* in economic, political, and social terms "despite the inflow and outflow of individual residents," at least in Chicago. That is, secular increases in the structural covariates of homicide do not necessarily translate into differential effects in multivariate models if communities maintain the same position relative to each other. Therefore, our assessment of H1 will include an examination of the extent to which nonsecular change occurred in a context of secular change.

Because longitudinal studies are costly and difficult to conduct, one of Bursik's (1988: 524) five criticisms of the social disorganization tradition was that current research conducted within that framework was based on cross-sectional data, which "must assume that local communities are not undergoing a redefinition of their role in the ecological system." In other words, this work must take for granted that communities do not change over time relative to each other or in terms of overall structural conditions. The existing cross-sectional tests of the relationship between immigration (and other structural conditions) and crime, although consistent with Shaw and McKay's (1969 [1942]) early specification of social disorganization theory, might require updating at a theoretical level, particularly if nonsecular changes have occurred in a given ecological setting.

Sorting through the mixed findings of this body of work is impossible because it is incapable of identifying where in the criminal "career" cycle a given community might be (see Schuerman and Kobrin, 1986). If we have indeed entered an era of structural (or "system") instability—a debatable contention about which more research clearly is required (see Sampson and Morenoff, 2006 for a promising approach; see also Peterson and Krivo, 2005)—then a valid quantitative assessment of social disorganization would have to model any changes in crime rates as they relate to possible changes in structural covariates over time. Following this logic, a longitudinal approach is the most appropriate test of this notion. The analytical methodology employed in the present study allows for just such comparisons. Specifically, our regression models focus on whether, and to what degree, temporal differences in structural and compositional factors within communities are associated with differences in levels of lethal violence. This approach is consistent with several arguments underpinning disorganization theory. As Bursik and Webb (1982: 28) pointed out, the

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dynamic changes associated with the "invasion and succession of new demographic groups is at the heart" of the ecological approach to crime.

Another basic assumption of classic disorganization theory is that various forms of *neighborhood* or *residential* instability (as opposed to the *ecological system* instability associated with nonsecular changes) weaken social control and cause crime. But in the current period of urban transformation, this begs the question, "What does residential 'stability' mean in an era of concentrated poverty in some areas and immigration revitalization in others?" We have good reasons to suspect that residential stability, often defined in terms of population turnover and the proportion of vacant buildings, will have different effects in the San Diego of today compared with the Chicago of yesterday. Shaw and McKay (1969 [1942]) were concerned with an especially destructive source of instability in the industrializing cities of the Midwest—the expansion of the industrial center that pushed into surrounding residential areas and caused neighborhood deterioration. But how should we conceive "instability" in an era when immigration often suppresses crime (cf. Lee and Martinez, 2009), fluctuating property values in some gentrifying urban neighborhoods encourage mobility as people sell their homes for profit or abandon them when prices fall, and concentrated poverty in homogenous, native-born, extremely disadvantaged areas seems to be driving urban crime rates? Unlike industrializing Chicago, large sections of San Diego transitioned from rural to suburban status during the time period of our study. Little reason exists to suspect this kind of change will increase crime rates, even though the "stability" of such areas (as measured by the commonly used proxy "length of time at current residence") is likely to be low.

To begin to reconceptualize the meaning of neighborhood stability and instability, we investigate the impact of two variables—independent of controls—that have been central to the communities and crime literature: immigration and residential instability. Both capture aspects of population turnover (i.e., in-migration and out-migration) that once were thought to contribute to disorganization. The connection with residential instability is clear, as it is sensible to assume that the movement of large numbers of people in and out of a community would disrupt networks of social control and increase crime. Immigration traditionally has been thought to increase disorganization as well because the "invasion" of immigrants into a community increases ethnic heterogeneity and poverty, introduces language barriers, and even might initiate a process of out-migration of established residents. In sum, residential instability is perceived as a barrier that makes it difficult for members of a neighborhood to agree on collective goals and effectively control the behavior of residents.

Indeed, some scholars have used the proportion of foreign born and the proportion of ethnic minorities as proxies for a "subculture" of "weakened

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social control" (cf. Schuerman and Kobrin, 1986: 71, 78). The very presence of such groups constitutes "a 'leading edge' of movement into emerging high-crime areas" (Schuerman and Kobrin, 1986: 78). In other words, when immigrants and ethnic minorities move into a neighborhood, disorganization and increased crime are the inevitable byproducts. But as we have discussed, this thesis has been challenged by the immigration revitalization perspective, a part of the more general "immigrant paradox" (Lee and Martinez, 2006: 91; Rumbaut, 1999) literature, which finds that immigration produces unexpectedly positive outcomes across a range of domains, including health, mental health, educational attainment, and crime.

It is likely that immigration and residential stability are related. Immigration might reduce stability, immigrants might settle in unstable areas, or both processes might occur simultaneously. We test the following bivariate relationship to clarify the extent to which immigration might be associated with instability over time:

Hypothesis 2 (H2): Immigration is associated with instability.

Because we examine this relationship over time, we can assess whether immigration precedes instability or whether immigrants are settling into already unstable areas. But our primary theoretical interest is the extent to which immigration and residential stability emerge as significant predictors of homicide in a multivariate longitudinal analysis. Drawing on the immigration revitalization perspective and our knowledge of San Diego, it is our contention that immigration will be associated with decreased homicide over time, controlling for residential stability and other structural covariates of homicide:

Hypothesis 3 (H3): Controlling for other factors, immigration decreases homicide.

HOMICIDE AND THE RACIAL/ETHNIC INVARIANCE HYPOTHESIS

To this point, our hypotheses have addressed important issues of stability and change over time and across neighborhoods that are central to the communities and crime literature, particularly with regard to social disorganization and immigration revitalization. We question whether ecological structures vary over time (H1) and the extent to which immigration is associated longitudinally with residential instability and homicide in the contemporary era (H2 and H3). But another important debate persists in the literature that must be engaged—the racial/ethnic invariance hypothesis. If structural sources of crime are similar for all groups, then it is unnecessary to test hypotheses for specific groups such as Latinos, non-Latino Whites, and non-Latino Blacks (Krivo and Peterson, 2000; McNulty,

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2001). As with other "major theories of crime" (Krivo and Peterson, 2000: 556), the social disorganization perspective argues that levels of neighborhood crime are not associated with the nativity or nationality characteristics of their inhabitants.

But previous research has provided mixed support for this notion. On the one hand, the weight of the empirical evidence documents the fact that community characteristics shape crime. On the other hand, it is less clear whether levels of crime endure regardless of the underlying community nativity, racial, and ethnic composition. Researchers comparing neighborhood levels of violent crime by nativity, race, and ethnicity have demonstrated both similarities and differences in the criminal victimization of specific groups and the covariates of victimization, depending on local context (Peterson and Krivo, 2005: 347). A recent authoritative review of the literature sided with the invariance hypothesis but noted that only two studies were methodologically sophisticated enough to test adequately the invariance hypothesis and concluded that the "state of knowledge needs substantial improvement" (Peterson and Krivo, 2005: 347).

Much of the previous work on racial invariance has focused on Black and White differences (Peterson and Krivo, 2005). But recent research indicates that immigrants, as well as Latinos and Asians, regardless of nativity status, fare better than expected on a host of social and economic factors, including violent crime rates, than native-born African Americans with similarly high levels of disadvantage (see Lee and Martinez, 2002, 2006; Martinez and Lee, 2000: 486; Sampson and Bean, 2006: 20). For example, of the three largest racial/ethnic groups in the United States, overall homicide rates are highest for Blacks, followed by Latinos, and then by Whites, and predictors of homicide have similarities and differences at the community level (Peterson and Krivo, 2005). This pattern has prompted some researchers to conclude "immigrants and Latinos are less violent, even more so when they live in concentrated immigrant areas" (Sampson and Bean, 2006: 21). In one recent study, Phillips (2002) found that structural conditions fully "explain" the gap between White and Latino homicide rates but only account for half of the difference between White and Black rates.

Most relevant to the present study are previous findings conducted in San Diego. The city of San Diego is a major entry point into the United States and is a border city with a large and impoverished Latino (heavily Mexican-origin) population comprising the largest racial/ethnic minority group in the city. Analyzing levels of lethal violence between the 1985–1995 period, researchers found that homicide among Latinos in San Diego generally follows the same pattern as among Blacks and Whites in terms of the predictive power of concentrated disadvantage (Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Martinez, Lee, and

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Nielsen, 2004). Yet Latinos experienced less violence and victimization than Blacks (and even Whites at times) given the relatively high levels of disadvantage (see also Peterson and Krivo, 2005). Furthermore, some predictors of Latino violence, especially community nativity composition, are unique, bringing in new issues that transcend race. Notably, the concentration of immigrants in San Diego also seems to tell a different story with respect to violence than the concentration of African Americans in cities beyond the Sunbelt (Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002). As with the other issues we have reviewed, much of this research is cross sectional, which hinders our ability to make temporal predictions in the present study. But taking all existing research into consideration, some of which focuses on immigration and some on Latinos, we agree with Krivo and Peterson (2000: 557) that "similarity of conditions" for different racial and ethnic groups "do not exist in the vast majority of places," including San Diego. Given the previous research, we expect that some factors (such as economic deprivation) will exhibit invariant effects on the homicide levels of different racial and ethnic groups, whereas other factors (such as immigration) are more likely to affect coethnics, such as Latinos, than African Americans. We therefore offer the following hypothesis:

Hypothesis 4 (H4): Effects on homicide of all structural factors except immigration are racially/ethnically invariant over time.

ACCOUNTING FOR TEMPORAL ORDER

Our first four hypotheses were motivated by theoretical concerns. But the problem of temporal ordering has also been a methodological theme of this article. We have argued in favor of a longitudinal approach that captures the dynamic processes at the heart of the neighborhood ecology of crime. Cross-sectional findings with regard to racial invariance or immigration might be confounded by the issue of time-ordering. Our over-time analysis seeks to overcome this limitation. But it is also important to determine the impact of immigration in racial/ethnic-specific models of changes in lethal violence, net of a given community's previous homicide levels. After all, it is possible that the negative immigration–crime relationship could be the result of immigrants moving into low-homicide areas. This effect might be especially prominent for Latinos in San Diego but less so for Blacks. We assess this aspect of the immigration–crime debate by including a temporally lagged count of homicides in our final set of total and group-specific models.

Hypothesis 5 (H5): The immigration effect remains robust despite the inclusion of a temporally lagged count of homicides.

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THE CURRENT SETTING: SAN DIEGO AND THE COMPLICATED STORY OF NEW IMMIGRATION

The 1980–2000 time period that we have selected for this study represents a strategic era of transformation in which to test our hypotheses because (as we will show) San Diego experienced a dramatic increase in the foreign-born population during this time. But unraveling the immigration–crime link in San Diego is complicated by the reality that the city's Mexican- and Asian-origin populations are a study in contrasts—both consist of old and new arrivals. Members of the two dominant immigrant groups also might reside in neighborhoods with varying levels of community disadvantage, affluence, and violence. Latinos outnumber Asians by a nearly 2:1 ratio, and Filipinos comprise nearly half of the Asian population (Lee and Martinez, 2006). With its historical origins in pre-1845 Mexico, Latinos originally moved in from various interior and border regions across Mexico, but after annexation, later arrivals also traveled from El Paso and other parts of the southwestern United States. In addition, migrants from Central and South America also moved through Mexico on their way to America. San Diego's much-maligned twin city Tijuana serves as the gateway through which a disproportionate number of border crossers (both legal and illegal) enter the United States (see Nevins, 2002).

Latinos are the largest ethnic minority group in the city, but migrants born abroad represent almost half of the Mexican-origin population (47 percent). Most professional positions are occupied by non-Latino Whites, whereas a smaller share of the professional employment is of Mexican or Asian origin. On the whole, the local Latino middle class is smaller and less influential than in other Latino immigrant destination cities such as San Antonio, El Paso, or Miami. So the "ethnic enclave" effect that has been hypothesized to buffer crime in some other immigrant destination cities is not as prominent in San Diego. Consider also that San Diego currently has only one Latino city council member and has not had a Latino mayor at least since California was annexed into the United States. This research setting therefore represents a more stringent test of the immigration revitalization perspective, again suggesting that it is well suited as a critical case study (Yin, 1994). Despite their underrepresentation in professional occupations and local politics, it is possible that over time, as the Latino and Asian populations in San Diego expanded, the concentration of immigrants might help to buffer neighborhoods against crime and delinquency (see Sampson and Bean, 2006). As Sampson (2008) pointed out, the beneficial effects of immigration as a social process reduce crime for nonimmigrant groups as well.

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DATA AND METHODS

In the current research, we analyze administrative neighborhood-level panel data for the three most recent decennial census years. These data are unique among neighborhood-level studies of immigration because they allow us to examine social processes over time, a subject that has generated much speculation but relatively little scholarship.

Data for the dependent variable—the number of homicide victims—were obtained from records maintained by the San Diego Police Department (SDPD) homicide investigation unit. Homicide information was gathered directly from the incident report written by the lead investigator on the case, including information about the location of the homicide, narratives, and other information about the incident. Each report was retrieved and copied by hand by the lead author at the SDPD and shipped back to the home office. The homicide files were read and coded by a trained staff of coders following a coding report used by other city-level homicide researchers (see Martinez, Stowell, and Cancino, 2008).

To maximize the stability of our regression estimates, the dependent variable for each panel year is the number of killings pooled during a 5-year period. Specifically, for each of the panel years, the dependent variable includes the count of homicides for the 2 years leading up to and after the given census. In other words, crime data for each of the time points under investigation are as follows: 1978–1982, 1988–1992, and 1998–2002. The use of pooled data is common in existing community-level research, primarily because the outcome of interest is a rare event (Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Martinez, Stowell, and Cancino, 2008; Stowell, 2007; see also Mears and Bhati, 2006: 21).

All homicides were geocoded and referenced using a San Diego base street map for the year 2000. The data on killings were aggregated into census tracts, thus permitting the matching of the *total number of homicides* and racial/ethnic specific (non-Latino White, non-Latino Black, and Latino) homicides to tract-level data compiled in the Neighborhood Change Data Base (NCDB) (GeoLytics, 2002). The NCDB contains data on population characteristics in addition to a variety of other social-structural correlates of lethal violence. This data source is particularly useful for longitudinal analyses because the neighborhood-level variables have been normalized to tract boundaries used in the 2000 Census, which avoids problems associated with boundary changes during the 20-year period being studied.[2]

2. A potential limitation to geocoding all incidents to 2000 streets is that changes to the city's street layout might produce disparities in the percent of homicides for which we can obtain geographic coordinates across the three time periods (see Paulsen and Robinson, 2004). This issue does not seem to be a problem in this

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Following the example of previous research on this topic, the independent variables are constructed using decennial census data. The variables to be included in the regression models include a series of social structural indicators informed by neighborhood-level examinations of social disorganization theory, and particularly the work of Sampson and colleagues (see Sampson and Morenoff, 2004; Sampson, Morenoff, and Raudenbush, 2005). In the current study, quantifying the impact of immigration on lethal violence is of most substantive interest. To obtain a more comprehensive understanding of the underlying processes, we will use a general measure of *immigration*, defined as the percent of the population born outside of the United States.

The measure of *economic disadvantage* used in the present study is an index that includes the following dimensions of community economic viability: percent of the population living in poverty, percent of families receiving public assistance, and percent of families with children younger than age 18 that are headed by females. It is necessary to control for levels of economic deprivation because neighborhood disadvantage has been shown to be a strong predictor of levels of criminal violence (see Lee, Martinez, and Rosenfeld, 2001; Sampson, Morenoff, and Raudenbush, 2005).

As discussed, social disorganization theory also highlights *residential stability* (or instability) as a factor that promotes (or undermines) networks of informal social control in communities. To account for neighborhood settlement patterns, we created a neighborhood stability index that includes the percent of households that are owner-occupied and the percent of persons who have lived in the same house for more than 5 years. Additionally, as a proxy for neighborhood affluence, we constructed a measure of *the percent of those aged 16 years and older employed in a professional or managerial capacity.* It is important to note that each of these occupational classifications primarily comprise non-service-sector jobs, including engineers, financial planners, lawyers, and scientists. We believe that it is important to include this measure, as previous research has documented the ability of neighborhood affluence to buffer against violent crime (see Sampson and Bean, 2006).

As a measure of the level of informal social control in neighborhoods, the models will include a measure of the relative presence of adults per child defined as the *ratio of adults* (persons aged 18 years and older) *to children* (persons aged 17 years and younger).

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case, however, as we had a successful match rate of around 95 percent for each of the three periods, a level well above the recommended acceptable "hit rate" of 85 percent (Ratcliffe, 2004).

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ESTIMATION PROCEDURE

The multivariate regression equations in the present study are estimated using a fixed-effects analytical procedure, a method permitting us to capitalize on the panel structure of the data. More specifically, the fixed-effects method allows us to introduce a temporal dimension by modeling the effect of change. Indeed, the fixed-effects estimation procedure focuses exclusively on within-unit changes. Following the description in Allison (2005: 93), we estimate the following regression equation:

$$\log\lambda_{it} = \mu_i + \beta x_{it} + \gamma z_i + \alpha_i \quad (1)$$

where $\log\lambda_{it}$ is the log-linear function of the dependent variables, μ_i is the regression intercept—essentially the average change in lethal violence across years—x_{it} represents the time-varying predictor variables, z_i represents factors that do not vary over time, and α_i is the control for unobserved fixed effects.

An attractive feature of the fixed-effects model is that it controls for all stable characteristics of neighborhoods without requiring all indicators to be included in the model. As Allison (2005: 2–3) contended, a benefit of being able to control for stable characteristics is that the fixed-effects procedure effectively "eliminates potentially large sources of bias," which are typically associated with model misspecification. The production of unbiased regression estimates, Johnson (1995: 1,068) concluded, "allows for stronger and more valid inferences about the effects than can be made in many alternative methods" (see also Argue, Johnson, and White, 1999).

The focus on within-unit changes also makes fixed-effects estimation attractive for substantive reasons. Specifically, the notion that levels of criminal violence in some areas are resistant to change is one key proposition advanced by social disorganization theory. Indeed, Shaw and McKay (1969 [1942]) perceived crime as a relatively stable characteristic of certain communities, one that is not affected by demographic or compositional changes. Fundamental to this claim is the notion that "successive changes in [neighborhood] nativity and nationality compositions" will not have appreciable impacts on levels of crime (1969 [1942]: 315). Furthermore, although some contemporary researchers hint at the fact that immigration might have a stabilizing influence on communities, the longitudinal relationship remains largely unknown (Martinez, Stowell, and Cancino, 2008). Because fixed-effects methods focus exclusively on the degree to which changes in demographic or social structural factors in a given neighborhood are associated with changes in levels of lethal violence, the regression results will cast new light on this under-analyzed aspect of disorganization theory.

Despite their strengths, some limitations persist in the fixed-effects regression techniques that bear mentioning. Specifically, the small portion

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of suburban neighborhoods with zero homicides (approximately 4–7 percent) in both the overall and race/ethnic specific analyses are automatically dropped from the analysis. Because the regression model changes, areas in which no homicides occurred for any of the three periods under investigation effectively are dropped from the likelihood function because no within-neighborhood variability can be analyzed (see Allison, 2005: 82). Substantively, the omission of such neighborhoods is not problematic for the purposes of this study, as we are interested in the dynamic link between immigration and lethal violence. In other words, the neighborhoods in which no homicides were committed will not contribute to a better understanding of the temporal *changes* in levels of lethal violence.[3]

Because the dependent variable consists of counts, the fixed-effects models will be estimated using a Poisson-based analytical approach. In the estimation procedure, we include the logged population as the regression "exposure" factor, which essentially translates the outcome variable into a rate during the estimation procedure. The diagnostics indicated that overdispersion (variance greater than the mean) is a problem in these data; thus, Poisson-based negative binomial regression is used (Osgood, 2000). In the following analyses, homicide counts are regressed on the independent variables.

RESULTS

DESCRIPTIVE AND BIVARIATE RESULTS

A key objective of the current study is to advance current knowledge on immigration and crime by introducing a longitudinal analytical design. Any study of change assumes that the analytical units have undergone observable changes during the period under investigation. In other words, if the average neighborhood structural and demographic composition remains stable over time, then it would be unnecessary to move beyond the use of cross-sectional data. Our first hypothesis was that ecological structures such as immigration, economic disadvantage, and residential stability would vary over time. The evidence indicates that between 1980

3. In a separate analysis (available upon request), we examined the structural and demographic compositions of these zero homicide communities. Clearly areas did experience some structural changes over time as they moved from rural to suburban communities. We do recognize that it is possible that the levels of other forms of violent and nonviolent criminal behavior in these areas also varied during this period. The limitation of available neighborhood-level longitudinal data prevents us from using broader measures of crime. Despite these limitations, we believe the strengths of the method, namely, that fixed-effects models produce unbiased estimates and, because they model temporal changes, outweigh these drawbacks.

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and 2000, many San Diego neighborhoods did shift structurally, thus supporting our first hypothesis. Table 1 contains the descriptive statistics for the lethal violence and neighborhood indicators that will be included in subsequent multivariate analyses.

Table 1. Descriptive Statistics

	1980	1990	2000
Total homicide rate	9.72	11.44	3.83
White homicide rate	6.51	5.44	2.43
Black homicide rate	30.24	34.20	12.53
Latino homicide rate	13.10	20.92	4.60
Percent foreign born	15.61(10.64)	19.83(12.41)	23.95(13.44)
Disadvantage index (standardized)	.41 (3.17)	.39 (3.27)	.40 (2.98)
Stability index	−.16 (1.86)	−.14 (1.92)	−.31 (2.01)
Adult/child ratio	5.44(12.83)	8.35(35.22)	6.02(13.71)
Percent professional employment	19.30 (8.98)	21.31 (9.24)	25.23(11.90)

NOTES: Standard deviations in parentheses. The total homicide rate is the number of homicides per 100,000. The White, Black, and Latino homicide rates are calculated as the number of racial/ethnic group-specific homicides per 100,000 group-specific persons. $N = 297$.

As a study focusing on neighborhood levels of homicide, the descriptive analyses provide a broad overview of the degree to which patterns of lethal violence have changed during the period under investigation. Establishing general trends in homicide levels across all census tracts is important because it will offer some insight into whether the patterns observed in San Diego during the past two decades are in some way exceptional. The descriptive analyses will reveal whether lethal violence in San Diego followed the rise and fall in the national homicide rates during this period or whether a disparate trend emerged (see Blumstein and Wallman, 2000). The same is true for levels of race/ethnic specific homicides. It is possible that the relative risk of homicide victimization in San Diego differs from broader trends. For example, previous research has shown that Latinos generally are victimized at higher rates than non-Latino Whites but not as high as non-Latino Blacks (see Martinez, 2002; Martinez and Lee, 2000). We will also be able to identify whether divergent racial/ethnic trends emerge in homicides (i.e., levels decreased for one group and increased for another).

Turning our attention to patterns of lethal violence, we note that the temporal variations in both overall and race/ethnic specific homicide rates are consistent with broader patterns. The data indicate that overall homicide rates, as well as those for specific race/ethnic groups, surged during the 1980s, peaked, and then declined sharply throughout the 1990s (see Blumstein and Wallman, 2000; Sampson and Bean, 2006). The uniformity

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of the rise and decline is notable—with the exception of White rates, which did not rise—as the overall and group-specific rates in 2000 fell to approximately one third of the corresponding 1980 values, thus mirroring patterns observed in previous research (Martinez, 2002; Peterson and Krivo, 2005). The descriptive findings highlight contours observed in previous studies, namely, that the Latino homicide rate for the city is higher than that for Whites but lower than the comparable rate for Blacks. Furthermore, despite the changes in the underlying risk of homicide victimization between 1980 and 2000, this general pattern was relatively stable during the entire period under investigation. Although these results show both consistencies and differences in lethal violence over time, it is not clear whether or to what degree they are influenced by neighborhood structural changes and immigration.

The large increase in the size of the foreign-born population is also evident in the descriptive results. By 2000, nearly a quarter of the population in the average neighborhood in San Diego was born outside of the United States, which is an increase of 53 percent in two decades. Furthermore, the observed change in the average community nativity composition was the most substantial relative to the other neighborhood indicators of interest. The lone exception is reflected in the fact that average levels of neighborhood disadvantage remained relatively stable over time. The results also show that in 2000, communities had lower levels of residential stability than they did in the 1980 census. The disparity in the size of the adult and child populations also varied over time, peaking in 1990. Finally, the presence of individuals employed in professional occupations experienced a sharp increase, growing by more than 30 percent in two decades.

Based on these descriptive findings, it is clear that neighborhood conditions were not static during the period under investigation. This finding suggests that the assumption of structural (or system) stability inherent in the cross-sectional analyses that have been conducted to date is problematic. Consistent with the rationale for our analytical approach, a longitudinal design does seem to be required to get to the "heart" (Bursik and Webb, 1982: 28) of the processes involved in shaping community levels of crime. However, as we have discussed, a key issue is whether neighborhoods maintained their relative positions to each other against a backdrop of secular change (Sampson and Morenoff, 2006). To address this issue, we examined correlations and scatterplots for our variables over time. The results (available upon request) indicate that neighborhoods remained generally, but not totally, stable relative to each other amid the temporal changes described in table 1. This trend was especially true for economic disadvantage and residential stability, as the correlations for these variables were always greater than .78, regardless of whether we compared 1980 with 1990, 1990 with 2000, or 1980 with 2000. A moderate degree of

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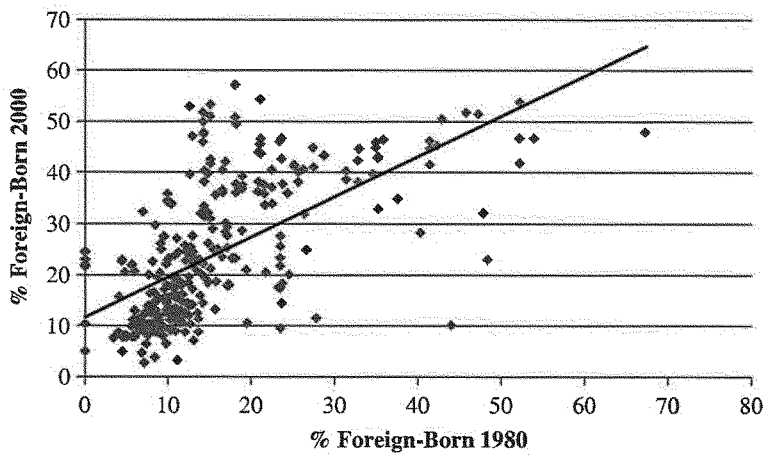
consistency was found for our immigration measure (r = .63) and homicide rates (r = .60) from 1980 to 2000. Figures 1 and 2 present scatterplots for these two key variables. So the question of positional "stability"—which we refer to as *system* stability to distinguish it from *residential* stability—can be answered as follows: 1) The relative position of neighborhoods in terms of disadvantage and residential stability remained largely the same, and 2) more variation was found over time when neighborhoods were compared in terms of immigration and homicide rates. In other words, some "system instability" occurs at the ecological level with regard to our key predictor variable of interest (immigration) as well as our outcome variable, although many neighborhoods also maintained their relative positions.

Figure 1. Neighborhood Change in Percent Foreign Born, 1980–2000



Before we turn to the multivariate models, we first must address our second hypothesis, which states that immigration is associated with residential (not system) instability. Because our indicator of stability includes a measure of length of residence in a given housing unit, the fact that stability declined dramatically over time is attributable, in part, to the influx of immigration the city experienced during this period. It is also a function of the transformation of previously rural areas into suburbs, as well as dramatically fluctuating real estate prices that provided the incentive to sell property or to abandon them. To examine the relationship between stability and immigration, we examined the bivariate correlations for these variables (available upon request). The correlations provide support for

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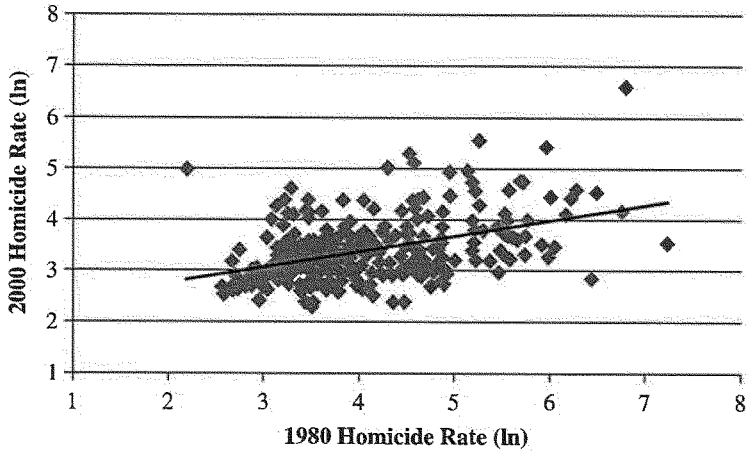
Figure 2. Neighborhood Change in Homicide Rates, 1980–2000



our second hypothesis. They reveal a weak negative (and significant) relationship between immigration and stability across all three time points, ranging from –.09 to –.24. Attention to the time ordering reveals an important aspect of the relationship. The strongest relationships (–.24) were between stability in 1990 and foreign born in 2000, as well as between stability in 1980 and foreign born in 1990 (–.20), which suggests that stable tracts at an earlier time point attract fewer immigrants at a later point in time. The observed pattern is consistent with the notion that immigrants, on the whole, tend to settle in less residentially stable neighborhoods. So immigration and instability are associated. But do these factors weaken social control and increase crime? We do not have data specifically on social control, but our multivariate analyses do allow us to answer indirectly the question with regard to the serious crime of homicide.

MULTIVARIATE RESULTS

As an initial examination of the temporal influences of neighborhood conditions, we regress the total number of homicide incidents on the community structural factors. The findings from this regression analysis are presented in table 2. Generally, the results indicate that observed patterns of lethal violence are shaped by changes in the broader neighborhood social structural composition. Consistent with theoretical expectations, socioeconomic disadvantage is associated positively with the number of homicide victims. In other words, the results indicate that the number of

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homicide victims tended to grow proportionately as the economic condition of neighborhoods deteriorated. The share of the labor force working in professional occupations is related inversely to the number of killings. The negative and significant effect of professional employment is anticipated, as scholars have argued that the presence of individuals involved in this occupational sector can serve as a buffer against criminal involvement (see Wilson, 1987).[4]

Table 2. Fixed-Effects Negative Binomial Regression of Homicide Victims on Neighborhood Structural Factors, San Diego, 1980–2000

Variable	*B*	SE
Adult/child ratio	.009	.004*
Disadvantage index (standardized)	.228	.021***
Stability index	.012	.028
Percent professional employment	–.017	.007*
Percent foreign born	–.018	.007*
Intercept	–8.091	.308***

NOTE: Logged population included as exposure. $N = 864$.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

The findings from the initial regression analysis do not offer support for the notion that residential stability affects homicide levels, net of other community factors. Because residential turnover was identified as one of the three primary sources of disorganization, this result runs counter to theoretical expectations. However, measures of residential mobility have received mixed support in previous research on this topic, which suggests that local context plays an important role in shaping the association between stability and observed levels of lethal violence (see Martinez, Stowell, and Cancino, 2008). And as we have discussed, good reasons persist to suspect that residential stability will not have the classic disorganizing effect in San Diego, not the least of which is its (weak) negative association with immigration. The size of the adult population relative to

4. In an effort to assess the stability of our findings, we reestimated the models eliminating tracts for which assessing change is likely to be unreliable. Specifically, we ran our regression models estimating the total number of homicide victims eliminating tracts with total populations of less than 500, 1,000, and 1,500 residents. Similar analyses were conducted for the race/ethnic specific models. The results from these analyses (available upon request) were highly consistent with the estimates produced when no population thresholds were imposed, which indicates that the inclusion of neighborhoods with small total populations has only a negligible influence on the parameter estimates.

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that of children also was associated positively with lethal violence. Conceptualized as an indicator of informal social control, a negative association between homicide and the number of adults per child might be anticipated. Yet, similar to the results reported here, this finding is consistent with previous cross-sectional studies, which have found this ratio to have either null or positive impacts on levels of criminal violence (see Martinez, Stowell, and Cancino, 2008; Sampson and Morenoff, 2004).

Of most substantive interest to this study, we also see that immigration does have a significant impact on overall levels of homicide. Consistent with hypothesis three, which was derived from the immigration revitalization perspective, the size of the foreign-born population is associated negatively with lethal violence.[5] Following Argue, Johnson, and White (1999), who contend that fixed-effects regression analyses are effective at modeling the effect of change, we interpret the significant negative effect of immigration as illustrating that *increases* in the size of the neighborhood foreign-born population over time lead to *decreases* in levels of lethal violence. Previous studies—virtually all of which have been cross-sectional in nature—have been hampered by an inability to assess temporal ordering, which is the key to understanding the dynamic forces driving processes of urban disorganization and revitalization. Although contemporary immigration researchers have speculated about the likely temporal impacts of immigration on neighborhood levels of violent criminal behavior, this study is one of the first to quantify such an association (see Martinez, 2002).

To test the racial/ethnic invariance of the factors associated with homicide (H4) and, specifically, to examine whether the immigration effect differs for various race/ethnic groups, we estimated the regression models using racially/ethnically disaggregated homicide data.[6] The results from this analysis are presented in table 3, which indicate strong similarities across groups with the exception of immigration, thus providing support for our fourth hypothesis. For example, we see for each group that structural disadvantage has a positive and significant impact on homicide victims. This finding is consistent with the social disorganization perspective

5. Although researchers commonly operationalize "recent immigrant" as the percent of the population who immigrated within the past decade (i.e., recent immigration), data limitations in the three census points prevent us from using a comparable measure in the current analysis (see Butcher and Piehl, 1998; Lee, Martinez, and Rosenfeld, 2001; Martinez and Lee, 2000). We believe this limitation is likely to have a conservative impact on our estimates, as previous research has demonstrated that the length of time in the United States might be associated with increased levels of criminal or other deviant behavior (see Portes and Rumbaut, 2001; Zhou and Bankston, 1998).

6. We exclude Asians from this analysis because too few victims were found, particularly in the earliest decade, to produce stable estimates at the tract level.

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as well as other theories that link deprivation to crime. Unlike the overall model, the results indicate that professional employment is not associated with levels of lethal violence in any of the racially/ethnically disaggregated models, again providing more support for the invariance hypothesis. The relative size of the adult population to that of children in a given neighborhood has a positive impact on the number of Black and White homicide victims, although it has no impact on Latinos, which is a minor wrinkle in the invariance story. Furthermore, consistent with the model for total homicides, neighborhood stability is not associated with the number of Black, White, or Latino homicide victims.

Table 3. Fixed-Effects Negative Binomial Regression of Race/Ethnic-Specific Homicide Victims on Neighborhood Structural Factors, San Diego, 1980–2000

	Black		White		Latino	
Variable	***B***	**SE**	***B***	**SE**	***B***	**SE**
Adult/child ratio	.008	.003*	.011	.004*	.006	.004
Disadvantage index (standardized)	.186	.023***	.226	.029***	.132	.024***
Stability index	.021	.033	−.008	.036	−.047	.037
Percent professional employment	.003	.011	−.003	.009	−.008	.011
Percent foreign born	.016	.009	−.025	.011*	−.017	.009*
Intercept	−8.508	.436***	−8.729	.377***	−8.165	.419***
N	826		853		863	

NOTE: Logged racial/ethnic-specific population included as offset.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed).

Focusing on the effect of immigration, we observe that increases in the share of foreign-born individuals are related inversely to the number of White and Latino homicide victims. The effect of immigration is in the positive direction for Blacks, although it fails to reach statistical significance. The null finding for Blacks is likely because of patterns of residential segregation, which results in the increased probability that immigrants will settle with coethnics (in San Diego, this means Latinos and Asians) rather than in Black neighborhoods. It again is worth noting that a similar combination of negative and null effects of immigration has been reported in previous studies focusing on immigration and crime (see Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Stowell and Martinez, 2007). But the current results extend our knowledge regarding this relationship by confirming that the observed negative association is not merely cross-sectional (i.e., a result of a decade-long influx) but rather that it is one that is sustained over time. Furthermore, the meaningful impact of immigration on Latino-specific homicides indicates that the effect of nativity composition

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might help to buffer against criminal involvement despite the generally high levels of disadvantage and social isolation many Latinos experience in San Diego (see Martinez, 2002; Martinez, Stowell, and Cancino, 2008). The finding that immigration reduces homicide even for nonimmigrant groups—in this case, Whites but not Blacks—suggests that Sampson's (2008: 32) argument about the "externalities" associated with immigration (e.g., economic revitalization and decreased segregation) very well might help account for some of the Black–White crime gap. Furthermore, our findings add credibility to Sampson and Bean's (2006: 21) argument that "increasing diversity and immigration" might have contributed to the observed reductions in violence across the United States. Indeed, the evidence from the current study lends support to this conclusion for the city of San Diego.

To examine further the racial invariance hypothesis, we conducted formal significance tests across our models (Paternoster et al., 1998). These results (available upon request) indicate that the effect of disadvantage was significantly different for Whites and Latinos—although it is important to note that the disadvantage coefficients were in the same direction—and that the effects of immigration differed significantly when we compared Whites with Blacks as well as Latinos with Blacks. These findings suggest that the impact of structural conditions on homicide over time is mostly invariant, but immigration might represent an exception. It is also possible that immigration in Black neighborhoods in San Diego is not high enough to have reached a "tipping point" and that a higher rate of immigration into Black neighborhoods would be associated with lower homicide levels. If that were to occur, then the invariance hypothesis would receive even stronger support. Findings from a study of Black neighborhoods in northern Miami are suggestive; immigration is associated with lower levels of Black homicide in areas where large numbers of immigrants have settled (Lee and Martinez, 2002).

Recall that our descriptive statistics established that homicide in San Diego decreased as immigration increased across three time points, spanning two decades of heavy immigration. Although cross-sectional analyses cannot surmount the problem of time ordering, to explore this issue further, we reran the longitudinal models and included a temporally lagged count of homicides.[7] This test of our fifth hypothesis allowed us to determine the impact of immigration on changes in lethal violence, net of other

7. To create the homicide lags, we first sorted the data by tract and year and used SAS (SAS Institute, Inc., Cary, NC) to calculate lags as the number of homicides that took place in a given neighborhood for the previous decennial census period. We completed this operation for the total number of homicides as well as for the counts disaggregated by race/ethnicity. Because we do not have information for killings around the 1970 census period, we could not create a lagged value to

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factors including a given community's previous homicide levels. The results from these analyses (available upon request) do not change the substantive pattern of findings for any models. In fact, the immigration coefficients are somewhat stronger, and the tests of statistical significance yielded smaller probabilities. With one exception, the lagged number of homicides had a positive and statistically significant effect on levels of lethal violence.

We also ran residual change regression models to assess the degree to which changes in lethal violence were not solely a result of initial levels of neighborhood structural conditions (see Bursik and Webb, 1982). Although data limitations—particularly our use of homicide counts rather than rates and the nonnormal distribution of our data—precluded an exact replication of the Bursik and Webb methodology, we used an analogous procedure to address this issue.[8] Results (available upon request) indicate that both the percent foreign-born and the disadvantage index offer meaningful improvement in predicting homicides, the latter having a somewhat stronger effect in each decade. In other words, the impact of these two variables was not the result of initial conditions alone.

CONCLUSION

During the past two decades, the United States has experienced a large influx of immigrants crossing the border into cities such as San Diego (Chácon and Davis, 2006; Martínez, 1994). Our analysis sought to illuminate the temporal link in neighborhoods between immigration and crime while drawing attention to recent research revealing an inverse association

attach to our 1980 data. Therefore, the lags for the earliest time point were all set to missing, and the 1980 data were dropped from the analysis because of these missing values.

8. We thank Robert J. Bursik Jr. and Glenn Deane for their help with this operation. Briefly, we first regressed homicide counts (t) on homicide rates for the previous decade ($t-1$). This technique was used because in our estimation equations, we specify the logged population as the regression "exposure," which again translates our dependent variable into a rate during the estimation procedure. A new variable containing standardized residuals from these initial estimations was saved, and the means for the residuals for each decade was plotted. Conceptually, these mean values indicate the rates "purged of their momentum." For purposes of comparison, additional regressions were estimated including only our immigration measure, only the disadvantage index, or only the fully specified model to predict homicides. By plotting the mean of the residual terms from these analyses along with those obtained from the crime-only models, we can illustrate the improvement in prediction associated with a given indicator. That is, the disparity between the plot line for the mean residual crime rate and those for the other indicators represents the contribution in prediction associated with a given indicator over the expected changes in the system.

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between the presence of newcomers and lethal violence (see Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Martinez, Stowell, and Cancino, 2008; Sampson, 2008; Sampson and Bean, 2006; Sampson, Morenoff, and Raudenbush, 2005; Stowell, 2007). Conventional wisdom holds that the shifting nativity composition would be associated strongly with increased levels of criminal violence (cf. Schuerman and Kobrin, 1986). But we found that over time, *more immigration means fewer overall homicides*. Furthermore, the results are relatively robust, as increases in immigration also correspond to fewer non-Latino White and Latino homicides in San Diego communities. The transformation of San Diego into an increasingly immigrant city with a mobile ("unstable") population has contributed paradoxically to lower levels of lethal violence. But not all groups shared the benefits, as the proportion of the foreign born was not a significant predictor of Black homicide. A likely explanation for this result is the fact that newcomers from abroad are not settling into traditionally Black neighborhoods in numbers that would have a meaningful impact on levels of homicide (Sampson and Wilson, 1995). We suggest that future research explore this question in more detail, with appropriate attention given to our finding that not all causes of crime are racially/ethnically invariant.

In addition, our findings show that nonsecular changes in immigration and homicide over time resulted in a moderate degree of "system-level" instability in the ecology of San Diego, which altered the relative positions of neighborhoods during two decades. This dynamic change is part of the story of suppressed crime in communities that have experienced increases in immigration and has been missing from the largely cross-sectional research that has been conducted to date. Disorganization still is associated with increased homicide, as our findings for economic deprivation indicate. But immigration and residential instability do not seem to be central to the disorganization process in San Diego during the closing decades of the twentieth century.

Our longitudinal approach, combined with the lack of a politically and economically strong ethnic enclave in San Diego, suggest to us that our results offer additional support for the emerging immigration revitalization perspective. The immigration effect remained robust even in our final set of regression models (H5) that included a temporally lagged count of previous levels of homicide. These results provide us with evidence that immigrants are not simply moving into low-homicide areas. More generally, this result suggests that immigration halts (and might reverse) some of the disorganization–crime–disorganization spiral that unfolds throughout long periods of time and is thought to play a central role in the creation of permanently high-crime neighborhoods (Schuerman and Kobrin, 1986). Our inclusion of temporal lags thus responds to one of Bursik's (1988: 542) three major suggestions for moving beyond the limitations of

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previous conceptions of social disorganization. In this case, he argued that scholars explore the "nonrecursive aspects" of the theory. The argument here is that at a certain point in a neighborhood's career, crime actually might foster social disorganization, which in turn, produces more crime that later leads to more disorganization and so on until the neighborhood "may be considered 'lost' territory for the purposes of effective crime reduction" (Schuerman and Kobrin, 1986: 98). Because the immigration effect gained strength with the addition of the temporal lag for homicide, we suggest that policy makers might wish to reflect on the possibility that immigration might play a key role in revitalizing neighborhoods that were otherwise "destined to become crime-ridden enclaves" (Schuerman and Kobrin, 1986: 98). Our results on this point are suggestive at best but are worthy of future consideration.

At a more general level, we wish to consider the extent to which the immigration revitalization perspective might be compatible with the social disorganization perspective and whether a synthesis of the two might better advance the communities and crime literature. They often have been viewed as competing ecological viewpoints (cf. Lee, Martinez, and Rosenfeld, 2001). But Shaw and McKay (1969 [1942]) were not narrow defenders of "their" theory; they routinely drew on complimentary approaches (e.g., strain theory) to advance their project of explaining ecological patterns of crime, and they had a deep knowledge of local conditions that helped ensure that their explanations were grounded in place-specific realities. Indeed, they spent decades gathering their own data and living in their primary research setting. Similarly, one of the current authors was born in San Diego and lived there for virtually the entire time period under study, whereas another has spent more than a decade traveling to the city on extended data collection trips. These experiences with San Diego's dynamic changes allow us to place our findings in context, especially with regard to immigration and residential instability.

If Shaw and McKay (1969 [1942]) were conducting research today in San Diego and other Sunbelt cities, then their inductive approach likely would have convinced them that perhaps both good and bad types of disorganization exist (for a "new" form of bad disorganization, see Rosin, 2008). By sensitizing researchers to this possibility, the immigration revitalization perspective can inform social disorganization theory and help point the way to a more complete understanding of urban crime. After all, the social disorganization approach focused on the intervening mechanism of social control in its explanation of crime. Although we cannot test this notion directly, our results suggest that scholars should consider seriously the possibility that immigration and residential instability are not inhibiting social control, at least in immigrant destination Sunbelt cities like San Diego.

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Although our study offered a unique opportunity to assess variations on how homicides in immigrant destination cities might unfold throughout a long period of time, some limitations merit mentioning. We perceive these limitations more as recommendations for future research than as challenges to the findings discussed. For example, we believe future longitudinal research on this topic would benefit from the incorporation of a more refined measure of new immigrants, similar to those typically used in earlier studies. Currently available data sources necessitated that we use an overall measure of immigration in this study (i.e., percent foreign born) rather than one that captures more precise characteristics of the foreign-born population, such as recent immigration or ethnicity (see Lee, 2003; Martinez, Stowell, and Cancino, 2008; Stowell, 2007).

Similarly, we believe that scholars should collect data for a wider array of immigrant destination cities in addition to those like San Diego that are situated near the U.S.–Mexico border. Because perceptions about border issues color public discourse and, consequently, policy decisions, it is increasingly important to understand the impact of immigration on levels of violence in border communities. But it is also true that immigrant settlement patterns are changing, with a larger share of foreigners moving into "nontraditional" immigrant destination cities. Indeed, the large-scale movement of immigrants into the nontraditional areas, particularly in the Midwest, has fostered a "new hostility in the nation's heartland" (Frey, 2006: 2). Because little is known about the impact of this shifting settlement pattern on levels of criminal behavior, research that examines the immigrant–crime link using data that includes information for nontraditional destinations might allow for greater generalizability. Conversely, we might find that local conditions continue to trump general predictions (Martinez, Lee, and Nielsen, 2004). Either way, we would hope that subsequent studies use longitudinal data to avoid the problems that we have identified with cross-sectional research.

Finally, we suggest that future work explore types of crime other than homicide. Although lethal violence is an enduring public concern, some antiimmigrant rhetoric is addressed to nonlethal violence and property crime. We believe that it is important for research on immigration to continue to play an important role in a range of criminological- and social-scientific research initiatives. Much like a century ago, movement from abroad is behind a dynamic transformation across the nation. With the changing demographic landscape, scholars are presented with an opportunity to deepen our understanding of patterns of crime in American communities and whether they are shaped by the arrival of immigrants. It is our hope that the current study not only contributes to the accumulated empirical evidence but also can be used to inform academic and public-policy discussions on the immigration and violent crime linkage as it

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unfolds over time. It is only by modeling the underlying dynamic relationships that we can draw conclusions about whether immigration, residential stability, and other structural characteristics of neighborhoods lead to disorganization or revitalization. In this way, we will get to the heart of the community–crime connection, thereby continuing to do justice to the legacy of Shaw and McKay (1969 [1942]).

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Ramiro Martinez, Jr. is a professor in the School of Criminal Justice at Florida International University in Miami, Florida. He is a member of the Racial Democracy, Crime and Justice-Network at The Ohio State University and a member of the National Consortium on Violence Research. His research interests include race/ethnicity/immigration, crime, and the criminal justice system. He received the Texas State University College of Liberal Arts Distinguished Alumni Achievement Award and Texas State Alumni Association's Alumni Achievement Award both in 2009. He is a past recipient of the American Society of Criminology's Division on People of Color and Crime Coramae Richey Mann Award for outstanding scholarship in the area of race, crime, and justice; the Florida International University Faculty Award for Excellence in Research; and the American Sociological Association Latina\o Section Award for Distinguished Contributions to Research. He is the author of *Latino Homicide: Immigration, Violence and Community* (2002, Routledge Press) and editor of *Immigration and Crime: Race, Ethnicity, and Violence* (2006, New York University Press).

Jacob I. Stowell received his PhD from the University at Albany—SUNY and is currently an assistant professor in the Department of Criminal Justice and Criminology at the University of Massachusetts—Lowell. In addition to his interest in immigration, he also is involved in research that focuses on the effect of segregation on criminal violence and other dimensions of inequality. His work has been published in journals such as the *American Journal of Sociology* and *Social Science Quarterly.*

Matthew T. Lee is an associate professor in the Department of Sociology and a conflict management fellow at the University of Akron. He is the author of *Crime on the Border: Immigration and Homicide in Urban*

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Communities (2003. LFB Scholarly), and his work has appeared in journals such as *Criminology*, *Social Problems*, *Social Psychology Quarterly*, and *Sociological Quarterly*. His current research interests include immigration and crime, organizational deviance, and altruism.

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IMMIGRATION AND THE RECENT VIOLENT CRIME DROP IN THE UNITED STATES: A POOLED, CROSS-SECTIONAL TIME-SERIES ANALYSIS OF METROPOLITAN AREAS*

JACOB I. STOWELL
Department of Criminal Justice and Criminology
University of Massachusetts–Lowell

STEVEN F. MESSNER
KELLY F. MCGEEVER
LAWRENCE E. RAFFALOVICH
Department of Sociology
University at Albany–State University of New York

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A good deal of research in recent years has revisited the relationship between immigration and violent crime. Various scholars have suggested that, contrary to the claims of the classic Chicago School, large immigrant populations might be associated with lower rather than higher rates of criminal violence. A limitation of the research in this area is that it has been based largely on cross-sectional analyses for a restricted range of geographic areas. Using time-series techniques and annual data for metropolitan areas over the 1994–2004 period, we assess the impact of changes in immigration on changes in violent crime rates. The findings of multivariate analyses indicate that violent crime rates tended to decrease as metropolitan areas experienced gains in their concentration of immigrants. This inverse relationship is especially robust for the offense of robbery. Overall, our results support the

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hypothesis that the broad reductions in violent crime during recent years are partially attributable to increases in immigration.

Academics and political pundits have puzzled over the dramatic reduction in crime in the United States that has been observed over the last decade or so. With crime rates approaching 40-year lows, the drop in crime has been recognized as a welcome, yet largely unexpected, phenomenon. In searching for explanations for this pattern, factors ranging from increased levels of incarceration to the legalization of abortion have been cited as possible explanations (Levitt and Dubner, 2005: 117–46; see also Blumstein and Wallman, 2000). Despite widespread speculation, however, empirical research has yet to identify the leading causal mechanisms behind this fortuitous social change.

In an attempt to cast new light on this discussion, the current research focuses on a potentially important causal factor: the degree to which immigration may have contributed to the observed reductions in crime. Traditionally, both criminological theory and conventional wisdom have held that immigration is a social process that promotes, rather than suppresses, crime (Martinez, 2002). Taking a historical view of eras preceding the recent crime drop, Gurr (1989: 76) argues that "America's three great crime waves can be linked to immigration . . ." [and that] ". . . episodes of violent crime wound down as immigrants were incorporated into the expanding economy."[1] The belief that immigrants are responsible for crime has filtered into the public consciousness and has been, and continues to be, used to fuel nationalistic/isolationist rhetoric and anti-immigration movements (Brimelow, 1996; Lamm and Imhoff, 1985).

The longstanding and widely held notion that immigration leads to crime seems to be at odds with recent trends at the national level. According to the Bureau of Justice Statistics, continuing a decade-long decline, national violent victimization rates reached an all-time low in 2002. Furthermore, the overall decrease in violent crime is not attributable to large declines in one particular type of offending, as the rates of homicide, rape, robbery, and assault have all experienced substantial reductions.[2] At the same time that violent crime rates have been falling dramatically, immigration to the United States has been thriving. Since 1990, the size of foreign-born population grew by 56 percent, which is an addition of nearly 11 million individuals.[3] The fall in crime rates has thus been accompanied by the influx of large numbers of foreign-born individuals.

1. Gurr (1989) and Brimelow (1996) argue that America's "three great crime waves" began in 1800, 1850, and 1960; each lasted between 20 and 30 years.
2. Bureau of Justice Statistics (http://www.ojp.usdoj.gov/bjs).
3. U.S. Census Bureau American FactFinder (http://factfinder.census.gov/home/saff/main.html?_lang=EN).

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In addition to the evidence provided by these simple trends, an emerging body of scholarship has raised questions about whether immigration is related to increased levels of violent crime (Hagan and Palloni, 1998; Lee, 2003; Lee and Martinez, 2002; Martinez, 2002; Martinez and Lee, 2000). Using neighborhood-level data, Martinez and colleagues consistently find little empirical support for the immigrant/crime link. Indeed, as Martinez and Lee (2000: 486) argue, "in many cases, compared with native groups, immigrants seem better able to withstand crime-facilitating conditions than native groups." Findings such as these have led some researchers to take seriously the notion that immigration may contribute to *lower* crime rates. For example, Sampson and Bean (2006: 21) have recently speculated that although it has been largely overlooked, "the broad reduction of violence in the United States over the past decade was due in part to increasing diversity and immigration" (see also Sampson, 2006).

When considering the connection between immigration and crime, it is important to note that Sampson and colleagues do not attribute the crime drop solely to the fact that immigrants themselves are generally less likely to engage in criminal behavior, although evidence exists to support the claim of lower levels of offending for immigrant populations. In his study of Chicago neighborhood residents, Sampson (2006:A27) reports that immigrants were "45 percent less likely to commit violence than were third-generation Americans." To the extent that a similar pattern is observed more generally, it stands to reason that the increase in immigration has contributed to the crime drop because the *immigrants* themselves commit fewer criminal acts. Another intriguing aspect of this relationship, according to Sampson, is the fact that *communities* in which immigrants settle are also less violent; this benefit extends to native-born residents. Sampson (2006:A27; see also Sampson and Bean, 2006) reports that "living in a neighborhood of concentrated immigration is directly associated with lower levels of violence" for non-Latino whites and blacks as well. It is not yet clear, however, the degree to which this latter, macrolevel immigrant buffer is associated with the broad reductions in crime.

The purpose of this study is to examine systematically the extent to which immigration helps explain recent changes in serious crime rates in the United States. We go beyond previous research by estimating dynamic models using pooled, cross-sectional time-series data for a relatively large sample of metropolitan areas during the 1994–2004 period. Given that the theoretical speculations about the impact of recent immigration on criminal violence have been cast primarily in terms of "change," dynamic models are most appropriate for assessing these hypotheses. In short, we attempt to cast new light on the role of increased immigration in the widely observed, but poorly understood, crime decline in the United States over recent years.

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THEORY AND PREVIOUS RESEARCH

THE CHICAGO SCHOOL AND CLASSICAL SOCIAL DISORGANIZATION THEORY

The social disorganization perspective associated with the Chicago School has provided the theoretical foundation for most of the extant research on immigration and crime (see Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002, 2008; Martinez, Stowell, and Cancino, 2008; Stowell, 2007). The theoretical link to this topic is clear given that this perspective represents "arguably . . . the most important and well-known assessment of the immigration and crime experience in the United States" (Martinez, 2008: 501). Because more comprehensive discussions of the theory can be found in the literature, we provide an abridged review here (see Bursik and Grasmick, 1993; Kornhauser, 1978).

Shaw and McKay (1969 [1942]) provide the classic application of the insights of the Chicago School for an understanding of crime. Adhering to the principles of urban ecology, Shaw and McKay argue that structural characteristics of urban neighborhoods, and not the characteristics of their residents, make them particularly susceptible to the commission of delinquent acts. Social disorganization theory identifies three primary structural characteristics (economic deprivation, residential instability, and racial/ethnic heterogeneity) that can be used as proxies to gauge the extent to which an area is socially disrupted (see Sampson and Groves, 1988: 780).

When considering the Chicago School tradition, it is important to note the particular attention disorganization theorists pay to what they perceive to be the disruptive force of immigration. Indeed, social disorganization theory holds that increased disorder and, by implication, rates of crime are especially likely during periods of large-scale immigration. Disorganization theorists argue that community formation is undermined by an increase in language and cultural differences maintained by a diverse immigrant population (Thomas and Znaniecki, 1920). Moreover, informal networks of monitoring and social control prove difficult to establish because the areas into which immigrant groups settle are often resource poor and residentially unstable. Thus, the theory speaks directly to why we would expect higher rates of violent crime in areas with large proportions of immigrants and during times with higher levels of immigration.

RESEARCH ON IMMIGRATION

In contrast with the disorganization perspective's assertion of a positive relationship between immigration and crime, noncriminological research on immigration portrays the process of immigration in a more positive light and offers insight into the possibility of an inverse association

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between immigration and crime (Kao and Tienda, 1995; Model, 1995; Ogbu, 1991; Palloni and Morenoff, 2001; Zhang and Sanders, 1999). Indeed, immigration scholars refer to the fact that immigrants (and particularly Latinos) fare better than native-born groups on several health-related outcomes (i.e., infant birth weight as well as adult and infant mortality) as a paradox (or the "Latino paradox") (see Palloni and Morenoff, 2001; Sampson and Bean, 2006). According to the literature, these advantages are unexpected "given the relatively high levels of disadvantage" experienced by many immigrants in this country (Sampson and Bean, 2006: 20).

A leading explanation for the observed differences is selectivity theory, which posits that immigrants who decide to come to the United States do so for the opportunity to improve their life chances. Central to the selectivity theory is the belief that "persons who migrate are more ambitious, talented, and diligent than those who do not" (Model, 1995: 538). Furthermore, selectivity explanations suggest that in addition to these dispositional qualities, migrants are also more likely to have access to stronger social networks, which "provide information, facilitate the move, and ease the process of adjustment and adaptation" (Palloni and Morenoff, 2001: 160; see also Hagan, Levi, and Dinovitzer, 2008). Therefore, the claims regarding immigrants made by selectivity theory are at odds with those typically associated with classical social disorganization theory. The selectivity hypothesis also suggests that immigration may not be associated with the disruption of ecological dynamics as cited by the Chicago School theorists. Therefore, we believe that theories of immigration offer strong reasons to anticipate an inverse relationship between immigration and crime.

RESEARCH ON IMMIGRATION AND CRIME

Published studies examining the impact of immigration on crime in American society emerged nearly a century ago (see Martinez, 2002; Martinez and Lee, 2000; Rosales, 1999). The early research on this topic, despite their methodological limitations, raised questions about whether immigration promoted higher levels of crime. As rates of immigration have increased over the past decade, criminologists have shown a renewed interest in understanding the extent to which patterns of criminal behavior are tied to the influx of foreigners (Martinez, 2002, 2008; Martinez and Valenzuela, 2006). With the increased interest in this topic, the growing body of research literature on immigration and crime contains studies that focus on several distinct substantive dimensions and different levels of analysis.

Individual-level studies consistently report that immigrants are less likely to be involved in or to be institutionalized for criminal involvement than native-born individuals (see Butcher and Piehl, 1998a; Hagan and

Palloni, 1998; Sampson, 2008; Sampson and Bean, 2006). Furthermore, research also confirms an interesting pattern, whereby the likelihood of criminal involvement increases across immigrant generations (see Hagan, Levi, and Dinovitzer, 2008; Hagan and Palloni, 1999). According to Hagan, Levi, and Dinovitzer (2008: 101), although the effect "wears off" over time spent in the United States, first-generation and, to a lesser extent, second-generation immigrants "enjoy some protection, or resilience, from crime."

Research also fails to find support for the increased criminality of undocumented immigrants; this group is portrayed as the most crime-prone segment of the foreign population (Hickman and Suttorp, 2008; Nevins, 2002; Sampson, 2008: 29). In a study of immigrants released from the Los Angeles County Jail, Hickman and Suttorp (2008: 77) conclude that "deportable aliens (a combination of both undocumented immigrants and those with an expired visa status) [do not represent] a unique threat to public safety." Most of the recent criminological research on immigration, however, examines the association between immigration and crime using macrolevel data. Because the current study follows this tradition, the following review focuses on the impacts of immigration on aggregate levels of crime. A schematic summary of the relevant contemporary research is provided in table 1.[4]

The work of Martinez and colleagues represents the most extensive research program on the macrolevel connection between immigration and crime (Lee, 2003; Lee and Martinez, 2002; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Martinez and Lee, 2000; Martinez, Stowell, and Cancino, 2008; Nielsen, Martinez, and Lee, 2005). The research conducted by Martinez has focused on both neighborhood- and city-level influences on violent crime, and particularly on levels of homicide. The examination of this question at different levels of aggregation certainly contributes to a more comprehensive understanding of the impact of immigration on violent crime. Furthermore, although much of the existing research relies on cross-sectional analytical designs, the findings reported by Martinez and colleagues have implications for the current study because they provide the most systematic tests of the immigrant/crime link to date.

In *Latino Homicide* (2002), Martinez compares neighborhood structural conditions and homicide rates between Latino and non-Latino racial groups in five American cities (Miami, El Paso, San Diego, Chicago, and Houston). Using racially and ethnically disaggregated victimization data, he finds that rates of Latino homicide victimization tend to be higher than

4. For more comprehensive reviews of contemporary and historical research on immigration and crime, see Martinez (2002, 2006) and Peterson and Krivo (2005).

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Table 1. Summary of Contemporary Research on Immigration and Crime

Author	Measure of Immigration	DV	Impact	Data	Design	Study Period
Metropolitan Level						
Butcher and Piehl (1998b)	% Recent immigrant (<1 year)	Overall and violent crime rates	NS	National sample ($N = 43$)	Cross sectional/longitudinal	1980–1990
Hagan and Palloni (1998)	% Undocumented immigrants	Overall, violent, property arrest rates	+/NS	Sample from 5 western states ($N = 34$)	Cross sectional	1980
Reid et al. (2005)	% Foreign-born noncitizens % Recent immigrant (<10 years) % Foreign-born Asian % Foreign-born Latino English proficiency	Homicide, robbery, larceny, burglary rates	–/NS	National sample ($N = 150$)	Cross sectional	2000
Phillips (2002)	% Foreign-born	Race-specific homicide rates	NS	National Sample ($N = 191$)	Cross sectional	1990
Neighborhood Level						
Lee (2003)	% Recent immigrant (<10 years)	Homicide	–/NS	El Paso, Miami, San Diego	Cross sectional	1985–1994
Lee and Martinez (2002)	% Recent immigrant (<10 years)	Homicide	No regressions	Miami	Cross sectional/Descriptive	1985–1995
Lee, Martinez, and Rodriguez (2000)	Latino immigration	Homicide	No regressions	Miami, El Paso	Cross sectional/Descriptive	1985–1994
Lee, Martinez, and Rosenfeld (2001)	% Recent immigrant (<10 years)	Homicide	–/NS	Miami, El Paso, San Diego	Cross sectional	1990
Martinez (2002)	% Recent immigrant (<10 years)/ethnicity	Various homicide types	No regressions	Miami, El Paso, San Diego, Chicago, and Houston	Longitudinal/descriptive	1980–1994
Martinez and Lee (2000)	% Foreign-born/Ethnicity	Homicide	No regressions	National/city trends	Longitudinal/descriptive	1980–1994
Martinez, Stowell, and Cancino (2008)	% Recent immigrant (<10 years)	Homicide	–/NS	San Diego, San Antonio	Cross sectional	2000
Morenoff and Astor (2006)	Immigrant generation	Self-reported nonlethal violence	+	PHDCN	Cross sectional	1995–2002
Sampson, Morenoff, and Raudenbush (2005)	Immigrant concentration	Self-reported nonlethal violence	+	PHDCN	Cross sectional	1995–2002
Sampson (2008)	Immigrant concentration	Self-reported male violence	–	PHDCN	Cross sectional	1995–2002
Sampson and Morenoff (2004)	Immigrant generation	Homicide	–/NS	PHDCN	Cross sectional	1995
Sampson, Raudenbush, and Earls (1997)	Immigrant concentration	Perceived violence, victimization, homicide	+/–/NS	PHDCN	Cross sectional	1995
Stowell (2007)	Immigration measures based on ethnicity	Total, expressive, instrumental violence	–/NS	Miami, Houston, Alexandria (VA)	Cross sectional	2000
City Level						
Martinez (2000)	Immigrant concentration	Latino homicide rates	+/NS	National sample ($N = 111$)	Cross sectional	1980

ABBREVIATIONS: DV = dependent variable; NS = not significant

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similar rates for non-Latino whites and lower than those for non-Latino blacks. The methodology employed in this analysis is largely descriptive, but Martinez points out that Latinos are victims of homicides at lower rates than non-Latino blacks, even though both groups live in structurally similar neighborhoods. Indeed, Martinez (2002: 141) argues that rates of Latino homicide are lower than expected given the levels of structural disadvantage for many Latino neighborhoods.

Quantitative evidence in the research literature indicates a negative effect of immigration on violent crime. Again using racially disaggregated homicide data for three cities, Lee, Martinez, and Rosenfeld (2001; see also Lee, 2003) find that the size of the recently arrived immigrant population in Miami is inversely related to levels of homicide. For the other two cities under investigation (El Paso and San Diego), the authors report that immigration has nonsignificant effects in all but one regression analysis. The authors interpret the combination of negative and null findings as nonsupportive of either theoretical expectations or popular sentiment regarding the association between immigration and crime (Lee, Martinez, and Rosenfeld, 2001: 571). Similarly, in a study of Chicago neighborhoods that collected observational data using the systematic social observation method, Sampson and Raudenbush (1999) report an inverse association between community levels of concentrated immigration and robbery rates.[5] The research mentioned above does find some support for social disorganization theory in that social structural factors such as poverty and residential instability have significant positive effects on violence.

Sampson and colleagues have also begun to conduct research on immigration using the data collected as part of the Project on Human Development in Chicago Neighborhoods (PHDCN) (see Sampson, 2008; Sampson and Bean, 2006; Sampson, Morenoff, and Raudenbush, 2005). For example, in an analysis of the racial/ethnic differences in levels of criminal involvement, Sampson, Morenoff, and Raudenbush (2005) conclude that both individual- and community-level indicators associated with immigration predict lower levels of criminality. Specifically, Sampson, Morenoff, and Raudenbush (2005: 231) find that the negative "contextual effect" of living in immigrant communities emerged "even after a host of other factors, including the immigrant status of the person, were taken into account." In addition, Sampson and Bean (2006: 21) argue that although levels of criminal participation differ across generations, their data suggest that "it is no longer tenable to assume that immigration and diversity

5. The "concentrated immigration" variable is an index composed of percent Latino, percent foreign born, and the density of children (percent of persons aged 6–15 years).

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automatically lead to social disorganization and consequently crime" (see also Sampson, 2006).

Although research has shown that immigration is not associated with higher levels of crime across neighborhoods, other studies examine this relationship using larger area units. In their study of the impacts of noncitizen and illegal immigration on 1980 metropolitan arrest rates, Hagan and Palloni (1998) find that the size of the illegal immigrant population is unrelated to either violent or property arrest rates (see also United States Commission on Immigration Reform, 1994). Although Hagan and Palloni (1998: 380) report that the number of noncitizens is related to an increase in property arrests, they conclude that "there is no compelling or systematic evidence at the SMSA [Standard Metropolitan Statistical Area] level that immigration causes crime." Similarly, in a study of 43 metropolitan areas, Butcher and Piehl (1998b) find that whereas immigrants generally tend to settle in areas with high crime rates, changes in the size of the immigrant population between 1980 and 1990 are unrelated to changes in either overall or violent crime rates. Most recently, in an analysis of metropolitan-level data, Reid et al. (2005) find immigration to be either unrelated or inversely related to levels of violent and nonviolent crime.

A series of studies also shows that Latino-specific homicide rates are influenced by the social structural conditions of cities. Indeed, the city-level studies that focused on Latino violence were among the first to call attention to the important role that immigration may have in shaping patterns of lethal violence. In research that compared victimization rates in Miami and El Paso, Lee, Martinez, and Rodriguez (2000) find that killings involving Latinos are distributed similarly across homicide types (i.e., acquaintance, intimate, and stranger) in both cities. The authors conclude that the higher number of homicides committed in Miami is reflective of the more violent urban and regional contexts in that area of the country. Similarly, using a sample of 111 U.S. cities, Martinez (1996) finds that economic inequality among Latinos is associated with increased levels of Latino homicide victimization. A similar finding is reported by Martinez (2000), who reports that immigration is associated with increased levels of felony homicides (i.e., those committed during the commission of another crime). Martinez (2000) concludes that the increase in felony homicides in areas with large Latino immigrant populations is likely the result of increased contact with other non-Latinos (see also Blau, 1977). It is important to note a more general trend in these studies; the social structural characteristics are related to homicide, and this pattern is consistent with the expectations of social disorganization theory.

Two general conclusions can be drawn from the research on the effects of immigration on crime. First, the weight of the evidence suggests that immigration is not associated with increased levels of crime. To the extent

that a relationship does exist, researchers often find a negative effect of immigration on levels of crime, in general, and on homicide in particular. This finding runs counter to the theoretical logic of the classical social disorganization perspective. Second, the criminological research on immigration also finds partial support for the theory, which is evidenced by the strong and significant impacts of other social structural characteristics on levels of crime. Just as social disorganization theory predicts, indicators such as poverty and residential instability emerge as significant predictors of crime. In light of these contrasting results, immigration researchers generally interpret the unexpected negative impact of immigration on crime as a limitation of social disorganization theory.

LIMITATIONS OF PREVIOUS RESEARCH

As mentioned, most prior immigration/crime research has used a combination of neighborhood-level data available for decennial years and has independently collected crime measures. This approach has advanced knowledge on this topic considerably by casting new light on the associations between immigration and crime. Still, it is not clear the extent to which temporal variations in criminal violence may be linked to the recent surge in immigration. Yet as displayed in table 1, only one study employing multivariate modeling has incorporated longitudinal analyses, and this study is based on a relatively small sample of cases—43 metropolitan areas (Butcher and Piehl, 1998b). Moreover, this study examines change in crime from 1980 to 1990, which thereby misses completely the decade of large-scale immigration and the dramatic crime decline.

In addition, more recent immigrant settlement patterns indicate the need to broaden the geographical scope of research on immigration and crime. The work of Martinez and colleagues, which comprises the bulk of the research literature, focuses largely on the influence of immigration on homicide victimization across neighborhoods in a small number of Sunbelt cities (Miami, El Paso, San Diego, and Houston) (Lee, 2003; Lee and Martinez, 2002; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Martinez and Lee, 2000; Nielsen, Martinez, and Lee, 2005; Stowell, 2007; for exceptions, see Martinez, 1996, 2000). Clearly, focusing scholarly attention on such areas is warranted given the fact that they continue to attract a disproportionate share of the foreign-born population. Recent census data indicate that approximately two thirds of foreigners reside in one of six states (California, Texas, Florida, Illinois, New York, and New Jersey).[6]

6. Migration Policy Institute (http://www.migrationinformation.org/datahub/maps.cfm).

Moreover, because Latinos represent the bulk of the immigrant population, it is appropriate that studies of immigration and crime concentrate on areas that tend to be traditional destinations for foreign-born Latinos.

Yet despite the continued settlement of immigrants among relatively few states, there is also the need for scholars to be sensitive to changes to this general immigration trend. In part, this is driven by the diversity of the contemporary immigrant stream, which is well documented in prior research (Alba et al., 1999; Kleniewski, 1997; Massey, 1995). Furthermore, recent research on immigrant settlement patterns has found that, increasingly, foreign-born individuals are not choosing to live in traditional gateway states (i.e., California and New York). A recent article in the *New York Times* reports that the size of the immigrant populations in Midwestern and Southern states is growing at a high rate (Lyman and Goodman, 2006). According to the 2006 American Community Survey, between 2000 and 2006, the size of the foreign-born population in ten states grew by at least 40 percent.[7] It is not yet known the degree to which the growth in the immigrant population in these areas has had a meaningful effect on their levels of criminal violence. Therefore, obtaining a more comprehensive understanding of whether immigration facilitated the observed decline in crime requires researchers to consider not only areas classified as traditional immigrant destination cities but also those that are emerging as such.

THE CURRENT STUDY

This study seeks to advance current knowledge on the relationship between immigration and crime by addressing several limitations of previous research. Most notably, we employ a longitudinal analytical design to test whether changes in levels of immigration are systematically associated with the recent reductions in crime in the United States. Previous claims by scholars in the field imply dynamic influences of immigration on crime. For example, Martinez (2006: 12) argues that although research has yet to reveal "a definitive causal connection . . . over time it appears that more immigrants means less violence." Prima facie evidence certainly supports this argument; yet whether a statistical relationship exists, net of other social structural conditions, remains an open question. Furthermore, we use data for a broad array of geographic areas, which may cast new light on how nontraditional immigration settlement patterns may influence the relationship between immigration and crime (see Frey, 2006; Lyman and Goodman, 2006).

7. The ten states with the fastest-growing foreign-born populations are Delaware, South Carolina, Nevada, Georgia, Tennessee, Alabama, Arkansas, North Carolina, and Indiana.

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DATA

The data used in this study are collected from two sources. The independent variables are constructed using annual data collected during the Current Population Survey (CPS), as provided by the Integrated Public Use Microdata Series (IPUMS) (see King et al., 2004).[8] The CPS provides data for individuals aged 15 years and older from a national probability sample of approximately 50,000 households (see Noonan, Smith, and Corcoran, 2007). More specifically, this study makes use of the March Annual Demographic file and Income Supplement, which contains information on an array of demographic and socioeconomic factors. Additionally, what makes the CPS data particularly well suited for our purposes is that the data also identify the metropolitan area in which a respondent resides. Using the geographic identifiers, we were able to generate metropolitan-level estimates for each indicator to be included in the regression models. The indicators of criminal behavior were constructed using the Federal Bureau of Investigation's (FBI's) Uniform Crime Reports (UCR). The UCR data for 1994–2004 were extracted from the Interuniversity Consortium for Political and Social Research. Specifically, all violent crime counts known to police, and the corresponding population totals, were collected for the 103 metropolitan areas previously identified.

We recognize that sampling variability introduces some error in metropolitan-level estimates, particularly in areas with small sample sizes. In their discussion of the CPS data, the Census Bureau indicates that "estimates for the smaller metropolitan areas (those with populations under 500,000) should be used with caution because of the relatively large sampling variability associated with these estimates."[9] For this reason, only metropolitan areas that had a population of 500,000 or more in 2000 are included in our sample. Using this population selection criterion, our sample includes 103 metropolitan regions (see appendix A). The demographic compositions of the metropolitan areas in our sample are highly correlated with their corresponding decennial census values (see appendix B), which suggests that sampling error is not likely to have unduly influenced the estimates in our sample.

Because immigrants tend to settle in large urban areas, our sample of metropolitan areas includes a large share of the nation's foreign-born

8. The IPUMS-CPS is derived from the CPS conducted by the U.S. Census Bureau. An advantage of IPUMS-CPS data is that they are "harmonized" to ensure compatibility over time (http://cps.ipums.org/cps/).

9. Minnesota Population Center data on the population statistics of metropolitan areas in Minnesota: http://cps.ipums.org/cps-action/variableDescription.do?mnemonic=METAREA.

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population. Again using decennial census data for the purposes of comparison, our sample covers nearly 90 percent of all immigrants who reside in a metropolitan area and more than 84 percent of all foreign-born individuals living in the United States (89.2 percent and 84.2 percent, respectively). Our sample also includes similar shares of the recent immigrant population, accounting for 89.1 percent of the individuals living in a metropolitan region and 83.8 percent of those who arrived during the 1990s. This stable pattern clearly indicates that the largest metropolitan areas continue to attract and to retain most of those born outside of the United States.

Before describing the measures, the rationale for our focus on metropolitan areas warrants continued discussion.[10] This study departs from most existing research on immigration and crime, which examines the impact of immigration on neighborhood levels of violence (see Martinez, 2006). Although neighborhood-level analyses are obviously valuable and are within the tradition of the Chicago School, such analyses to date have encompassed an important but limited portion of the territory in which immigrants reside. More central to the current study is the fact that the bulk of previous research on this topic has been cross-sectional in nature, as noted above. These limitations are understandable given practical considerations of data availability. Tract-level information on immigrant populations from the U.S. Census is available only for decennial years, and it would not be feasible to generate estimates from annual survey data at this level of geography. In addition, crime data for urban neighborhoods must be secured from local police agencies, which becomes a monumental task for many urban communities. Data for many metropolitan areas from Census and FBI sources, in contrast, are available to permit pooled, cross-sectional time-series analyses of the impact of changes in immigration on changes in crime rates.

We also note that there is precedent in the literature for analyses of the relationship between immigration and crime at the metropolitan level (see Butcher and Piehl, 1998b; Hagan and Palloni, 1998; Phillips, 2002; Reid et

10. The selection of the unit of analysis for macrolevel research on homicide has long been the subject of debate (e.g., Bailey, 1984; Land, McCall, and Cohen, 1990; Messner, 1982). Focusing specifically on homicide, Parker, McCall, and Land (1999: 118–9) adopt a strong position, positing that "the unit of analysis *is not* [original emphasis] a relevant concern for studies of homicide" and concluding that "any general theory of homicide should generate empirical invariant findings regardless of the level of analysis examined." We adopt a more moderate position and acknowledge that certain macro units might be more desirable than others depending on the substantive objectives of the research and the hypothesized causal processes. As explained in the text, our decision to use metropolitan areas is driven by pragmatic considerations. Data for these units allow us to conduct dynamic analyses with refined time resolution (annual change) for a reasonably diverse and relatively large sample of ecological units.

al., 2005). For example, using a sample of 191 metropolitan areas, Phillips (2002) reports that immigration is unrelated to white, black, and Latino homicide rates. Most recently, using a national sample of 150 metropolitan areas, Reid et al. (2005) find a series of negative and null effects of immigration on both violent and nonviolent crime rates. Although the metrolevel studies conducted by Butcher and Piehl (1998b) and Hagan and Palloni (1998) employ data for earlier decades (1980 and 1990), their results offer little support for the notion that immigration is positively associated with levels of criminal deviance. It is worth noting that even though metrolevel studies represent a small subset of the research on immigration and crime, the findings from these studies reveal that factors such as economic disadvantage and residential instability are related to higher levels of crime. Such results mirror closely those presented in the studies that concentrate on smaller geographic areas (see Lee, 2003; Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002, 2006; Martinez and Lee, 2000; Morenoff and Astor, 2006; Sampson and Bean, 2006; Stowell, 2007).

MEASURES

DEPENDENT VARIABLES

The dependent variables for the analysis are the FBI's violent crime index and the constituent elements of the index (i.e., rates of criminal homicide, robbery, forcible rape, and aggravated assault). The violent crime rate is the sum of the four violent offenses. The overall and specific crime rates are expressed per 100,000 based on population totals as provided from the UCR.

INDEPENDENT AND CONTROL VARIABLES

Our primary independent variable, concentrated immigration, is a measure that has been used extensively by Sampson and colleagues (Morenoff and Sampson, 1997; Sampson and Morenoff, 2004; Sampson, Raudenbush, and Earls, 1997). It is composed of the summed z-scores for the percent Latino and the percentage of the population who are foreign born. In the current study, we employ a general measure of immigration based on the IPUMS "nativity" variable, which includes all individuals born outside of the United States.[11]

11. We recognize that the issue of undocumented immigration is often conflated with immigration more generally. We explored the possibility of including a proxy for the number of undocumented immigrants in our models, following a methodology that has been used in previous research (see Hagan and Palloni, 1998; Passel, 2005). However, because the annual estimates tend to be unstable, we could not include such a measure in our regression models. As Passel (2005: 10) argues, because of "inherent uncertainties in the residual technique, the difference in

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We also collected data on multiple variables that have been previously identified as having an effect on crime rates: population (logged); racial composition (percent of the population who are non-Latino black); age/sex structure (percent of the population who are males aged 18–24 years); educational attainment (percent of population 25 years and older with more than a high-school education); employment status (percent of individuals identified as in or not in the labor force who are unemployed); family formation (percent of total families headed by unmarried females with at least one child present); divorce rate (percent of the population aged 15 years and older who are divorced); poverty (percent of persons living in poverty); and the mean household income. The adult/child ratio has also been identified as a possible measure of informal social control. We created the adult/child ratio as the number of individuals aged 18 years and older divided by the number of individuals aged less than 18 years. In addition, residential stability has been linked to crime rates and is measured by those who moved (percent of the population who are living in a different house than they were 1 year ago) and those who live in owner-occupied housing units. Any regional effects are captured by dummy variables for the broad U.S. Census regions (South, West, and Northeast, with Midwest serving as the reference category).

We include two final control variables that have been identified as potentially important factors for explaining the dramatic crime drop in the United States in the 1990s—police size and a proxy for firearm availability (Levitt, 2004; Rosenfeld, 2002). Police size is measured as the total number of sworn officers per 1,000 population based on the 1994–2004 UCR program: Police Employee (LEOKA) Data. Researchers on firearms generally agree that, in the absence of accurate administrative data or representative survey data, the most reliable proxy measure of firearm availability is the proportion of suicide deaths where guns were used (Azrael, Cook, and Miller, 2004; Cook and Ludwig, 2006; Kleck, 2004).[12] We constructed such a proxy measure using data from the Multiple Cause of Death Mortality Data from the National Vital Statistics System of the National Center for Health Statistics.[13]

successive annual estimates of the unauthorized population is not a valid measure of growth." The development of more reliable estimates of this segment of the foreign-born population is certainly an important issue that should be addressed in future research.

12. Kleck (2004) reports that the suicide proxy measure is valid for cross-sectional research but advises against using any of the standard proxies for longitudinal analyses. In contrast, Azrael, Cook, and Miller (2004) as well as Cook and Ludwig (2006) conclude that the percentage of suicides involving guns is a useful proxy for both kinds of analyses.

13. Another factor that has been cited as a likely cause of both the rise and fall of violent crime is the operation of crack cocaine markets (Blumstein, Rivara, and

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Many of our covariates are highly correlated, which creates a collinearity issue. We therefore followed standard procedures and conducted principle component analysis on residential stability, percent female-headed household, percent poverty, percent with more than a high-school education, percent young males, mean household income, percent non-Latino black, percent unemployed, and the percent divorced. We repeated the factor analysis for each of the 11 time periods. An economic resources factor emerges from the analysis consistently. Variables that load highly (average factor loadings over the 11 years in parentheses) include the percent not in poverty (.81), percent with more than a high school education (.78), mean household income (.88), and percent female-headed households (.67).[14] With an average eigenvalue of 2.50, this explains, on average, 62.45 percent of the variation in the construct. We created an economic resources index using the factor scores.

Residential stability variables (percent owner occupied and percent of the population who moved) are also highly correlated. We therefore computed an index using the summation of the z-scores. All other variables are entered into the regression as single-item covariates. Finally, all variables (save the regional dummies) are expressed as first differences for change–change dynamic time-series models. Descriptive information is provided in appendix C.

STATISTICAL PROCEDURES

Unit–root tests indicate that MSA-specific crime rates are difference–stationary processes, so they are measured as annual change (Raffalovich,

Rosenfeld, 2000). In an effort to control for this variable, we collected data on arrest rates for the sale and manufacture of cocaine from the UCR Program County-Level Detailed Arrest and Offense Data for our study period. Unfortunately, after aggregating to the Metropolitan Statistical Area (MSA) level, we found that this measure contains significant missing data for strategic cases in a study of the effects of immigration. Specifically, data are not available for the seven Florida MSAs for 1996–2000. Alabama and South Carolina MSAs had missing data for 1994, Chicago failed to report data from 1995 on, and the Milwaukee MSA did not report arrest data for 1998–2000. Despite the missing data, we estimated equations for the reduced sample and found no significant effects of the cocaine variable on changes in the violent crime index or the rates for the specific violent offenses. The only substantive difference between models with and without the cocaine measure concerning the immigration variable is that the effect on the violent crime index, although still negative, falls below statistical significance. The significant negative effect on robbery rates remains in the analyses with the cocaine measure. See Ousey and Lee (2007) for evidence indicating that the effect of cocaine markets on homicide rates has attenuated over time.

14. To be consistent with the other factors in the resource index, the polarity of the female-headed household was adjusted. As originally constructed, the factor loading for female-headed households was –.67.

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1994). Because we have data for a relatively large number of MSAs (103) but a short time-span (11 years), we estimate random-effect variable parameter regression models (Goldstein, 1995; Longford, 1993; Snijders and Bosker, 1999):

$$\Delta C_{i,t} = B_{i,0} + \sum B_{ki} X_{kit} + \sum B_m Z_{mi} + \sum B_r X_{rit} * Z_{ri} + \varepsilon_{i,t} \quad (1)$$

where:

$\Delta C_{i,t}$ is annual change in the MSA-specific crime rate for MSA i in year t,

$X_{ki,t}$ are independent variables that vary across both MSAs and time,

Z_{mi} are independent variables that vary across MSAs but are constant over time,

$X_{ri}*Z_{ri}$ are cross-level interactions, and $\varepsilon_{i,t}$ is the stochastic error.

B_{ki} *and* B_m are parameters to be estimated.

$B_{i,0}$ *and* B_{ki} are assumed to have multivariate normal distributions across MSAs and the error process is initially assumed to be AR(1):

$$e_{i,t} = p_i\, e_{i,t-1} + v_{i,t} \quad (2)$$

We use full-information maximum likelihood to estimate all models. Regarding the data used in the current study, with 103 cross-sectional units and 10 time points after computing first differences, the number of cases for analyses would be 1,030 were data available for all variables. CPS data were not available for Orange County, CA, for 1994–1995, which reduces the N to 1,028. Data were also missing on police size for Worcester, MA, for the years 1994–1999. Given that police size serves as a control variable and not as a strategic independent or dependent variable, we replaced the missing values on police size with the average value for the years with data for Worcester. Analyses without imputed values (available upon request) yield highly similar results to those reported in the tables.

RESULTS

We begin the analysis with an examination of the trends in selected variables of primary interest for our sample of metropolitan areas, as depicted in figure 1. The top panel of the figure displays the trend in violent crime index averaged across all 103 MSAs for the 1994–2004 period. The data tell a familiar story. For the metropolitan areas in the sample, the rates of violent crime exhibit a dramatic decline throughout the 1990s, with some leveling off thereafter.

The trend for the immigrant concentration index, which is displayed in the bottom panel of figure 1, is virtually an inverted image of that for violent crime rates. Consistent with the pattern widely reported for the nation at large, the relative size of the Latino/foreign-born population shows a sharp and sustained increase during the 1990s, which continues

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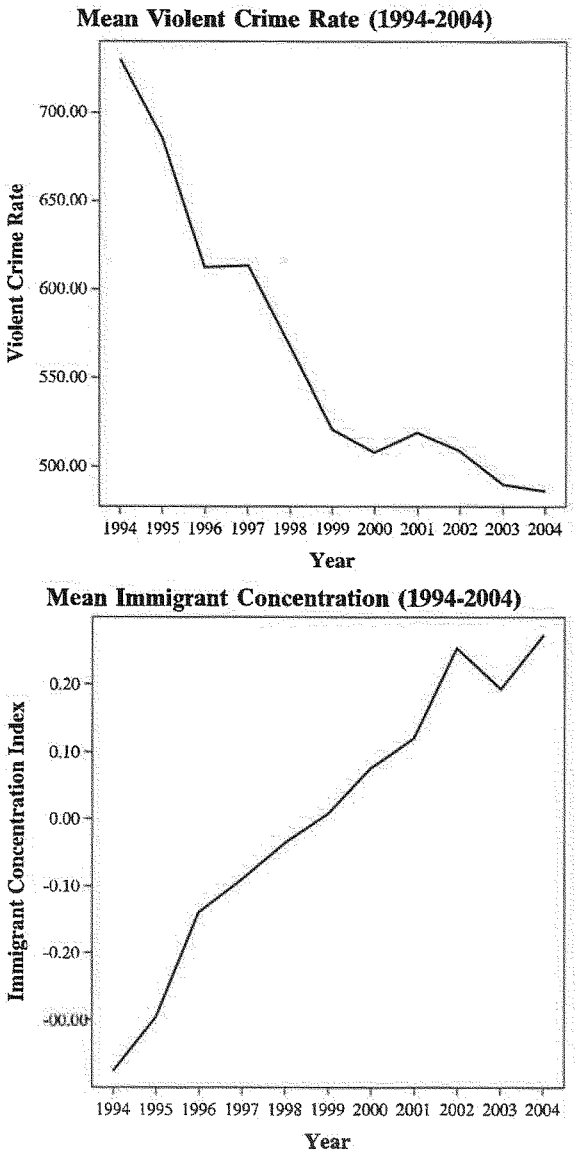
Figure 1. Longitudinal Trends in Violent Crime and Immigrant Concentration



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with a brief interruption into the twenty-first century. These simple graphs thus lend credibility to the hypothesis that the growth in immigration played a role in the recent crime decline in the United States.

Table 2 reports the results of the dynamic regression modeling. For each of our dependent variables, we report two equations. The first is a baseline model that includes only the impact of changes in immigrant concentration on changes in the violent crime rates. The second equation adds the control variables. The coefficients for the immigration variable are for the most part similar across models, and we thus focus our discussion on the results in the fully specified models.

The coefficients for the immigrant concentration index are always negative in sign, and they reach statistical significance for the violent crime index (–22.913, $p < .05$), the robbery rate (–9.223, $p < .001$), and aggravated assault (–13.253, $p < .05$). In other words, for these offenses, increases in the relative size of the Latino/foreign-born population are associated with significant decreases in criminal violence.

Although the immigration index yields statistically significant effects for the violent crime index, robbery rates, and rates of aggravated assault, our regression models indicate that the overall role of immigration for the crime decline is modest, accounting for just over 6 percent of the observed crime drop (6.16 percent). This number was derived by comparing the predicted values of the change scores for these offenses under two scenarios: 1) with the value of change in the immigration concentration index set at 0 (no change) and 2) with the value of change in immigration set at its mean pooled over the entire sample of MSAs/years. In these prediction models, all other variables have been set at their respective means. This effect is consistent across offense categories, where the changes in levels of violence also averaged about 6 percent.

The consistent impact of changing immigration, although modest with respect to the crime decline for a given year, is nevertheless nontrivial. Table 3 reports the predicted changes in the index rate of violent crime, the robbery rate, and the aggravated assault rate based on the equations in table 2 for differing levels of the immigration concentration index. As before, all other predictors are fixed at their means. The results show that those MSAs at the high end of the distribution for immigrant growth are estimated to have experienced a much greater drop in violent crime than those at the bottom end (contrast especially the predicted values at the tails of the distributions). For example, we observe that a change in immigration concentration at 2 standard deviations above the mean translates into an average of 40.5 fewer violent crimes (per 100,000), compared with a reduction of 8.1 violent crimes (per 100,000) in areas that experienced changes in immigrant concentration at 2 standard deviations below the mean. This pattern is consistent for both robbery and aggravated assault

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Table 2. Dynamic Regression of Violent Crime Rate and its Components (N = 1028)

	Change in Violent Crime Rate	Change in Violent Crime Rate	Change in Robbery Rate	Change in Robbery Rate	Change in Homicide Rate	Change in Homicide Rate
Change immigration concentration index	–20.01* (8.563)	–22.913* (8.871)	–8.218** (3.072)	–9.223** (3.176)	–.333# (.180)	–.296 (.186)
Change economic resources factor	—	–1.171 (6.774)	—	–2.997 (2.413)	—	.030 (.144
Change stability index	—	–2.164 (2.938)	—	–.165 (1.038)	—	.070 (.063)
Change percent non-Latino black	—	–.452 (.854)	—	.205 (.304)	—	.027 (.018)
Change percent young males	—	1.188 (2.080)	—	.150 (.740)	—	.013 (.044)
Change percent unemployed	—	2.729* (1.268)	—	1.437** (.444)	—	.071* (.028)
Change percent divorced	—	–.218 (1.590)	—	–.241 (.564)	—	.018 (.034)
Change adult/child ratio	—	–37.041 (113.53)	—	–56.552 (40.295)	—	–2.299 (2.422)
Change in population (ln)	—	–11.456 (35.053)		–12.386 (12.589)		–.848 (.732)
Change in percent of suicides by firearms	—	–.225 (.449)	—	–.145 (.157)	—	–.012 (.010)
Change in officers per 1,000 population	—	5.216 (7.608)	—	2.925 (2.739)	—	.099 (.157)
South	—	–9.466 (6.733)	—	–2.216 (2.552)	—	–.180 (.129)
West	—	–11.337 (7.350)	—	–2.381 (2.786)	—	–.062 (.140)
Northeast	—	2.197 (7.505)	—	–1.616 (2.843)	—	.127 (.143)
Constant	–22.94** (2.509)	–17.037** (5.470)	–8.866** (.960)	–6.942** (2.067)	–.303*** (.048)	–.254* (.105)
AR(1)	–.233*** (.030)	–.248*** (.030)	–.146*** (.031)	–.170*** (.032)	–.396*** (.029)	–.412*** (.029)
–2 log likelihood	12290.3	12277.9	10179.0	10159.3	4404.1	4385.3

NOTES: Standard errors are in parentheses.
# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

rates, which declined sharpest in areas with the largest increases in immigration concentration (–15.9 and –23.3 events per 100,000, respectively).

Turning to the control variables (see table 2), only the unemployment measure exhibits a significant effect on changes in rates of criminal

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Table 2 (continued). Dynamic Regression of Violent Crime Rate and its Components (N = 1028)

	Change in Aggravated Assault Rate	Change in Aggravated Assault Rate	Change in Rape Rate	Change in Rape Rate
Change immigration concentration index	–11.640#	–13.253*	–.077	–.319
	(6.129)	(6.357)	(.610)	(.631)
Change economic resources factor	—	2.236	—	–.165
		(4.844)		(.481)
Change stability index	—	–2.253	—	.029
		(2.095)		(.208)
Change percent non-Latino black	—	–.744	—	.001
		(.611)		(.061)
Change percent young males	—	1.024	—	–.193
		(1.487)		(.148)
Change percent unemployed	—	.735	—	.219*
		(.901)		(.090)
Change percent divorced	—	.075	—	–.018
		(1.135)		(.113)
Change adult/child ratio	—	17.740	—	3.296
		(81.086)		(8.058)
Change in population	—	1.023	—	2.836
		(25.145)		(2.493)
Change in percent of suicides by firearms	—	–.016	—	–.032
		(.318)		(.032)
Change in officers per 1,000 population	—	1.120	—	.856
		(5.464)		(.542)
South	—	–6.492	—	–.504
		(4.924)		(.484)
West	—	–8.585	—	–.243
		(5.376)		(.528)
Northeast	—	3.204	—	.479
		(5.488)		(.539)
Constant	–13.190***	–9.330*	–.570**	–.549
	(1.824)	(3.996)	(.180)	(.393)
AR(1)	–.211***	–.218***	–.225***	–.233***
	(.030)	(.031)	(.030)	(.031)
–2 log likelihood	11,600.4	11,588.7	68,56.3	6,840.2

NOTES: Standard errors in parentheses.
# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

violence. The coefficient for this variable is consistently positive and is statistically significant in all models with the exception of that for aggravated assault. In other words, for our sample of metropolitan areas, decreasing unemployment is generally associated with falling violent crime rates. This finding runs counter to some previous assessments of the role of unemployment in the crime decline. In his review of the literature, Levitt (2004) concludes that the strong economy in the 1990s might have made a modest contribution to the drop in property crimes, but it had little effect on

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Table 3. Predicted Change in Violent Crime Rates at Differing Levels of Change in the Immigration Index[a]

Level of Change in the Immigration Concentration Index	Offense Type		
	Violent Crime Index	Robbery	Aggravated Assault
+2 SD	–40.5	–15.9	–23.3
+1 SD	–32.3	–12.3	–18.6
Mean	–24.2	–9.4	–13.9
–1 SD	–16.2	–6.2	–9.3
–2 SD	–8.1	–2.9	–4.6

[a] All other independent variables fixed at their means.

changes in violent crime.[15] Arvanites and DeFina (2006) arrived at a different conclusion about the importance of a strong economy for crime reduction, but they also detect no impact of unemployment rates per se. Using state-level data and a broader measure of economic activity than the conventional unemployment rate (i.e., the inflation adjusted gross state domestic product), they find that business cycle growth reduced robbery rates (and other index property crime rates). The unemployment rate, in contrast, exerts no significant effect on changes in crime rates in their analyses. Rosenfeld and Fornango (2007) similarly conclude that general economic conditions contributed to the crime drop but that the relevant conditions are not adequately captured by the unemployment rate. In their pooled, cross-sectional time-series analyses for major regions in the United States between 1970 and 2003, they find a significant effect of an indicator of consumer confidence on robbery rates (rising confidence accompanies decreases in robbery rates) but a negative effect of the unemployment rate, which they interpret with reference to "opportunity" dynamics (2007: 751).

The reasons for these discrepant results concerning the impact of changing levels of unemployment on changing crime rates across studies are unclear. They could reflect differing units of analysis (e.g., MSAs versus states and regions) and/or differences in model specification. In any event,

15. Conklin (2003: 172) assigns a greater role to economic factors, suggesting that "the improving employment situation [in the 1990s] reinforced the downward trend in crime rates, even if it did not cause the initial decline." Rosenfeld (2002: 30) similarly proposes that "there seems little doubt that the record drops in unemployment rates, including those for minority teenagers during the economic boom of the 1990s, contributed in some way to the crime decline over the same period."

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our findings for unemployment in conjunction with those for related economic indicators in recent studies suggest that continued inquiry into the impact of macroeconomic conditions on crime rates is amply justified (see Rosenfeld and Fornango, 2007: 765).

The analyses thus far have operationalized the relative size of the immigrant population by means of the immigration concentration index to be consistent with past research. The two components of the index—percent foreign born and percent Latino—are of course conceptually distinct. It is thus instructive to disaggregate the index and to reestimate the regressions to determine whether one or the other component seems to be "driving" the effects observed in table 2.

The results of the disaggregated analyses, reported in table 4, indicate that changes in both percent foreign born and percent Latino are significantly associated with changes in robbery rates, which is consistent with the findings for the composite index. The results for the violent crime index also suggest that each component contributes to the crime decline, although the measure of percent Latino only reaches a level of significance at the .10 level with a two-tailed test (.05 with a one-tailed test). The pattern for aggravated assault is different. For this offense, the coefficient for percent Latino falls below statistical significance, whereas the coefficient for percent foreign born is only significant at the .10 level (two-tailed test). Two conclusions are suggested by the disaggregated analyses. The use of the composite index tends to offer greater support for claims of a salutary effect of immigration on changing levels of criminal violence than the use of the components in isolation. In addition, the effects of immigration seem to be most robust for the violent offense of robbery.[16]

Finally, to illustrate the implications of the research design, we have conducted a standard cross-sectional analysis to compare the results with those of our dynamic model. For these purposes, we computed the average rate smoothed over a 3-year period centered on the 2000 decennial census year. Given the highly skewed nature of the violent crime rates expressed as levels rather than as difference scores, we transformed them for the cross-sectional analyses by applying natural logs. We then used point estimates of the predictor variables for the smoothed 1999–2001 period to predict the logged violent crime rates.

The results of the cross-sectional analyses are reported in table 5. Considering the control variables first, the picture is generally consistent with

16. In a separate analysis (available upon request), we also estimated models with random intercepts and/or slopes, and models with intercepts, slopes, AR(1) parameters, and error variances that differed among regions. In no case did these variable parameter models have significant random effects or significantly affect model log-likelihoods. The effects in table 2 do not differ among MSAs.

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Table 4. Dynamic Results with Disaggregated Immigrant Concentration Components (N = 1,028)

	Change in Violent Crime Rate	Change in Violent Crime Rate	Change in Robbery Rate	Change in Robbery Rate	Change in Homicide Rate	Change in Homicide Rate	Change in Aggravated Assault Rate	Change in Aggravated Assault Rate	Change in Rape Rate	Change in Rape Rate
Change percent foreign born	−2.850*	—	−1.040*	—	−.042	—	−1.654#	—	−.077	—
	(1.227)		(.440)		(.026)		(.879)		(.087)	
Change percent Latino	—	−1.844#	—	−.865*	—	−.018	—	−1.052	—	.020
		(1.064)		(.378)		(.023)		(.760)		(.075)
Change economic resources factor	.405	−.895	−2.312	−3.039	.048	.040	3.150	2.410	−.156	−.103
	(6.716)	(6.844)	(2.394)	(2.437)	(.143)	(.145)	(4.802)	(4.892)	(.477)	(.485)
Change stability index	−2.112	−1.750	−.109	−.035	.069	.076	−2.224	−2.017	.022	.040
	(2.941)	(2.935)	(1.040)	(1.037)	(.063)	(.063)	(2.096)	(2.092)	(.208)	(.208)
Change percent non-Latino black	−.275	−.326	.288	.237	.029	.030	−.641	−.667	<.000	.010
	(.846)	(.857)	(.301)	(.305)	(.018)	(.018)	(.604)	(.612)	(.060)	(.061)
Change percent young males	.929	1.449	.056	.270	.009	.016	.874	1.174	−.200	−.197
	(2.084)	(2.091)	(.742)	(.743)	(.044)	(.044)	(1.490)	(1.494)	(.148)	(.148)
Change percent unemployed	2.666*	2.557*	1.409**	1.373**	.071*	.069*	.694	.640	.221*	.212*
	(1.268)	(1.267)	(.444)	(.443)	(.028)	(.028)	(.900)	(.900)	(.090)	(.090)
Change percent divorced	−.182	−.027	−.215	−.158	.017	.022	.094	.212	−.022	−.012
	(1.591)	(1.588)	(.565)	(.563)	(.034)	(.034)	(1.136)	(1.133)	(.113)	(.113)
Change adult/child ratio	−25.885	−55.732	−52.825	−64.764	−2.120	−2.512	24.210	6.996	3.715	3.275
	(113.89)	(113.81)	(40.467)	(40.367)	(2.428)	(2.426)	(81.325)	(81.236)	(8.076)	(8.064)

NOTES: Standard errors are in parentheses.

# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

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Table 4. Dynamic Results with Disaggregated Immigrant Concentration Components (N = 1,028) (continued)

	Change in Violent Crime Rate	Change in Violent Crime Rate	Change in Robbery Rate	Change in Robbery Rate	Change in Homicide Rate	Change in Homicide Rate	Change in Aggravated Assault Rate	Change in Aggravated Assault Rate	Change in Rape Rate	Change in Rape Rate
Change in population	−10.681	−19.295	−12.403	−15.387	−.819	−.960	1.505	−3.521	2.980	2.697
	(35.155)	(34.944)	(12.635)	(12.552)	(.734)	(.728)	(25.215)	(25.052)	(2.499)	(2.481)
Change in percent of suicides by firearms	−.237	−.226	−.149	−.145	−.012	−.012	−.022	.017	−.032	−.032
	(.449)	(.449)	(.157)	(.157)	(.010)	(.010)	(.319)	(.319)	(.032)	(.032)
Change in percent of officers per 1,000 population	5.012	5.033	2.838	2.849	.096	.097	1.011	1.003	.857	.844
	(7.609)	(7.623)	(2.742)	(2.744)	(.157)	(.158)	(5.464)	(5.470)	(.541)	(.542)
South	−9.469	−9.919	−2.242	−2.387	−.179	−.186	−6.493	−6.757	−.496	−.514
	(6.727)	(6.747)	(2.548)	(2.562)	(.129)	(.129)	(4.922)	(4.928)	(.483)	(.484)
West	−11.600	−11.764	−2.520	−2.528	−.064	−.069	−8.737	−8.842	−.238	−.262
	(7.340)	(7.366)	(2.781)	(2.797)	(.140)	(.140)	(5.370)	(5.380)	(.528)	(.528)
Northeast	2.263	1.640	−1.614	−1.841	.129	.120	3.243	2.878	.489	.469
	(7.500)	(7.520)	(2.840)	(2.854)	(.144)	(.143)	(5.486)	(5.492)	(.539)	(.539)
Constant	−17.208**	−17.389**	−7.029**	−7.058**	−.256*	−.260*	−9.426*	−9.534*	−.544	−.563
	(5.463)	(5.481)	(2.064)	(2.075)	(.105)	(.105)	(3.992)	(3.999)	(.392)	(.393)
AR(1)	−.250***	−.246***	−.173***	−.166***	−.412***	−.413***	−.220***	−.218***	−.233***	−.232***
	(.030)	(.030)	(.032)	(.032)	(.029)	(.029)	(.031)	(.031)	(.031)	(.031)
−2 log likelihood	12,279.2	12,281.6	10,162.1	10,162.5	4,464.0	4,387.1	11,589.5	11,591.1	6,839.7	6,840.4

NOTES: Standard errors are in parentheses.

# $p < .10$ (two-tailed tests); * $p < .05$; ** $p < .01$; *** $p < .001$.

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Table 5. Cross-Sectional Regression of Violent Crime Rate and its Components (N = 103)

	ln Violent Crime Rate		ln Robbery Rate		ln Homicide Rate		ln Aggravated Assault Rate		ln Rape Rate (n=102)	
Immigration concentration index	.033	(.038)	.065	(.041)	.040	(.039)	.034	(.044)	−.099*	(.044)
Economic resources factor	−.152*	(.059)	−.113#	(.063)	−.191**	(.061)	−.171*	(.068)	−.068	(.068)
Stability index	−.040	(.035)	−.014	(.037)	−.022	(.036)	−.035	(.040)	−.127**	(.040)
Percent non-Latino black	.015*	(.006)	.033***	(.006)	.033***	(.006)	.006	(.007)	−.005	(.007)
Percent young males	−.042	(.046)	−.032	(.048)	.007	(.047)	−.045	(.052)	−.042	(.052)
Percent unemployed	−.014	(.025)	−.011	(.027)	.004	(.026)	−.015	(.029)	−.031	(.029)
Percent divorced	.030	(.026)	.039	(.028)	.027	(.027)	.026	(.030)	.026	(.030)
Adult/child ratio	.190	(1.632)	1.833	(1.735)	3.028#	(1.680)	−.298	(1.879)	−2.930	(1.862)
ln population	.046	(.066)	.105	(.070)	.157*	(.068)	.041	(.076)	−.072	(.077)
Percent of suicides by firearms	−.009	(.006)	−.007	(.006)	.009	(.006)	−.008	(.007)	−.009	(.007)
Officers per 1,000 population	.088	(.055)	.141*	(.059)	.117*	(.057)	.069	(.064)	.081	(.064)
South	.338*0	(.152)	−.097	(.161)	−.146	(.156)	.592**	(.175)	.303#	(.173)
West	.301*	(.149)	.219	(.158)	.189	(.153)	.395*	(.172)	.199	(.171)
Northeast	−.250	(.158)	−.349*	(.168)	−.320#	(.163)	−.151	(.182)	−.403	(.185)
Constant	5.225***	(1.619)	1.591	(1.721)	−4.302*	(1.667)	5.199**	(1.865)	6.666**	(1.948)
R^2	.500		.517		.619		.475		.388	

NOTES: Standard errors are in parentheses.
# $p < .10$ (two-tailed test); * $p < .05$; ** $p < .01$; *** $p < .001$.

theoretical expectations and past research. MSAs with high levels of economic resources tend to have relatively low violent crimes rates, whereas areas with large non-Latino black populations and those located in the South and West have relatively high violent crime rates.

The key findings in table 5 are those for the immigration concentration index. The coefficients for this variable are statistically nonsignificant and positive in all case but one—the rape rate. Our results are consistent with the bulk of previous research in that there is little support for the claim that immigration is positively associated with levels of most forms of criminal violence for the year 2000 (see Martinez, 2002, 2006; Sampson, 2006). Our dynamic models add continued support for this conclusion, as *increases* in the relative size of the Latino/foreign-born population during a period of exceptionally high immigration are significantly associated with *decreases* in the violent crime index, robbery rates, and aggravated assault rates.

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SUMMARY AND CONCLUSIONS

The largely unexpected, yet precipitous, reductions in violent crime rates the United States has experienced over the past two decades have received much scholarly and popular attention. Despite the increased attention to this topic, quantifying the factors most responsible for the observed crime drop has proven challenging. Over this same period, the size of the foreign-born population in this country has grown exponentially, prompting some to hypothesize that the two trends are more than merely coincidental. Specifically, Sampson and colleagues have argued that immigration represents a credible, although counterintuitive, social process that has contributed to the reductions in crime (Sampson, 2006; Sampson and Bean, 2006). The objective of this research has been to assess whether there was a meaningful temporal association between immigration and crime. More specifically, we have examined the degree to which *changes* in immigration were associated with *changes* in levels of criminal violence across metropolitan areas.

The finding of primary interest in this study is that of an inverse relationship between changes in immigration and changes in the violent crime index, the robbery rate, and the aggravated assault rate, net of other demographic and social structural controls. This effect is particularly pronounced and robust for the offense of robbery. Although the contribution of immigration to the overall crime drop for these offenses is modest, metropolitan areas exhibiting high levels of immigration are estimated to have fared much better than those with comparatively low growth in the size of the immigrant population. Considered as a whole, our results support the hypothesis of Sampson and colleagues that the broad reductions in crime over recent years are partially attributable to increases in diversity and immigration (see Sampson, 2008; Sampson and Bean, 2006).[17]

17. With a nonexperimental design, it is possible that unmeasured exogenous factors may account for both the increase in immigration and the reduction in violence. For example, improvements in the economy are a likely factor that may attract immigrants (because of enhanced employment prospects) and partially mitigate conditions conducive to violent crime. In such a scenario, then, the relationship between immigration and violence is an artifact of broader social processes. We do not perceive this as a viable explanation for our findings. The immigration effect in our models is net of unemployment, which is significantly related to changing criminal violence in the expected positive direction. We also control for changes in a wide range of socioeconomic characteristics, including poverty and mean income, in our deprivation factor. We thus concur with Sampson (2008: 31), who argues that "economics can't easily explain the story . . . [economic factors such as] real wages were declining and inequality increasing throughout the 1990s by most accounts, which should have produced increases in crime." Similarly, Sampson also notes that rates of immigration have remained high early this decade even after the sharp declines in the stock market.

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More generally, we concur with Sampson and Bean's (2006: 21) argument that "in today's world it is no longer tenable to assume that immigration and diversity automatically lead to social disorganization and consequently crime." Yet we recognize that this position is at odds with both popular perceptions and traditional theoretical accounts of the relationship between immigration and crime. Consistent with the findings presented in previous research, the results from this study illustrate the inability of social disorganization theory, at least in its classical formulation, to explain the immigration/crime relationship in a contemporary urban context (see Lee, Martinez, and Rosenfeld, 2001; Martinez, 2002; Stowell, 2007).

Given that the current wave of immigration is unlike those in past eras, in terms of both its volume and its racial/ethnic diversity, perhaps this limitation is not entirely unexpected. On their own, the results from this study may suggest that the inverse impact of immigration may be driven by the unique characteristics of the foreign-born population currently entering the United States. That is to say, the streams of immigrants entering today are somehow different (i.e., less criminogenic) than those of past eras. We do not believe that the weight of the evidence supports such a conclusion. Rather, we agree with Hagan and Palloni (1999: 617) who argue that the notion of the immigrant/crime link continues to be an "area of popular misperception and policy distortion," as it has been since the early 1900s (see also Martinez, 2002; Rosales, 1999). Taking a wider perspective, it seems that the results presented in the current study are consistent with many of its predecessors in finding that the crime-producing effects of immigration are overstated.

One strength of the current study, we believe, is that it is one of the first to examine the dynamic influence of immigration on violent crime in the 1990s. As such, our findings bear more directly on claims about causal processes, which to date have remained largely speculative. The relationship between immigration and crime is complex, however, and this study also leaves unanswered several important questions pertaining to some intricacies of this association. In the following discussion, we identify limitations of the current study and recommend topics to be pursued in future research on this subject.

To a large extent, by incorporating a longitudinal analytical design and a national sample, this study extends current knowledge on "whether" immigration contributed to reductions in violent crime, but it does not provide much insight into "how" this process operates. The mechanisms through which immigration may buffer against levels of violence can be explained in many ways. Perhaps the most straightforward account entails a purely compositional effect; this effect is highly consistent with the immigrant selectivity thesis. For reasons enumerated above, immigrants may be

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less prone to criminal violence than the native born. Violent crime rates should accordingly fall as the relative size of the immigrant population grows.

Another possibility, however, is that immigration alters social conditions in a manner that reduces levels of offending among immigrants and the native born alike. Most notably, it may be that immigrant communities are characterized by higher levels of collective efficacy, which in turn results in lower levels of crime (Sampson and Raudenbush, 1999; Sampson, Raudenbush, and Earls, 1997). Cross-sectional studies that have made use of the PHDCN data collected in Chicago seem to support this notion of a contextual effect (see Morenoff and Astor, 2006; Sampson and Bean, 2006). Current knowledge on this topic will be enhanced by research that examines the impact of immigration on levels of collective efficacy over time.

Another potential contextual mechanism through which immigration may lead to lower levels of crime is by influencing structural changes in the areas into which immigrants settle. In other words, it may be that immigration may promote positive changes in the "general character" of their communities, which would result in lower levels of crime (see Bursik, 2006: 26). This point is consistent with the "Immigration Revitalization Perspective" described by Martinez and colleagues, which argues that "immigration may encourage new forms of social organization that mediate disorganizing influences in inner city communities" (Lee, 2003: 131; see also Martinez, 2006). Following the logic of this perspective, it would be expected that immigration would be inversely related to the macrolevel conditions known to be associated with violence (i.e., poverty and unemployment).

We note that an assessment of the respective compositional and contextual contributions made by immigration to the reduction in criminal violence will require different data and a different design from that used in the current study. A standard technique for demonstrating contextual effects is to disaggregate the data according to relevant characteristics of the population. Thus, in the current case, differentiating between the compositional and contextual explanations for the observed association between immigration and violent crime would require data on offending specific to immigrant status (immigrants versus the native born). Although collecting the necessary data for such an assessment for relatively large and representative samples is a formidable task, analyses based on such data would shed considerable light on the mechanisms linking immigration and criminal violence.

The noted shortcomings notwithstanding, our study represents a formal test of the somewhat controversial hypothesis that growing immigration contributed meaningfully to the drop in violent crime. Although the

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impact of immigration on receiving societies has been of interest to criminologists and social scientists dating back more than a century, the arrival of newcomers continues to be perceived in a negative light. Despite the strong rhetoric, few researchers have rigorously tested the dynamic causal connection between immigration and crime. Our study shows that the contemporary surge in immigration has been associated with the heralded decline in selected forms of criminal violence. Indeed, although it is still not counted among the "usual suspects" used to account for the crime drop, the current study underscores the fact that immigration is a viable explanatory factor (Sampson and Bean, 2006: 21). It is our hope that these results can be used to inform both public policy discussions and academic research initiatives in the years ahead.

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Jacob I. Stowell is an assistant professor in the Department of Criminal Justice and Criminology at the University of Massachusetts–Lowell. His research interests include immigration, immigrant mortality, and neighborhood effects. His current research focuses on studying the spatial and temporal impacts of immigration on levels of violent criminal behavior. Recent articles have been published in the *American Journal of Sociology*, the *ANNALS of the Academy of Political and Social Science*, and *Social Science Quarterly*.

Steven F. Messner is the Distinguished Teaching Professor of Sociology, University at Albany, State University of New York. His research focuses on social institutions and crime, understanding spatial distributions and trends in crime, as well as crime and social control in China. In addition to his publications in professional journals, he is coauthor of *Crime and the American Dream* and *Perspectives on Crime and Deviance,* and he is coeditor of *Theoretical Integration in the Study of Deviance and Crime* and *Crime and Social Control in a Changing China.*

Kelly F. McGeever is a PhD candidate in the Department of Sociology at the University at Albany, State University of New York. Her research interests include communities and crime, Global Information Systems applications and the spatial analysis of crime, and sentencing outcomes. Her dissertation tests "not in my backyard" (NIMBY) arguments about

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the negative effects of group quarters, such as halfway houses and correctional facilities, on neighborhood quality. Her most recent publication appeared in the *Journal of Quantitative Criminology*.

Lawrence E. Raffalovich is an associate professor in the Department of Sociology at the University at Albany, State University of New York. His research interests include social inequality, economic sociology, and quantitative research methods. Recent publications include (with Glenn D. Deane, David Armstrong, and Hui-shien Tsao) "Model selection procedures in social research: Monte-Carlo simulation results" in the *Journal of Applied Statistics* 35:1093–114 and (with Elena Vesselinov) "The power of property in comparative perspective" in *Research in Social Stratification and Mobility* 20:361–84.

Appendix A. Metropolitan Areas

Akron, OH
Albany-Schenectady-Troy, NY
Albuquerque, NM
Allentown-Bethlehem-Easton, PA
Ann Arbor, MI
Atlanta, GA
Austin-San Marcos, TX
Bakersfield, CA
Baltimore, MD
Baton Rouge, LA
Bergen-Passaic, NJ
Birmingham, AL
Boston, MA-NH
Buffalo-Niagara Falls, NY
Charleston-North Charleston, SC
Charlotte-Gastonia-Rock Hill, NC-SC
Chicago, IL
Cincinnati, OH-KY-IN
Cleveland-Lorain-Elyria, OH
Colorado Springs, CO
Columbia, SC
Columbus, OH
Dallas, TX
Dayton-Springfield, OH
Denver, CO
Detroit, MI
El Paso, TX
Fort Lauderdale, FL
Fort Wayne, IN
Fort Worth-Arlington, TX
Fresno, CA
Gary, IN
Grand Rapids-Muskegon-Holland, MI
Greensboro—Winston-Salem—High Point, NC
Greenville-Spartanburg-Anderson, SC
Harrisburg-Lebanon-Carlisle, PA
Hartford, CT
Honolulu, HI
Houston, TX
Indianapolis, IN
Jacksonville, FL
Jersey City, NJ
Kansas City, MO-KS
Knoxville, TN
Las Vegas, NV-AZ
Little Rock-North Little Rock, AR
Los Angeles-Long Beach, CA
Louisville, KY-IN
Tucson, AZ
Tulsa, OK
Vallejo-Fairfield-Napa, CA
Ventura, CA
Washington, DC-MD-VA-WV
McAllen-Edinburg-Mission, TX
Memphis, TN-AR-MS
Miami, FL
Middlesex-Somerset-Hunterdon, NJ
Milwaukee-Waukesha, WI
Minneapolis-St. Paul, MN-WI
Mobile, AL
Monmouth-Ocean, NJ
Nashville, TN
Nassau-Suffolk, NY
New Haven-Meriden, CT
New Orleans, LA
New York, NY
Newark, NJ
Norfolk-Virginia Beach-Newport News, VA-NC
Oakland, CA
Oklahoma City, OK
Omaha, NE-IA
Orange County, CA
Orlando, FL
Philadelphia, PA-NJ
Phoenix-Mesa, AZ
Pittsburgh, PA
Portland-Vancouver, OR-WA
Providence-Fall River-Warwick, RI-MA
Raleigh-Durham-Chapel Hill, NC
Richmond-Petersburg, VA
Riverside-San Bernardino, CA
Rochester, NY
Sacramento, CA
St. Louis, MO-IL
Salt Lake City-Ogden, UT
San Antonio, TX
San Diego, CA
San Francisco, CA
San Jose, CA
Sarasota-Bradenton, FL
Scranton—Wilkes-Barre—Hazleton, PA
Seattle-Bellevue-Everett, WA
Springfield, MA
Stockton-Lodi, CA
Syracuse, NY
Tacoma, WA
Tampa-St. Petersburg-Clearwater, FL
Toledo, OH
West Palm Beach-Boca Raton, FL
Wichita, KS
Wilmington-Newark, DE-MD
Worcester, MA-CT
Youngstown-Warren, OH

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Appendix B. Correlation of Decennial Census Data and CPS Data for 103 Metropolitan Areas, 2000

	Correlation
Total Population	.997***
Racial/Ethnic Composition	
% Non-Latino white	.956***
% Non-Latino black	.933***
% Latino	.976***
% Asian	.971***
Nativity Composition	
% Foreign born	.950***
% Recent immigrant	.895***

*$p < .05$; **$p < .01$; ***$p < .001$.

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Appendix C. Descriptive Statistics (N = 1,131)[a]

	Mean	Standard Deviation	% Change 1994–2004
Dependent Variables (per 100,000 Population)			
Violent crime rate	567.61	266.86	–33.37
Robbery rate	183.24	104.16	–38.32
Homicide rate	6.55	4.35	–37.49
Aggravated assault rate	348.71	183.73	–31.74
Rape rate	29.12	12.74	–17.68
Independent Variables[b]			
Percent first-generation immigrant	11.38	9.92	52.16
Percent Latino	13.06	16.27	27.13
Percent in poverty	12.11	5.86	–19.67
Percent with less than a college degree	72.88	5.16	–7.79
Household income	$53,487.99	$11,671.85	51.86
Percent female-headed household	15.20	4.77	–10.26
Percent home ownership	65.82	9.44	10.21
Percent moved in the last 5 years	16.17	5.16	–19.70
Population	1,648,900	1,609,390	21.07
Percent non-Latino black	13.14	10.16	–7.15
Percent young males	4.85	1.37	–.10
Percent unemployed	5.46	2.66	–16.05
Percent divorced	9.56	2.40	5.32
Adult/child ratio	.74	.04	1.48
Percent guns used in suicide	51.61	13.03	–16.53
Number of officers per 1,000 population	2.30	.93	29.09
Constant (N = 103)	**Frequency**		
Northeast	22		
South	36		
Midwest	21		
West	24		

NOTES: Please see text for detailed information about index construction.

[a] Because of the use of first differences, the N for the regression analyses is 1,028.

[b] The first eight independent variables listed are the components used in three indices.

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Immigrant Sanctuary Policies and Crime-Reporting Behavior: A Multilevel Analysis of Reports of Crime Victimization to Law Enforcement, 1980 to 2004

Ricardo D. Martínez-Schuldt[a] **and Daniel E. Martínez**[b]

Abstract

Sanctuary jurisdictions have existed in the United States since the 1980s. They have recently reentered U.S. politics and engendered contentious debates regarding their legality and influence on public safety. Critics argue that sanctuary jurisdictions create conditions that threaten local communities by impeding federal immigration enforcement efforts. Proponents maintain that the policies improve public safety by fostering institutional trust among immigrant communities and by increasing the willingness of immigrant community members to notify the police after they are victimized. In this study, we situate expectations from the immigrant sanctuary literature within a multilevel, contextualized help-seeking framework to assess how crime-reporting behavior varies across immigrant sanctuary contexts. We find that Latinos are more likely to report violent crime victimization to law enforcement after sanctuary policies have been adopted within their metropolitan areas of residence. We argue that social policy contexts can shift the nature of help-seeking experiences and eliminate barriers that undermine crime victims' willingness to mobilize the law. Overall, this study highlights the unique role social policy contexts can serve in structuring victims' help-seeking decisions.

Keywords

sanctuary policies, Latinas/Latinos, crime reporting, National Crime Victimization Survey (NCVS)

Sanctuary jurisdictions are geographic areas, such as cities, counties, or states, with policies that attempt to limit cooperation between local officials, such as service sector bureaucrats or law enforcement, and the federal government in immigration enforcement (Collingwood and Gonzalez O'Brien 2019; Kittrie 2006; Martínez-Schuldt and Martínez 2019; Menjívar et al. 2018). Sanctuary jurisdictions have existed in the United States since the 1980s, but the past few years have seen a rise in contentious political debates regarding their legality and influence on public safety (Martínez, Martínez-Schuldt, and

[a]University of Notre Dame
[b]University of Arizona

Corresponding Author:
Ricardo D. Martínez-Schuldt, University of Notre Dame, 4053 Jenkins Nanovic Hall, Notre Dame, IN 46556
Email: rmarti23@nd.edu

Cantor 2018). In 2017, for example, 150 bills related to immigrant sanctuaries were introduced into state legislatures, with a majority of the bills (59 percent) being restrictive or anti-sanctuary in some form (Collingwood, El-Khatib, and Gonzalez O'Brien 2019). Drawing on a larger politicized discourse that links immigration to crime, critics argue that sanctuary jurisdictions violate federal law and create criminogenic conditions that threaten local communities (see, e.g., Executive Order 13768).

Proponents of the policies, including mayors, city council members, sheriffs, police chiefs, and academics, maintain that immigrant sanctuaries actually enhance public safety by increasing the likelihood that members of immigrant communities report crime victimization to local police or cooperate with law enforcement investigations (Kittrie 2006). Higher levels of cooperation and trust in law enforcement emerge because sanctuary policies counter or prevent legal cynicism from developing within immigrant communities (Kittrie 2006; Martinez et al. 2018). Legal cynicism, or the belief that legal institutions and the criminal justice system are illegitimate, inhibits crime victims and witnesses who are members of immigrant communities from interacting with law enforcement due to perceptions that doing so will render them, their friends, their families, or fellow community members vulnerable to deportation (Kirk et al. 2012; Menjívar et al. 2018; Theodore and Habans 2016).

Research shows that the U.S. immigration enforcement system fosters widespread legal cynicism in immigrant communities (Theodore and Habans 2016; Zatz and Smith 2012). Since the 1990s, the immigration system has undergone a process of devolution, whereby the federal government has granted local officials elevated powers and expanded roles in immigration enforcement (Abrego et al. 2017; Armenta 2017; Coleman 2007; Hagan, Eschbach, and Rodriguez 2008; Kubrin 2014; Meissner et al. 2013). Members of immigrant communities, regardless of their immigration status, have been subject to greater scrutiny and social control at the local level (Kanstroom 2007), which has led to allegations of discrimination and racial profiling during routine traffic stops (Capps et al. 2011; Meissner et al. 2013) and the widespread confinement of immigrants, especially black and Latino men, in immigrant detention centers with frequent reports of mistreatment and abuse (Abrego et al. 2017; Golash-Boza and Hondagneu-Sotelo 2013; Hernández et al. 2018; Phillips, Hagan, and Rodriguez 2006).

Mutual trust and cooperation between residents and local institutions like law enforcement are integral to the safety and well-being of communities (Desmond, Papachristos, and Kirk 2016; Skogan and Frydl 2004). A primary way people mobilize the law and receive justice is by notifying law enforcement of criminal victimization experiences (Black 1973). The state's formal mechanisms of social control often hinge on the criminal justice system reacting to denizens' reports of crime (Gottfredson and Gottfredson 1988; Reiss 1992). Furthermore, the mobilization of formal control through the criminal justice system can enhance public safety in communities where informal social control mechanisms are inhibited by weak private and parochial social networks (Carr 2003). In the absence of informal and formal mechanisms of control, violent subcultures promoting extralegal retribution may develop and further undermine public safety (Anderson 1999; Sampson 2012). Immigrant and nonimmigrant members of communities thus stand to benefit from an overall increase in trust and cooperation between residents and local institutions.

Previous studies have only speculated as to whether sanctuary policies build institutional trust between immigrant community members and law enforcement (Gonzalez, Collingwood, and El-Khatib 2017; Lyons, Vélez, and Santoro 2013; Martínez-Schuldt and Martínez 2019; Wong 2017). Rather than directly analyze patterns in attitudes or behaviors indicative of institutional trust, prior research has assessed if sanctuary policies are directly related to county-, city-, or state-level crime

patterns (Gonzalez et. al 2017; Kubrin and Bartos 2020; Wong 2017) or if they moderate the crime-buffering effects of immigration (Lyons et al. 2013; Martínez-Schuldt and Martínez 2019). Researchers have not yet directly examined if crime victims' probabilities of reporting crime incidents to law enforcement systematically vary across immigrant sanctuary policy contexts. Such evidence would suggest that levels of institutional trust are higher in immigrant sanctuary contexts.

We address this gap in the research by examining 25 years of data (1980 to 2004) on more than 35,000 incidents of violent crime victimization and 135,000 incidents of property crime victimization. We merge a variety of data sources, including the National Crime Victimization Survey (NCVS): MSA Data, to construct a longitudinal and multilevel dataset of crime victimization incidents nested in the core counties of the 40 largest U.S. metropolitan statistical areas (MSAs). Two central research questions guide our analyses. First, *are crime victims more likely to report victimization in the context of sanctuary policies?* Second, given the conjecture in the literature, *are the macro-level effects of sanctuary policies exclusive to ethno-racial minority victims who are most likely to be socially connected to immigrant communities*?

This article has broader theoretical implications for the study of victims' help-seeking decisions (Xie and Baumer 2019a). A large body of evidence demonstrates that incident- and individual-level characteristics consistently shape decisions to report crime victimization to the police (Baumer and Lauritsen 2010; Felson et al. 2002; Gottfredson and Hindelang 1979; Skogan 1984; Xie and Baumer 2019a). Fewer studies directly assess how socio-environmental conditions structure crime-reporting behavior (but see Baumer 2002; Desmond et al. 2016; Gutierrez and Kirk 2017; Xie and Baumer 2019a), which has led to calls for greater attention to the macro-level contexts within which victimization occurs (Goudriaan and Nieuwbeerta 2007; Xie and Baumer 2019b). We contribute to this line of investigation by demonstrating how social policy contexts can interact with victims' personal characteristics to structure help-seeking behavior. We argue that social policy contexts can shift the nature of help-seeking experiences and eliminate barriers that undermine crime victims' willingness to mobilize the law.

THEORETICAL INSIGHTS ON DECISIONS TO REPORT CRIME VICTIMIZATION

Micro-Level Explanations

Research into the correlates of crime-victimization reporting spans academic disciplines, but empirical studies have focused on the role of micro-level factors, especially the characteristics of crime incidents (Xie and Baumer 2019a). Researchers consistently find that decisions to notify law enforcement are influenced by the severity of crimes, or the extent of physical, emotional, and material costs of victimization (Copes et al. 2001; Gottfredson and Hindelang 1979). Micro-level research typically mobilizes rational choice models to argue that post-victimization decisions are a function of cognitive processes that follow a simple "cost-benefit" calculation, whereby victims weigh the potential costs of involving law enforcement against potential benefits (Skogan 1984:120). With minor or less severe incidents, there is little to gain from calling the police. If a victim sustains serious injuries or experiences significant property loss, or if an offender utilizes a weapon (and poses a continued threat to the individual or larger community), there may be greater benefits to involving law enforcement.

Other micro-level approaches emphasize that individuals' attitudes toward the police shape decisions (Xie and Baumer 2019a). Rather than following cost-benefit analyses, people make decisions about whether to engage with police in relation to the levels of legitimacy they attribute to law enforcement and the criminal justice system (Anderson 1999; Sunshine and Tyler 2003; Tankebe 2013). Experiences with procedural

injustice can foster negative dispositions toward police, which reduces people's willingness to seek support from law enforcement (Carr, Napolitano, and Keating 2007). Some studies conclude that attitudes toward law enforcement are significant determinants of decisions to notify police (Sunshine and Tyler 2003; Tankebe 2013), but the evidence remains mixed (Xie and Baumer 2019a).

The logic of micro-level perspectives on post-victimization decisions can be extended to explain why immigrant community members may be less willing to interact with law enforcement. For immigrant community members, reporting crime victimization or cooperating with investigations may introduce a perceived or real risk that irregular immigration statuses will come to light (Menjívar et al. 2018; Theodore and Habans 2016). In so far as deportation (or the risk of deportation) is a salient cost of interacting with law enforcement, members of immigrant communities may be systematically less likely to interact with the police. Fussell (2011:595) finds that a similar process, referred to as the "deportation threat dynamic," plays out in the context of labor rights violations and results in migrant workers being hesitant to contact labor enforcement agencies when confronting exploitative working conditions (see also De Genova's [2002] discussion of deportability). Alternatively, the devolution of immigration enforcement, which has elevated the power and expanded the role of local police in immigration enforcement (Coleman 2007), may challenge the view that local law enforcement are primarily concerned with street-level crimes, thereby undermining law enforcement's institutional legitimacy in immigrant communities.

Macro-Level Explanations: Black's Theory of Law

Black's (1976) theory of law offers greater insights on why and how immigrant community members may or may not mobilize the law by notifying the police about crime victimization experiences, particularly across different immigrant policy contexts. Indeed, Black's (1976) theoretical approach has guided several studies of victims' decisions to call the police (Avakame, Fyfe, and McCoy 1999; Baumer 2002; Copes et al. 2001; Gottfredson and Hindelang 1979; Xie and Lauritsen 2012), but it has not yet been explicitly applied to the case of immigrant communities. According to Black (1976:2), "law," broadly defined as "governmental social control," is a quantifiable characteristic of societies that can be captured by a number of measures, including the rate by which victims notify law enforcement that a crime has occurred. High rates of police notification indicate greater levels of law for a particular social group or in a specific geographic setting.

Black (1976) further argues that social phenomena such as law vary in relation to different aspects of social life. Of the aspects of social life Black (1976) identifies, three are of particular relevance to a theoretical understanding of immigrant community members' mobilization of law: stratification, morphology, and social control. Stratification, or the "vertical aspect of social life" (Black 1976:17), refers to group-based differences in social rank and access to material resources. According to Black (1976), members of structurally advantaged groups have greater access and are more likely to mobilize law (e.g., by calling the police when victimized). Drawing on Black's (1976) theory of law, we would expect that victims in immigrant communities, especially Latinos, are systematically less likely to call the police because of their greater exposure to structural disadvantages (Hoynes, Page, and Stevens 2006; Lynch 2016). The concept of morphology, which refers to the broader "horizontal" relationships among society members, including group-based levels of societal integration,[1] also implies that immigrant community members are less likely than other groups to mobilize the law (Black 1976). As past research reveals, transformations in the U.S. immigration enforcement system, including the devolution of immigration enforcement, the criminalization of immigration, the separation of families, and mass deportation, have disrupted and further marginalized immigrant

communities, which is theorized to undermine their ability or willingness to mobilize law (Kanstroom 2007; Menjívar, Abrego, and Schmalzbauer 2016; Stuart, Armenta, and Osborne 2015; Theodore and Habans 2016).

Finally, Black (1976:107) distinguishes law as governmental social control from other forms of social control such as "etiquette, customs, ethics, bureaucracy, and the treatment of mental illness." Theoretically, formal and informal forms of social control vary inversely such that formal social control (i.e., law) is lower in areas with greater levels of alternatives. Given that immigrant communities possess uniquely high levels of informal social control due to the importance of social capital and the density of social ties (Martinez 2006), we would expect less mobilization of law in immigrant communities.

A Multilevel, Contextualized Help-Seeking Framework

In an effort to integrate insights from the extant micro and macro perspectives, Xie and Baumer (2019a) proposed a multilevel, contextualized theoretical framework to provide a more holistic explanation of help-seeking behaviors,[2] which includes crime victims' decisions to notify law enforcement. The authors concede that help-seeking outcomes are a function of cognitive processes, but by integrating multiple perspectives, Xie and Baumer's (2019a:226) framework emphasizes that "crime incidents, individual, interpersonal, sociostructural, and cultural factors" all affect post-victimization decisions.

According to the multilevel help-seeking model, determinants of decision-making can be organized into three key conceptual groups: victimization and harm, personal/household characteristics, and the external environment (Xie and Baumer 2019a). Measures of victimization and harm, as emphasized in much of the micro-level research, capture the severity of criminal incidents, such as the extent of harm caused to a victim or the value of property lost or destroyed (Copes et al. 2001; Gottfredson and Hindelang 1979; Skogan 1984). Personal/household characteristics refer to people's attitudes toward institutions (Anderson 1999; Carr et al. 2007; Sunshine and Tyler 2003; Tankebe 2013), as well as victim attributes that are demonstrated or theorized to be related to help-seeking behaviors, such as membership within a structurally disadvantaged ethnic/racial group or a differentially integrated social group (Black 1973). Finally, external environments refer to the characteristics of the broader social contexts within which victimization occurs, including victims' social networks, local community/neighborhood of residence, or larger macro-level units such as cities, counties, MSAs, or states. Notwithstanding a few notable studies (Baumer 2002; Desmond et al. 2016; Gutierrez and Kirk 2017; Xie and Baumer 2019b), the external environmental correlates of police notification remain understudied (Xie and Baumer 2019a).

Xie and Baumer's (2019a) multilevel, contextualized help-seeking framework makes a number of theoretical contributions to the literature on victims' decisions to notify law enforcement, but two insights are particularly relevant for understanding immigrants' crime-reporting behavior in sanctuary policy contexts. First, as thoroughly discussed by Xie and Baumer (2019a), the multilevel, contextualized help-seeking framework emphasizes that victims' decisions to call the police can be directly shaped by the characteristics of the external environment within which victimization occurs. As evidence, Gutierrez and Kirk (2017) find that crime reporting is inversely related to the relative size of the immigrant population across MSAs, with the negative effect being stronger for violent crime than for property crime. The authors acknowledge that their study cannot identify the underlying mechanisms suppressing crime reporting, but Gutierrez and Kirk's (2017) findings (see also Xie and Baumer 2019b) are consistent with a number of theoretical expectations derived from micro-level perspectives (Skogan 1984), Black's (1976) theory of law, and the broader multilevel, contextualized help-seeking framework (Xie and Baumer 2019a). An implication of past multilevel research is that social scientists must be attuned to how contextual

factors may be relevant to crime victims' decisions to seek help following victimization. In our study, we are particularly interested in how variation in immigrant sanctuary policy contexts structures crime victimization reporting to law enforcement.

Second, and most important to the present study, the multilevel, contextualized help-seeking framework implies that external environmental conditions can moderate the effects of victimization, harm measures, and victims' personal/household attributes (Xie and Baumer 2019a). That is, the effects of victimization and harm or personal/household characteristics, such as victims' ethno-racial identity, may vary by social contexts, and social contextual effects may be conditional on victim characteristics and the nature of criminal incidents.

A relatively small body of literature has investigated cross-level interactions in the context of help-seeking behavior (Baumer 2002; Goudriaan and Nieuwbeerta 2007; Xie and Baumer 2019b). Baumer (2002), for example, considers a number of theoretically expected cross-level interactions, but only finds that the nonlinear effect of neighborhood disadvantage on the reporting of simple assault is somewhat stronger for black versus non-black victims. Goudriaan and Nieuwbeerta (2007) find that juvenile victims' willingness to seek assistance from school employees varies across combinations of two factors: (1) whether the offender is a schoolmate, and (2) the location of the incident (i.e., on or off school grounds). In contrast to studies documenting cross-level interactions, Xie and Baumer (2019b) find no evidence that the inverse relationship between immigrant neighborhood concentration in new immigrant destinations and reporting behavior varies by victims' race and ethnicity; this suggests members of immigrant communities in new immigrant receiving areas are uniformly affected by a lack of community social integration or the development of legal cynicism.

Based on prevailing theories of crime victimization reporting, we expect members of immigrant communities are less likely to report crime victimization. In light of Xie and Baumer's (2019a) framework, however, we also anticipate that the underlying mechanisms deterring or encouraging victims' help-seeking behavior vary in relation to the characteristics of the external environments within which victimization occurs. As mentioned, we are specifically interested in the role of immigrant sanctuary policies in shaping crime victims' decisions to notify law enforcement. Given the focus of our study, we briefly discuss the literature on immigrant sanctuary policies before situating the mechanism-based policy effects in a multilevel help-seeking framework.

IMMIGRANT SANCTUARY POLICIES

Prior research provides a detailed overview of the history of sanctuary policies in the United States, noting that these policies are often enacted at the local level in response to federal action or inaction in matters related to immigration enforcement (Bau 1994; Kittrie 2006; Martinez et al. 2018; Ridgley 2008). The stated purpose of these policies is often to better integrate members of immigrant communities and to signal that local leaders and institutions are receptive and responsive to the needs of immigrant groups (Kittrie 2006; Lyons et al. 2013; Martinez et al. 2018). Between 1979 and 2008, at least 65 cities or local law enforcement agencies adopted sanctuary policies in some form,[3] and various local governments continue to propose and implement such policies (Collingwood et al. 2019). For instance, in 2014, Immigration and Customs Enforcement (ICE) identified 608 counties, of the almost 2,500 counties reviewed, as sanctuary jurisdictions (Wong 2017).

Contemporary Sanctuary Policies and Their Critics

Contemporary sanctuary policies take a variety of forms. Some policies stipulate that local law enforcement will refuse to comply with ICE requests to detain unauthorized immigrants for extended periods of time so ICE can

assume custody, except in situations where they are holding someone for serious violent crimes or other felonies.[4] Other policies state that city officials or employees will not make inquiries into individuals' immigration status unless otherwise required by law. Given these complexities, Kittrie (2006) developed a parsimonious typology clarifying the various forms and functions of contemporary sanctuary policies, suggesting they generally consist of "don't ask," "don't enforce," or "don't tell" components. "Don't ask" policies limit local officials' role in inquiring about people's immigration status; "don't enforce" policies limit their ability to make arrests or detain people solely for an immigration violation, and "don't tell" policies limit communication between local and federal officials regarding people's immigration status (Kittrie 2006:1455). Immigrant sanctuaries may adopt policies that take on one or a combination of these three forms. Regardless of how they fit within Kittrie's typology, contemporary sanctuary policies are largely consistent with the U.S. Justice Department's (2007:44) definition of "state laws, local ordinances, or departmental policies limiting the role of local law enforcement agencies and officers in the enforcement of immigration laws."

Sanctuary ordinances are not without opposition. Criticism of sanctuary policies is in part predicated on the belief that they have caused "immeasurable harm to the American people and to the very fabric of our Republic" (Executive Order 2017:8799). From this perspective, such policies threaten public safety because they "encourage illegal immigration and undermine federal enforcement efforts" (Garcia 2009:1). Anti-sanctuary rhetoric, as illustrated by President Trump's 2017 Executive Order 13768 titled "Enhancing Public Safety in the Interior of the United States," often links immigrants to criminality at the individual level, and immigration to crime at the macro level,[5] despite substantial evidence to the contrary (Bersani, Loughran, and Piquero 2014; Light and Miller 2018; Martínez-Schuldt and Martinez 2019; Ousey and Kubrin 2018; Powell, Perreira, and Mullan Harris 2010). As an indication of the dangers posed by sanctuary jurisdictions (and immigrants), opponents often draw on anecdotal incidents in which removable noncitizens commit acts of violence (Yee 2017).

Recent research has assessed the claims made by critics of sanctuary policies, at least with regard to their criminogenic effects. As of yet, no empirical evidence supports the claim that sanctuary policies systematically foster crime. Rather, studies consistently find either no evidence of a relationship or a negative association between sanctuary policies and crime (Gonzalez et al. 2017; Kubrin and Bartos 2020; Lyons et al. 2013; Martínez-Schuldt and Martínez 2019; Wong 2017). As an explanation for their findings, some scholars speculate that sanctuary policies may strengthen community social control and reduce crime by enhancing the trust immigrants place in social institutions, especially local law enforcement (Lyons et al. 2013; Martínez-Schuldt and Martínez 2019).

Specifically, sanctuary policies may enhance institutional trust by mitigating victims' fears that they or their friends, family, and surrounding community members will be at risk of deportation if crime victims and witnesses cooperate with local law enforcement. In effect, immigrant sanctuary policy contexts may (1) mitigate the structural conditions that would otherwise deter members of immigrant communities from contacting the police (Black 1976), (2) assuage fears related to real or imagined threats of deportation (Menjívar and Bejarano 2004), or (3) prevent the development of legal cynicism (Lyons et al. 2013; Martínez-Schuldt and Martínez 2019). Thus, sanctuary policy contexts may shift the nature of help-seeking experiences, thereby increasing the likelihood of law mobilization.

RESEARCH HYPOTHESES

We integrate the expectations of the sanctuary–crime literature with a multilevel, contextualized model of help-seeking behavior to develop hypotheses in response to our two research questions: *Are crime victims more*

likely to report victimization in the context of sanctuary policies? Are the macro-level effects of sanctuary policies exclusive to ethno-racial minority victims who are most likely to be socially connected to immigrant communities? As described below, our data source on crime victimization incidents allows us to categorize victims according to their self-identified ethnicity and race. For the purposes of this study, we collapse respondents into four distinct ethno-racial groups: black, Latino, white, and other. The group "other" includes individuals who identify as Asian, American Indian, Indian, Pacific Islander, and bi- or multi-racial—it does not include anyone who identifies as "Latino." We do not expect sanctuary policy contexts to have a uniform direct effect on crime victimization reporting, but we do expect the following:

Hypothesis 1: Latino victims will be more likely to report violent and property crime victimization to law enforcement if they reside in MSAs with sanctuary policies.

Hypothesis 2: "Other" victims (i.e., people who are neither black, Latino, nor white) will be more likely to report violent and property crime victimization to law enforcement if they reside in MSAs with sanctuary policies.

We expect the effects of immigrant sanctuary policy contexts will be specific to Latino and "other" victims for several reasons. First, during our study period, a relatively larger proportion of Latino and the group we refer to as "other" were foreign-born, compared to black and white victims. An analysis of the 5 percent Public Use Microdata Samples from the U.S. Census reveals that 26 to 40 percent of Latinos, and 43 to 49 percent of the "other" group were foreign-born in 1980, 1990, and 2000 (Ruggles et al. 2015). In contrast, only 4 to 5 percent of the white population, and 3 to 6 percent of the black population, were foreign-born. Second, unauthorized migrants disproportionately originated from Latin American countries during our study period, particularly Mexico, and from Asia, although to a much lesser extent. For example, 71 to 79 percent of unauthorized migrants originated from Mexico, Central America, or South America from 1990 through 2000 (Passel and Cohn 2016). Taken together, these demographics suggest Latinos are much more likely to be members of immigrant communities, unauthorized immigrants, or members of mixed-status families[6] compared to white and black individuals. Given these latter two considerations, we expect Latino and "other" victims will be more responsive to changes in sanctuary policy contexts relative to black or white victims.

DATA, MEASUREMENT OF VARIABLES, AND ANALYTIC SAMPLE

To assess if patterns in crime-victimization reporting vary across sanctuary policy contexts, we draw on a variety of data sources. First, our dependent variable and information on victimization incidents come from the National Crime Victimization Survey (NCVS): MSA Data. The NCVS is a nationally representative survey of U.S. households that includes information on the crime victimization experiences of household members who are at least 12 years old (Lauritsen and Schaum 2005). The NCVS: MSA Data, made available in 2007, combines annual information from 1979 through 2004,[7] including subnational geographic identifiers, for the core counties of the 40 largest metropolitan areas in the United States (U.S. Department of Justice 2007). The NCVS: MSA Data has been a major source of data for researchers interested in understanding subnational patterns in criminal victimization (Gutierrez and Kirk 2017; Xie and Lauritsen 2012; Xie et al. 2012).

As part of the NCVS, respondents who have experienced crime victimization indicate whether they reported their victimization to law enforcement. For the purposes of our analyses, we construct a binary variable indicating whether victims reported these incidents to law enforcement. This dichotomous indicator serves as our dependent variable.

Because prior research suggests correlates of reporting behavior vary by the type of victimization experienced (Gutierrez and Kirk 2017), we separately analyze patterns in the reporting of violent (e.g., completed or attempted rape, robbery, or assault) and property (e.g., completed or attempted theft, motor vehicle theft, or burglary) crime victimization. As noted in Table 1, 47 percent of violent crime victimization incidents and 35 percent of property crime incidents were reported to local law enforcement during our study period.

In addition to our dependent variable, the NCVS: MSA Data provides other relevant information on the nature of victimization and harm in a given incident as well as personal/household characteristics of victims. We leverage these data to control for victims' sex (female or male), age (continuously measured[8]), educational attainment (no high school, high school, or college graduate), marital status (married or not married), and homeownership (yes or no). We also include measures unique to violent and property victimization that may affect crime victims' willingness to seek intervention by law enforcement (Felson et al. 2002; Skogan 1984). Our analyses of violent crime include binary measures for whether a gun or other weapon was present and if the victim was injured. The analyses of property crime include a measure of the value of property lost, damaged, or stolen. We account for the relational distance between victims and offenders (see Black's [1976] discussion of morphology) by including a categorical variable that indicates if victims knew the offender (*offender: known*), if the offender was a stranger (*offender: stranger*), or if victims were unsure whether they knew the offender (*offender: don't know*). Including the victim-offender relationship measure would reduce the analytic sample of the property-victimization models by about 94 percent due to substantial missing data. We thus limit our inclusion of the victim-offender relationship measures to our analyses of violent crime incidents.

Given our second research question, we are interested in examining how, if at all, ethno-racial minority victims differentially respond to changes in immigrant sanctuary policy contexts. NCVS respondents report their racial identity and if they are of Hispanic origin (we refer to this group as Latinos). We draw on these measures to construct a variable that groups victims into four categories: black, Latino, white, and other (e.g., Asian, American Indian). The NCVS allows us to further disaggregate the "other" group by racial identification, but we preserve this broad category because it only comprises about 3 percent of the violent and property crime samples. In contrast, white, black, and Latino individuals make up 68, 17, and 12 percent, respectively, of the violent crime sample, as well as 71, 15, and 11 percent of the property crime sample. Table 1 provides descriptive statistics for the incident-level variables used in our analyses across the samples of violent and property crime victimization incidents.

Following prior research, we capture our focal independent variable, whether a sanctuary policy was adopted within an MSA between 1980 and 2004, from a list of sanctuary jurisdictions identified by the National Immigration Law Center (NILC)[9] in 2008 (Gonzalez et al. 2017; Lyons et al. 2013; Martínez-Schuldt and Martínez 2019). NILC organizes its list of local sanctuary jurisdictions by U.S. states, briefly describes each policy, and provides links to municipal documents for verification. The policies listed by NILC vary in form but conform in procedural function with Kittrie's (2006) typology and the U.S. Department of Justice (2007) definition of sanctuary jurisdictions. Following prior research (Martínez-Schuldt and Martinez 2019), we consulted additional sources[10] to update NILC's list. Ultimately, we only found one MSA (Cleveland-Lorain-Elyria in the NCVS: MSA Data) within which a sanctuary policy was adopted during our study period but that was not included on NILC's list.[11]

Our measure of sanctuary policy adoption is a dichotomous indicator of the presence (1) or absence (0) of one or more sanctuary policies, regardless of type, within an MSA

Table 1. Descriptive Statistics of Incident-Level Variables Used in the Analysis

	Violent Crime Victims				Property Crime Victims			
	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
Dependent variable								
Reported to law enforcement	.47	.50	.00	1.00	.35	.48	.00	1.00
Victimization and harm								
Injury sustained: yes	.46	.50	.00	1.00				
Weapon present: gun	.12	.32	.00	1.00				
Weapon present: none	.69	.46	.00	1.00				
Weapon present: other	.19	.39	.00	1.00				
Value of property lost[a]					4.33	2.07	.00	11.65
Personal characteristics								
Age	29.21	12.98	14.50	60.00	35.11	13.93	14.50	60.00
Edu. attainment: college grad.	.18	.38	.00	1.00	.26	.44	.00	1.00
Edu. attainment: high school	.49	.50	.00	1.00	.53	.50	.00	1.00
Edu. attainment: no high school	.33	.47	.00	1.00	.21	.41	.00	1.00
Ethnicity/race: Latino	.12	.33	.00	1.00	.11	.32	.00	1.00
Ethnicity/race: black	.17	.37	.00	1.00	.15	.35	.00	1.00
Ethnicity/race: white	.68	.47	.00	1.00	.71	.46	.00	1.00
Ethnicity/race: other	.03	.18	.00	1.00	.03	.18	.00	1.00
Home ownership: yes	.48	.50	.00	1.00	.55	.50	.00	1.00
Married: yes	.27	.44	.00	1.00	.44	.50	.00	1.00
Sex: female	.42	.49	.00	1.00	.54	.50	.00	1.00
Offender: don't know	.01	.12	.00	1.00				
Offender: known to victim	.50	.50	.00	1.00				
Offender: stranger	.49	.50	.00	1.00				
Observations								
MSAs	40				40			
MSA-years	996				996			
Incidents	35,329				136,053			

[a]Log value.
Note: Values displayed prior to mean centering.

(j) during a specific year (t). That is, we do not distinguish between the various types of policies identified by Kittrie (2006). We lag the passage of sanctuary policies by one year to account for a potential delay in the diffusion of victims' awareness of the policies (see also Martínez-Schuldt and Martínez 2019). If a policy is adopted in MSA (j) in 1999, for example, then MSA (j) is recorded as having a sanctuary policy in 2000, 2001, 2002, 2003, and 2004. After we lagged the policy measure, 140 of the MSA-years (14 percent) had at least one sanctuary policy enacted at some point during the study period (see Table 2). Although not specified in Tables 1 and 2, about 20 percent of both the violent and property crime samples (7,239 violent crime incidents and 26,549 property crime incidents) occurred within an immigrant sanctuary policy context.

The remaining macro-level variables in our analyses come from the U.S. Decennial Census (1980, 1990, and 2000) and pooled data from the American Community Survey (ACS, 2005 to 2009). We obtained annual tabular data for the consistent core counties in each MSA within the NCVS: MSA Data for each year available and aggregated county-level sociodemographics to the MSA level. Our models include MSA-level measures identified as being related to crime-reporting behavior (Baumer 2002; Felson et al. 2002; Gutierrez and Kirk 2017; Xie and Baumer 2019b) and to the adoption of immigrant sanctuary policies—including educational attainment of the population (*percent college graduates*) and the percent of the voting population who voted for the Democratic candidate (*percent Democratic voters*[12]) in presidential elections (Collingwood and Gonzalez O'Brien 2019; Gonzalez et al. 2017; Lyons et al. 2013). The rest of the macro-level measures include *percent black*, a measure capturing the relative size of the male population between ages 15 and 34 (*percent crime-prone population*), *percent immigrant, percent Latino, percent owner-occupied homes*, and a structural disadvantage index (*disadvantage index*). Similar to prior research, we generate the *disadvantage index* by combining several measures of structural disadvantage (Light and Miller 2018; Ousey and Kubrin 2014). We calculate the average for four standardized measures: the percent of children living in single-parent homes, percent of adults with no high school degree, percent of unemployed adults, and percent of the population living in poverty (see also Martínez-Schuldt and Martínez 2019). The coefficient of reliability, or Cronbach's alpha, for the measures that constitute our index of structural disadvantage is .77, which suggests the instrument is reliable. Following prior research interested in macro-level correlates of crime and crime reporting, we use linear interpolation to account for inter-census years (Sharkey et al. 2017; Xie et al. 2012).

After combining data from the five sources, we have complete information on 35,329 incidents of violent crime victimization and 136,053 incidents of property crime victimization. These incidents occurred in 40 different MSAs across 25 years (1980 to 2004). Because of missing annual data for some MSAs, we yield 996 MSA-year observations (out of a potential 1,000). Table 2 presents the descriptive statistics for all MSA-level variables, including the measures used to construct the *disadvantage index*.

ANALYTIC APPROACH

We use multilevel logistic regression to assess patterns in victims' crime-reporting behavior over time and across MSAs. We estimate and present results from multilevel models in place of standard logistic regression models for three important reasons. First, our theoretical framing, which bridges the immigrant sanctuaries literature with recent developments in the scholarship on post-victimization decisions, is inherently concerned with the extent to which crime-reporting behavior is a function of multilevel processes (Xie and Baumer 2019a). A multilevel modeling approach allows us to directly investigate how much of the variation in crime reporting, at the micro level, is a function of differences

Table 2. Descriptive Statistics of MSA-Year Characteristics (N = 996), 1980 to 2004

	Mean	Std. Dev.	Minimum	Maximum
External environmental characteristics				
Sanctuary policy[a]	.14	.35	.00	1.00
% Black	13.69	8.19	1.26	36.43
% College graduates	29.57	6.61	14.75	51.77
% Crime-prone population	7.57	1.19	5.17	13.61
% Democratic voters	47.55	9.43	22.65	76.35
% Immigrant	12.71	9.70	1.89	50.94
% Latino	12.95	12.74	.52	59.72
% Owner-occ. homes	61.82	7.83	27.31	81.71
Disadvantage index	.00	.86	−1.75	2.34
% Children in single-parent homes	9.16	1.57	5.24	13.09
% No high school degree	18.06	4.86	7.87	34.99
% Unemployed	5.75	1.27	2.87	11.66
% Poverty	10.97	2.78	4.24	19.76
MSA population[b]	14.57	.59	13.07	16.08

[a]Policy adoption lagged one year.
[b]Log value.
Note: Values are prior to group-mean centering.

across higher-order units (MSAs and MSA-year). Furthermore, we are interested in how effects of micro-level factors, specifically victims' ethno-racial identification, vary across macro-units (i.e., random coefficients) and whether the inclusion of cross-level interactions can explain variance in micro-level effects across MSAs. We expect the reporting behavior of Latinos and "other" victims will vary across MSA-years and will be a function of sanctuary policy adoption. Again, a multilevel modeling approach allows us to more directly investigate our research questions and expectations. Second, the NCVS: MSA Data contains samples of crime incidents repeatedly nested within MSAs over the study period (1980 to 2004). As such, the NCVS: MSA Data comprises crime victimization incidents (level-1) nested in MSA-years (level-2) nested in MSAs (level-3), or an inherent multilevel structure (see also Xie et al. 2012). Finally, as described below, results of maximum likelihood tests suggest multilevel models with random intercepts at the MSA and MSA-year levels and random slopes for *ethnicity/race: Latino* and *ethnicity/race: other* are preferable to other models, including standard logistic regression models.[13]

We leverage the longitudinal nature of our data to estimate within- and between-MSA effects. Within-effects, which account for time-invariant unobserved factors by only considering within-unit variation across time (Allison 2009), are considered more suitable for causal inference (Giesselmann and Schmidt-Catran 2019). To disentangle within-effects from between-effects, we use a hybrid modeling approach wherein we include observed measures of the MSA-year characteristics along with additional measures of the MSA sample means in our analytic models (Allison 2009; Fairbrother 2014; Giesselmann and Schmidt-Catran 2019). Equation 1 reflects our models of the log odds of whether a property or violent victimization experience was reported to law enforcement for a given incident (y_{itj}), where i denotes an incident, t denotes a time point, and j denotes a specific MSA:

$$y_{itj} = \beta_0 + \beta_1 x_{itj} + \beta_2 x_{tj} + \beta_3 \bar{x}_j + \beta_4 time_{tj} + v_j + u_{tj} + e_{itj} \quad (1)$$

In addition to incident-level predictors (e.g., victim ethnicity/race, age, sex, injuries sustained) depicted by x_{itj}, Equation 1 illustrates that we decompose all MSA

characteristics, x_{ij}, into (1) x_{ij}, or the observed MSA-year measure, and (2) $\bar{x}_j$, or an average MSA measure for the study period. As a substantive illustration, the macro-level variable *sanctuary policy* is decomposed into a longitudinal component (*sanctuary policy* under the "MSA characteristics: within-effects" heading in Tables 3 and 8), which represents the timing of policy adoption between 1980 and 2004 within each MSA, and a cross-sectional component (*sanctuary policy* under the "MSA characteristics: between-effects" heading in Tables 3 and 8), which reflects the proportion of years for which a policy exists within each MSA over the study period. In practical terms, the within-effects can be interpreted as how changes in MSA characteristics over time correlate with shifts in reporting behavior over time within each MSA (e.g., how reporting behavior compares before and after the adoption of a sanctuary policy). In contrast, the between-effects can be interpreted as how crime victimization reporting behaviors differ across MSAs depending on varying average levels of MSA characteristics (e.g., how early policy adopters compare to later adopters and non-adopters). The random intercepts for *MSA, MSA-year*, and *incidents* are reflected by v_j, u_{ij}, and e_{itj}, respectively.

The within-effect components illustrated in Equation 1 are analogous to the within-effect or fixed-effect estimates derived from models that include MSA dummies[14] (Allison 2009; Fairbrother 2014; Giesselmann and Schmidt-Catran 2019). Importantly, recent research demonstrates that standard approaches to estimating within-unit effects do "not yield a genuine within estimator of cross-level interaction effects" and are "likely to yield biased estimates of cross-level interactions" when working with repeat cross-sectional data structures (Giesselmann and Schmidt-Catran 2019:194–5). Therefore, we follow the methodological approach outlined by Giesselmann and Schmidt-Catran (2019) to manually control for effect heterogeneity in our cross-level interactions that would otherwise be ignored by traditional approaches to within-unit estimation. In doing so, we produce and report the genuine within-effects of our cross-level interactions of interest. Given our focus on cross-level interactions, we also mean center *incident-level* factors and specify random coefficients for the level-1 variables interacted with level-2 conditions (*ethnicity/race: Latino* and *ethnicity/race: other*) across the models (Bauer and Curran 2005; LaHuis and Ferguson 2009). We estimate separate models for property and violent crime victimization using maximum likelihood estimation.[15]

Finally, a number of scholars have identified issues that arise in the interpretation of logistic regression coefficients or odds ratios as well as with comparisons of effect sizes between groups (i.e., interactions) and across models (Long and Mustillo 2018; Mood 2010; Williams 2009). In particular, unobserved heterogeneity affects coefficient estimates, even when unobserved variables are uncorrelated with observed independent variables (Long and Mustillo 2018; Mood 2010). Given this concern, we provide a supplementary investigation into any statistically significant cross-level interactions. Specifically, we use results from our regression models to estimate average discrete changes in predicted probabilities and provide tests of statistical significance (Long and Mustillo 2018).

RESULTS

Reports of Violent Crime Victimization

Table 3 presents results from three multilevel logistic regression models of the log odds that incidents of violent victimization were reported to law enforcement. The bottom section of Table 3 includes the variance components of the random intercepts for *MSA* and *MSA-year* as well as for the random slopes of *ethnicity/race: Latino* and *ethnicity/race: other* (see Model 1). Similar to prior research on outcomes related to crime victimization (Xie et al. 2012), the random components of the level-2 and level-3 intercepts in a multilevel model with

no covariates (available upon request) reveal that the majority (about 98 percent) of the variation in the dependent variable is attributable to incident-level or level-1 variation (i.e., $(\frac{\pi^2}{3}) / ([\frac{\pi^2}{3}] + [Var(MSA)] + [Var(MSA\text{-}year)]) = .98$). Despite *MSAs* and *MSA-years* accounting for only a limited proportion of the variance, results of likelihood ratio tests (available upon request) indicate that a three-level model allowing for random variation at each level and with random slopes for *ethnicity/race: Latino* and *ethnicity/race: other* is a significant improvement over a model with no random slopes, which, in turn, is preferable to both a two-level model (incidents nested in MSA-years) and a standard logistic regression model (LaHuis and Ferguson 2009; Schmidt-Catran and Fairbrother 2016). The likelihood ratio tests and Wald tests indicate that the slope variance of *ethnicity/race: Latino* and *ethnicity/race: other* is significant (Hox 2010). Based on the variance components presented at the bottom of Table 3, the variables included in Model 1 account for about 93 percent of level-3 variance and 59 percent of the level-2 variance, as the variance of the random intercepts are reduced from .014 to .001 (*MSAs*) and from .046 to .019 (*MSA-years*).

Model 1 also displays the fixed effects (i.e., the non-random components) of the measures of victimization and harm, victims' personal characteristics, and external environmental characteristics—although it does not include cross-level interactions. For the sake of brevity, we focus on the interpretation of effects relevant to our specific research questions and corresponding hypotheses. Additionally, we concentrate on the within-effects (see the coefficients listed under the "MSA characteristics: within-effects" headings) given their suitability for causal inference (Giesselmann and Schmidt-Catran 2019). First, we find no evidence that sanctuary policy adoption, in general, is associated with crime-reporting behavior. In Model 1, the longitudinal component of *sanctuary policy* (.033) is not statistically significant. As previously discussed, though, we expected the effect of sanctuary policy context to be conditional on victims' ethno-racial minority status (*ethnicity/race: Latino, ethnicity/race: other*).

Second, black victims have greater odds of reporting violent victimization to law enforcement, by about 18 percent ($e^{.165} = 1.18$; $p < .001$), although neither Latino nor "other" victims are statistically different from white victims in their crime-reporting behavior. Recall that we allow the coefficients of *ethnicity/race: Latino* and *ethnicity/race: other* to vary across MSA-years (i.e., random slopes). In both instances, Wald and likelihood ratio tests indicate that the slope variance components are statistically significant (Hox 2010; LaHuis and Ferguson 2009). According to the variance estimates presented in Model 1, 95 percent of the random *ethnicity/race: Latino* effect estimates, taking into account slope variation, fall between –.674 and .830 across MSA-years. The interval for the *ethnicity/race: other* estimate is larger, ranging from –1.498 to 1.344 across MSA-years. These estimates suggest there is no uniformly negative or positive effect on the likelihood that Latino and "other" victims report their violent victimization to law enforcement. Moreover, this finding clearly demonstrates that contextual differences are at the core of differences in reporting behavior for a subset of our sample. In some MSAs, Latino and "other" victims are more likely to report victimization; the opposite is true in other MSAs.

Models 2 and 3 investigate whether variation in the effects of *ethnicity/race: Latino* and *ethnicity/race: other* across MSA-years is related to changes in immigrant sanctuary policy context within MSAs over time. Consistent with our expectations, we find that Latino victims have greater odds of reporting violent crime victimization after passage of a sanctuary policy within victims' MSA of residence. That is, the within-effect component of the interaction term *Latino* × *sanctuary policy* is positive and statistically significant ($p < .01$). This suggests the odds of Latinos reporting crime victimization to law enforcement is greater in the years following adoption of sanctuary policies within their MSAs of residence.

Table 3. Multilevel Logistic Regression Models of Victims' Log Odds of Reporting Violent Crime Victimization to Law Enforcement, 1980 to 2004

	Model 1		Model 2		Model 3	
	Coef.	SE	Coef.	SE	Coef.	SE
Victimization and harm						
Injury sustained: yes	.614***	.024	.612***	.024	.611***	.024
Weapon present: gun	1.286***	.039	1.284***	.039	1.281***	.039
Weapon present: other	.543***	.030	.539***	.030	.540***	.030
Personal characteristics						
Age	.017***	.001	.017***	.001	.017***	.001
Edu. attainment: college grad.	.185***	.038	.182***	.038	.185***	.038
Edu. attainment: high school	.310***	.028	.307***	.028	.310***	.028
Ethnicity/race: Latino	.078	.044	.146**	.049	.049	.039
Ethnicity/race: black	.165***	.033	.164***	.033	.165***	.033
Ethnicity/race: other	−.077	.081	−.091	.066	−.064	.095
Home ownership: yes	−.033	.024	−.034	.024	−.034	.024
Married: yes	.337***	.028	.338***	.028	.334***	.028
Sex: female	.393***	.024	.392***	.024	.391***	.024
Offender: don't know	.220*	.095	.219*	.095	.222*	.095
Offender: stranger	.118***	.024	.120***	.024	.115***	.024
MSA characteristics: within-effects						
Sanctuary policy[a]	.033	.067	.026	.067	.033	.067
% Black	.006	.020	.006	.020	.007	.020
% College graduates	−.017	.013	−.016	.013	−.016	.013
% Crime-prone population	.055	.056	.050	.055	.060	.056
% Democratic voters	−.001	.005	−.001	.005	−.002	.005
% Immigrant	−.012	.013	−.010	.013	−.012	.013
% Latino	.017	.016	.014	.016	.016	.016
% Owner-occ. homes	−.006	.018	−.005	.018	−.009	.018
Disadvantage index	−.038	.099	−.012	.099	−.031	.099
MSA population[b]	−.776**	.300	−.806**	.299	−.776**	.298
Latino × sanctuary policy			.502**	.170		
Other × sanctuary policy					−.060	.335
MSA characteristics: between-effects						
Sanctuary policy[a]	−.144	.093	−.114	.092	−.158	.093
% Black	−.001	.020	−.001	.020	−.003	.020
% College graduates	−.011	.014	−.014	.014	−.009	.015
% Crime-prone population	−.017	.057	−.008	.056	−.015	.057
% Democratic voters	.008	.006	.008	.006	.008	.006
% Immigrant	.009	.014	.007	.014	.011	.014
% Latino	−.010	.017	−.007	.017	−.010	.016
% Owner-occ. homes	.006	.018	.005	.018	.008	.018
Disadvantage index	−.165	.120	−.193	.119	−.167	.120
MSA population[b]	.736*	.302	.768*	.301	.740*	.300
Latino × sanctuary policy			−.838***	.200		
Other × sanctuary policy					−.040	.412
Year fixed effects (1980 to 2004)	Yes		Yes			
Constant	.781	.620	.706	.612	.575	.645
Var(MSA)	.001	.001	.001	.001	.001	.001
Var(MSA-year)	.019	.007	.018	.007	.020	.007
Var(ethnicity/race: Latino)	.147	.051	.084	.044		

(continued)

Table 3. *(continued)*

	Model 1		Model 2		Model 3	
	Coef.	SE	Coef.	SE	Coef.	SE
Var(ethnicity/race: other)	.526	.216			.483	.209
Observations						
Level-3, MSAs	40		40		40	
Level-2, MSA-years	996		996		996	
Level-1, Incidents	35,329		35,329		35,329	

[a]Policy adoption lagged one year.
[b]Log-value.
Note: Victimization and harm and personal characteristics are mean centered. *Weapon present: none, edu: less than high school, ethnicity/race: white,* and *offender: known* are reference categories. Models include control for NCVS survey redesign, and interactions account for effect heterogeneity in *ethnicity/race: Latino* and *ethnicity/race: other.*
$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$ (two-tailed tests).

Wald and likelihood ratio tests indicate that the random component of *ethnicity/race: Latino* remains significant after accounting for the cross-level interaction between *ethnicity/race: Latino* and *sanctuary policy*. However, the cross-level interaction between *sanctuary policy* and *ethnicity/race: Latino* accounts for a relatively large share of the variance in *ethnicity/race: Latino* effect across MSA-years. Inclusion of the cross-level interaction term reduces the variance component of *ethnicity/race: Latino* (in a model omitting a random component for *ethnicity/race: other*) from .133 to .084, or a reduction of about 37 percent.

Finally, we find no evidence of a significant cross-level interaction between the sanctuary policy measure and *ethnicity/race: other.* Inclusion of the cross-level interactions accounts for little of the variance in *ethnicity/race: other* effect (see Model 3). Therefore, we fail to find evidence in support of our second hypothesis, at least in relation to "other" victims of violent crime.

Predicted Probabilities of Reporting Violent Crime Victimization

To better illustrate the relative magnitudes of our effect estimates, Table 4 provides probabilities and group differences in probabilities (average discrete changes in probabilities) for combinations of victims' ethno-racial identities and the presence or absence of a sanctuary policy (Long and Mustillo 2018). The predicted probabilities are derived from Model 2 in Table 3.

The first column in Table 4 displays the predicted probabilities that Latino, black, white, and other victims report violent crime victimization to law enforcement in an area without a sanctuary policy. Consistent with the substantive results presented in Model 2 in Table 3, Latino and black victims have slightly higher probabilities of reporting violent crime victimization to police (both around 50 percent) compared to white victims (about 47 percent). Although not displayed in Table 4, these differences in probabilities for Latino and black victims, when compared to white victims, are statistically significant ($p < .01$ and $p < .001$, respectively). The predicted probability of reporting victimization for "other" victims is not significantly different from white victims, but it is significantly lower than the probability for Latino victims ($p < .01$) and black victims ($p < .001$).

The second column in Table 4 displays the predicted probabilities for a hypothetical situation in which sanctuary policies are adopted within victims' MSAs of residence. The probabilities of reporting crime victimization for black, white, and "other" victims remain

Table 4. Average Discrete Change in Predicted Probabilities of Reporting Violent Crime Victimization to Law Enforcement (Other Variables Set at Observed Values)

	Baseline	Following Policy Adoption	Difference
Ethnicity/race: Latino	.498	.614	.116**
Ethnicity/race: black	.502	.508	.006
Ethnicity/race: white	.465	.471	.006
Ethnicity/race: other	.445	.451	.006
Difference in effect of sanctuary policy adoption			.110**

Note: Estimates derived from Table 3, Model 2.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

relatively stable in the context of sanctuary policies. Although there is an increase in the probabilities by less than one percentage point (.006), the differences between the baseline and post-policy adoption estimates for black, white, and "other" victims are not statistically significantly different from zero. In contrast, the predicted probability that Latino victims report violent crime victimization increases from 49.8 to 61.4 percent. The estimated difference for Latinos of about 12 percentage points is statistically significant ($p < .01$). The difference in the *effect* of sanctuary policy contexts for Latinos versus the other three ethno-racial groups is presented in the bottom-right corner of Table 4. According to our estimates, the difference in the policy-context effect (.110) is also statistically significant ($p < .01$). Taken together, results in Tables 3 and 4 reveal that the presence of sanctuary policies moderates the effect of ethno-racial identification on the likelihood that victims report crime victimization to law enforcement. The results can also be interpreted as implying that the effect of sanctuary policy contexts is specific to victims who identify as Latino. Ultimately, Latino victims have higher probabilities of reporting violent crime victimization to law enforcement *after* a sanctuary policy has been adopted within their MSA of residence.

Recent information from the 2018 NCVS helps illustrate and contextualize the 12 percentage-point difference between Latinos before and after policy adoption as reported in Table 4. According to the NCVS, around 14 percent, or over 800,000, of the around 6 million violent crime incidents in 2018 involved Latino victims (U.S. Department of Justice 2019). If 62 percent of violent crime incidents involving Latino victims were reported to law enforcement, rather than the 50 percent reported in Table 4, more than 90,000 additional incidents of violent crime would have been reported to police nationwide. As previously mentioned, the efficacy of the U.S. criminal justice system in the social control of crime predominately relies on law enforcement officials reacting to denizens' reports of crime (Reiss 1992), which makes it crucial that residents mobilize the law when victimized (Black 1973).

Parallel Trends and Alternative Timings of Sanctuary Policy Adoption

Estimates derived from hybrid modeling strategies, similar to other procedures that focus on within-unit effects, assume that outcomes across comparison groups have common or parallel time trends (Auspurg, Brüderl, and Wöhler 2019; Schmidt-Catran and Spies 2019). For the present study, the assumption is that patterns in the crime-reporting behavior of Latinos residing in MSAs that adopted sanctuary policies during the study period are equivalent to the patterns of other groups *prior to any changes in policy* (Kahn-Lang and Lang 2020). Furthermore, it is assumed that the reporting behavior of Latinos in MSAs with sanctuary policies

Table 5. Ordinary Least Squares Regression Models of Adjusted Reporting Trends

	Latinos vs. Other Groups (combined)		Latinos[a] vs. Other Groups (separated)	
	Coeff.	SE	Coeff.	SE
Time trend				
Year (1980 to 2003)	−.002***	.001	−.003*	.001
Group differences				
Latinos, eventual sanctuaries	−.029	.023	Ref.	
Black, eventual sanctuaries			.069**	.024
White, eventual sanctuaries			−.005	.024
Other, eventual sanctuaries			−.016	.024
Latinos, non-sanctuaries			.059*	.023
Black, non-sanctuaries			.094***	.023
White, non-sanctuaries			.025	.023
Other, non-sanctuaries			−.021	.023
Interaction (Group × Year)				
Latinos, eventual sanctuaries	−.001	.002	Ref.	
Black, eventual sanctuaries			−.001	.002
White, eventual sanctuaries			.001	.002
Other, eventual sanctuaries			.000	.002
Latinos, non-sanctuaries			.000	.002
Black, non-sanctuaries			.000	.002
White, non-sanctuaries			.001	.002
Other, non-sanctuaries			.001	.002
Constant	.503***	.008	.474***	.017
Model R^2	.117		.485	
Observations	196		196	

[a]Latinos, eventual sanctuaries is the set reference group.
Note: Year measured as 1980 = 0, 1981 = 1, . . . , 2003 = 23.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

would have followed the common trend of other groups had the policies never been adopted. Although it is possible to test for differences in preexisting trends, it is important to note that failing to find evidence of diverging trends is not, itself, evidence of *no difference* in time trends, nor is it confirmation that the counterfactual assumption is not violated (Kahn-Lang and Lang 2020). Still, we investigate whether there is evidence of diverging preexisting trends across groups in Table 5.

To test for differences in preexisting trends, we use the results presented in Model 2 in Table 3 to calculate adjusted probabilities of reporting violent crime victimization to law enforcement across MSAs where sanctuary policies were not adopted from 1980 through 2004 and MSAs within which sanctuary policies were eventually adopted during our study period. Given our main findings (Model 2, Table 3), we further disaggregate the reporting trends of each of the four ethno-racial groups identified in our study across the two types of MSAs. Importantly, we exclude all cases from MSA-years with existing sanctuary policies so that the *eventual sanctuaries* groups (see Table 5) only consist of Latino victims in MSAs prior to the actual adoption of sanctuary policies. The exclusion of Latino victims in MSAs *after* policy adoption facilitates a comparison of preexisting trends in reporting patterns between Latinos in *eventual sanctuaries* and the other groups. If the assumption of preexisting parallel trends is violated, we expect to find evidence of different time trends in groups' rates of reporting crime victimization prior to the adoption of sanctuary policies across MSAs. We use

Table 6. Multilevel Logistic Regression Models of Victims' Log Odds of Reporting Violent Crime Victimization to Law Enforcement with Sanctuary Policy Adoption Led by 1 and 3 Years, 1980 to 2004

	Sanctuary Adoption Led 1 Year		Sanctuary Adoption Led 3 Years	
	Coef.	SE	Coef.	SE
Victim characteristics				
Ethnicity/race: Latino	.083	.050	.083	.051
Ethnicity/race: black	.165***	.033	.165***	.033
Ethnicity/race: other	−.089	.066	−.088	.066
MSA characteristics: within-effects				
Sanctuary policy	−.089	.066	.073	.058
Latino × sanctuary policy	−.035	.094	−.029	.090
Observations				
MSA	40		40	
MSA-year	996		996	
Incidents	35,329		35,329	

Note: Model contains all other measures and specifications noted in Table 3, Model 2. Full results are available upon request.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

ordinary least squares to calculate the parameter estimates displayed in Table 5.

Table 5 reports results from two separate models. In the first model, presented in the left column, we compare Latinos in eventual sanctuaries to all other groups combined into one reference category (e.g., white individuals in eventual sanctuaries and Latinos in nonsanctuaries). Although we find evidence of a general decline in rates of reporting violent crime victimization over the study period, we fail to find evidence that the time trend for Latinos in eventual sanctuaries is significantly different from the reference group's time trend. The second model in Table 5 compares Latinos in eventual sanctuaries to each of the seven other groups individually. Again, the second model reports a general decline in rates of reporting across time, but we fail to find evidence that the time trend of Latinos in eventual sanctuaries is significantly different from the time trends of the other groups.

Table 6 further investigates the sensitivity of the results presented in Model 2 of Table 3 to alternative timings of sanctuary policy adoption. Rather than *lag* policy adoption by one year to allow for policy diffusion, as we do in our main analyses, the models presented in Table 6 *lead* policy adoption by one and three years. So, if a sanctuary policy was adopted within an MSA in 2000, we recode the policy as being adopted in 1999 (left column of Table 6) or 1997 (right column of Table 6). Thus, policies are coded as occurring one or three years earlier than what transpired in reality. We expect leading policy adoption will *weaken* the estimated effects of our cross-level interactions in Table 3 because we are pooling MSA-years that should, theoretically, have no policy effects with MSA-years that should have policy effects. Alternatively, it is possible that some other unobserved antecedent macro-level condition (e.g., pro-immigrant local cultures) affects both the probability that sanctuary policies are adopted and the likelihood that Latino victims report crime victimization. If this assumption is correct, the variables that lead policy adoption should maintain their significance because they capture the presence of the unobserved macro-level characteristics that result in passage of the policies in the first place.

Table 7. Multilevel Logistic Regression Model of Victims' Log Odds of Reporting Violent Crime Victimization to Law Enforcement with Sanctuary Policy Adoption Weighted by Latino Population Coverage, 1980 to 2004

	Sanctuary Policy Weighted by Coverage of Latino Population	
	Coef.	SE
Victim characteristics		
Ethnicity/race: Latino	.144**	.048
Ethnicity/race: black	.164***	.033
Ethnicity/race: other	−.091	.066
MSA characteristics: within-effects		
Sanctuary policy	.129	.095
Latino × sanctuary policy	.633**	.231
Observations		
MSA	40	
MSA-year	996	
Incidents	35,329	

Note: Model contains all other measures and specifications noted in Table 3, Model 2. Full results are available upon request.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

In both models presented in Table 6, we fail to find any evidence of a statistically significant cross-level interaction between Latino self-identification and one- and three-year leads of sanctuary policy adoption. Although not displayed in Table 6, subsequent analyses (available upon request) fail to find evidence of significant conditional effects in terms of predicted probabilities (Long and Mustillo 2018). We interpret these results as additional evidence that the adoption of sanctuary policies during our study period affected Latinos' post-victimization decisions. There is something unique about the help-seeking behavior of Latino violent-crime victims within MSAs *after* sanctuary policies are adopted.

Alternative Measures of Sanctuary Policy Adoption

Table 7 investigates the sensitivity of the results presented in Model 2 of Table 3 to an alternative measurement of sanctuary policy adoption. The models in Table 3 treat MSA-years as having immigrant sanctuary policies if a policy is passed within any of the MSA's core counties. We estimate additional models wherein we weight exposure by the proportion of an MSA's total Latino population that is covered by an immigrant sanctuary policy (for a similar approach, see Xie et al. 2012). If, for example, no Latinos lived in a core county that adopted a sanctuary policy, we would not treat the MSA as possessing a sanctuary policy (i.e., *sanctuary policy* = 0). If 50 percent of an MSA's Latino population lived in a core county that adopted a sanctuary policy, we would weight the policy by half (i.e., *sanctuary policy* = .5).

The results in Table 7 reveal that our weighted measure of sanctuary policy exposure is positively (.633) associated with the log odds that Latinos report crime victimization. The cross-level interaction is also statistically significant ($p < .01$). The estimate suggests Latinos' odds of reporting crime victimization increase as the relative size of the Latino population exposed to a sanctuary policy within the victim's MSA of residence increases. Estimates of the change in predicted probabilities based on the model presented in Table 7 are similar to those presented in Table 4.

Reports of Property Crime Victimization

Table 8 reports results of the multilevel logistic regression models of victims' log odds of reporting property crimes to law enforcement. Prior research suggests the ecological correlates of reporting property crime victimization may differ from those of violent victimization (Gutierrez and Kirk 2017). The models in Table 8 provide additional tests of our hypotheses in the context of property crime victimization. We were unable to achieve convergence of maximum likelihood estimation with models that include all covariates, random intercepts, and random components for both *ethnicity/race: Latino* and *ethnicity/race: other*. Following the analytic strategy described by Barr and colleagues (2013), we examined the results of the non-converged models and found that the slope variance of *ethnicity/race: other* was near zero, suggesting there is little variation in the effect of *ethnicity/race: other* across MSA-years. We then omitted the random component of *ethnicity/race: other*, re-estimated the model, and achieved convergence when specifying random intercepts and a random slope for *ethnicity/race: Latino*. As such, we exclude analyses of interactions between sanctuary policy context and *ethnicity/race: other* for the remainder of the article and, instead, focus on potential interactions involving *ethnicity/race: Latino*.

As with Table 3, the bottom section of Table 8 includes the variance components of the random intercepts at the *MSA* and *MSA-year* levels. Additionally, Model 4 reports the variance component for the random slope of *ethnicity/race: Latino*. Similar to the violent crime model, the variance components of the level-2 and level-3 intercepts in the multilevel model with no covariates (available upon request) indicate that almost all (about 99 percent) of the variation in the dependent variable is attributable to incident-level or level-1 variation (i.e., $(\frac{\pi^2}{3}) / ([\frac{\pi^2}{3}] + [Var(MSA)] + [Var(MSA\text{-}year)]) = .99$). Results of likelihood ratio tests (available upon request) suggest a three-level model allowing for random variation at each level is a significant improvement over a two-level model and standard logistic regression model (LaHuis and Ferguson 2009; Schmidt-Catran and Fairbrother 2016). Although Wald and likelihood ratio tests indicate the variance component on the random slope is not significant, LaHuis and Ferguson (2009) note it is still possible to uncover significant cross-level interactions. We investigate this possibility in Model 5.

Model 4 in Table 8 provides a baseline assessment of the correlates of crime reporting without consideration of any cross-level interactions. When compared to Model 1 in Table 3, there is one notable difference relevant to our research questions and hypotheses. We find that Latinos have significantly lower odds, by about 10 percent, of reporting property crime victimization to law enforcement ($p < .001$) relative to white victims. As in our assessment of violent crime, however, we find no evidence that changes in sanctuary policy context are directly associated with crime victimization reporting behavior.

Model 5 in Table 8 examines whether the effect of *ethnicity/race: Latino* varies across immigrant sanctuary policy context. We find no evidence to support this hypothesis. In Model 5, there is no significant interaction between *ethnicity/race: Latino* and the within-effect component of *sanctuary policy*. Furthermore, subsequent investigation of predicted probabilities yields no evidence of moderation (results available upon request). Thus, our results suggest that in the case of reporting crime victimization, the conditional effect of immigrant sanctuary policy context is unique to violent crime victims' likelihood of reporting their experiences to law enforcement.

CONCLUSIONS

Sanctuary policies (in various forms) have existed in the United States since the 1980s. State, county, and municipal officials have adopted sanctuary policies in an effort to resist the devolution of federal immigration enforcement and, in some instances,

Table 8. Multilevel Logistic Regression Models of Victims' Log Odds of Reporting Property Crime Victimization to Law Enforcement, 1980 to 2004

	Model 4		Model 5	
	Coef.	SE	Coef.	SE
Victimization and harm				
Value of property lost[a]	.544***	.004	.543***	.004
Personal characteristics				
Age	.013***	.001	.013***	.001
Edu. attainment: college grad.	.397***	.021	.396***	.021
Edu. attainment: high school	.283***	.018	.283***	.018
Ethnicity/race: Latino	−.109***	.025	−.091***	.029
Ethnicity/race: black	−.029	.019	−.030	.019
Ethnicity/race: other	−.021	.036	−.022	.036
Home ownership: yes	−.011	.014	−.011	.014
Married: yes	.098***	.014	.099***	.014
Sex: female	.186***	.013	.186***	.013
MSA characteristics: within-effects				
Sanctuary policy[b]	.062	.037	.063	.037
% Black	.004	.010	.003	.010
% College graduates	.008	.007	.008	.007
% Crime-prone population	.000	.033	.002	.033
% Democratic voters	−.005	.003	−.005	.003
% Immigrant	.003	.007	.003	.007
% Latino	−.022*	.009	−.023**	.008
% Owner-occ. homes	−.011	.009	−.011	.009
Disadvantage index	.157**	.054	.159**	.054
MSA population[a]	.028	.160	.020	.160
Latino × sanctuary policy			.028	.104
Other × sanctuary policy				
MSA characteristics: between-effects				
Sanctuary policy[b]	−.089	.099	−.094	.096
% Black	.005	.011	.007	.011
% College graduates	−.025*	.010	−.031**	.011
% Crime-prone population	−.032	.040	−.031	.039
% Democratic voters	.010*	.005	.008	.005
% Immigrant	−.005	.008	−.003	.008
% Latino	.024*	.010	.006	.015
% Owner-occ. homes	.011	.010	.009	.010
Disadvantage index	−.265**	.097	−.309**	.098
MSA population[a]	−.091	.165	−.050	.166
Latino × sanctuary policy			−.109	.123
Year fixed effects (1980 to 2004)	Yes		Yes	
Constant	.594	.689	.743	.674
Var(MSA)	.009	.003	.008	.002
Var(MSA-year)	.004	.002	.004	.002
Var(ethnicity/race: Latino)	.029	.018	.027	.018
Observations				
Level-3, MSAs	40		40	
Level-2, MSA-years	996		996	
Level-1, Incidents	136,053		136,053	

[a]Log-value.
[b]Policy adoption lagged one year.
Note: Victimization and harm and personal characteristics are mean centered. *Edu: less than high school* and *ethnicity/race: white* are reference categories. Models include control for NCVS survey redesign, and interactions account for effect heterogeneity in *ethnicity/race: Latino*.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

to promote immigrant integration into local communities. Some policymakers and political actors publicly express concern that sanctuary policies threaten public safety and systematically foster crime, arguing that such policies attract criminally-inclined noncitizens, promote criminal behavior among noncitizens, and shield noncitizens who engage in criminal behavior from deportation. As evidence of such claims, critics of sanctuary policies cite isolated yet highly publicized incidents of violence committed by deportable noncitizens in sanctuary jurisdictions. In contrast, other policymakers, law enforcement officials, and some scholars contend that sanctuary policies engender trust between immigrant communities and local institutions and in doing so, promote the safety and well-being of all residents, regardless of immigration status. Against the backdrop of this political discourse, scholars have increasingly conducted research on the broad topic of sanctuary policies (Collingwood et al. 2019; Collingwood and Gonzalez O'Brien 2019; Gonzalez et al. 2017; Kubrin and Bartos 2020; Lyons et al. 2013; Martínez-Schuldt and Martínez 2019; Wong 2017).

Empirical studies on the sanctuary–crime link, specifically, are relatively limited in number. The few existing studies have found that sanctuary policy adoption either has no effect or modestly reduces macro-level crime (Gonzalez et al. 2017; Kubrin and Bartos 2020; Martínez-Schuldt and Martínez 2019; Wong 2017). In addition, some researchers have found that sanctuary policies enhance the inverse relationship between immigrant concentration and crime (Lyons et al. 2013; Martínez-Schuldt and Martínez 2019). To explain these crime-buffering effects, scholars have speculated that sanctuary policies foster trust between migrant communities and local institutions, improve individuals' willingness to cooperate with crime investigations, and increase the likelihood that immigrants report crime victimization to law enforcement (Lyons et al. 2013; Martínez-Schuldt and Martínez 2019). To our knowledge, no study has empirically examined how the adoption of sanctuary policies affects help-seeking behavior. Instead, prior research has provided sophisticated analyses of crime data across varying immigrant sanctuary contexts with different units of analyses, and then only offered conjecture about how sanctuary policies engender changes in individual-level behaviors relevant to crime control (i.e., odds of notifying law enforcement about crime victimization).

Our study contributes to the existing body of research by situating expectations derived from the sanctuary–crime literature (Lyons et al. 2013; Martinez-Schuldt and Martinez 2019) within the context of a multilevel, contextualized help-seeking framework (Xie and Baumer 2019a). Theoretically, immigrant sanctuary policy contexts should mitigate structural conditions, such as weak societal integration (Black 1976), that deter members of immigrant communities from reporting crimes to the police, build institutional trust within immigrant communities (Lyons et al. 2013; Martínez-Schuldt and Martínez 2019), and ease fears of deportation (Menjívar and Bejarano 2004). As a consequence of these processes, members of immigrant communities or persons socially connected to immigrants should be more likely to report victimization to law enforcement in immigrant sanctuary policy contexts.

Our analyses of more than 35,000 incidents of violent crime and 130,000 incidents of property crime across 40 MSAs and over 25 years find that Latinos have a higher probability (by 12 percentage points) of reporting violent crime victimization to law enforcement after a sanctuary policy is adopted within their MSA of residence. We do not find any evidence that other ethno-racial groups' odds or probabilities of notifying police after victimization vary across sanctuary policy contexts. Crime-reporting behavior is a primary way *individual* victims mobilize the law to receive justice (Black 1973), but we argue that local community members stand to benefit from an increase in crime reporting by Latino victims. Local-level mechanisms of formal social control are generally contingent

on the criminal justice system reacting to denizens' reports of crime (Gottfredson and Gottfredson 1988; Reiss 1992). Furthermore, researchers suggest that effective forms of formal control can deter crime and enhance public safety in areas where informal mechanisms of control based on reciprocal social ties and collective efficacy are otherwise thwarted by weak social networks and structural disadvantage (Carr 2003).

An implication of the criminal justice system's reactive nature is that its effectiveness rests on its perceived legitimacy within local communities and the willingness of local residents to actively engage with law enforcement. Prior research reveals that negative personal interactions with the police, as well as high-profile cases of police misconduct, can undermine the legitimacy of law enforcement and negatively affect rates of police notification (Carr et al. 2007; Desmond et al. 2016). Beyond personal experiences and police misconduct, local policy contexts can shape community members' attitudes toward police as well as their behavior following victimization. In the case of immigrant communities, scholars have noted that local policies related to U.S. immigration enforcement may engender legal cynicism and reduce institutional trust in law enforcement and the criminal justice system (Menjívar et al. 2018; Theodore and Habans 2016; Zatz and Smith 2012). Importantly, our central finding that Latino violent crime victims are more likely to report victimization after sanctuary policies are adopted within their MSAs of residence suggests that changes in local policies may also increase the perceived legitimacy of the criminal justice system—at least to the extent that crime victims are more willing to report victimization.

Several factors may help further elucidate why sanctuary policies are relevant to the decisions of Latino violent crime victims but not other ethno-racial groups in our sample. Relative to the black and white populations in the United States, Latinos are more likely to be foreign-born. Latinos are thus more likely to respond favorably to sanctuary policy adoption given that they, their family members, or members of their broader communities stand to benefit most from shifts in policy contexts related to immigration enforcement or the social control of immigrants. Additionally, unauthorized immigrants overwhelmingly arrived to the United States from Mexico and Central American countries (i.e., over 70 percent during our study period), suggesting Latinos may be more likely to be undocumented, members of mixed-status families, or connected to deportable noncitizens via their immediate social networks. Research finds that the looming threat of deportation, particularly as immigration enforcement has devolved to local authorities, has led to a chilling effect whereby immigrant community members are less willing to access public services for which they are eligible and less amenable to trusting related institutions (Fix and Zimmerman 2001; Pedraza and Zhu 2015; Vargas 2015; Vargas and Pirog 2016). The media has documented instances in which unauthorized Latino immigrants have made victimization reports to local law enforcement, only to be placed in deportation proceedings (Silva 2018). Knowledge of these events may diffuse across communities, weaken Latinos' trust in (or increase their fear of) local-level law enforcement, and discourage migrants or their family members from reporting crime victimization. Therefore, Latinos may disproportionately benefit from the protections offered, or at least claimed, by subnational sanctuary policies.

The effects of immigrant sanctuary policy contexts on crime-reporting behavior appear to be limited to violent crime. Latino victims, relative to white victims, have lower odds of reporting property crime victimization, but we find no evidence that the negative effect associated with Latino identity is mitigated after sanctuary policies are adopted within MSAs. The divergent findings between violent and property crime victimization may be due to the differing natures of these experiences. Property crime victimization may be viewed as less serious, so Latinos may be less

inclined to engage with the criminal justice system even in the context of pro-immigrant policy contexts at the local level. Regardless, our finding that Latinos are more likely to report violent crime victimization after sanctuary policies are passed within their MSAs of residence is encouraging for proponents of sanctuary policies.

Our study also holds broader implications for research related to help-seeking behaviors (Xie and Baumer 2019a). Consistent with micro- and macro-level theoretical expectations, prior studies reveal how experiences with victimization and harm as well as personal characteristics influence whether victims mobilize the law and seek justice (Baumer and Lauritsen 2010; Felson et al. 2002; Gottfredson and Hindelang 1979; Skogan 1984). Recently, scholars have called for greater attention to the role of external environments within which victimization occurs (Xie and Baumer 2019a). Our empirical investigation of the NCVS: MSA Data implies that the majority of the variation in whether crime victims call the police is a function of micro-level factors and, unsurprisingly, we find little evidence that changes in macro-level conditions at the MSA-level during our study period are directly associated with police notification. However, we caution scholars from ignoring external environmental conditions altogether. In the present study, social policy effects are observable in the context of cross-level interactions (e.g., *Latino* × *sanctuary policy*). Our results suggest that social policy effects, or other effects of the external environment, may be conditional on micro-level circumstances. In the case of sanctuary policies, we argue that immigrant sanctuary policy contexts shift the nature of help-seeking experiences for Latinos and eliminate barriers that may otherwise undermine victims' willingness to mobilize the law and seek justice (Menjívar et al. 2018; Theodore and Habans 2016; Zatz and Smith 2012). Future studies of help-seeking behaviors may better identify the role of external environmental conditions (e.g., within neighborhoods, counties, MSAs, states) by situating their theoretical expectations within a multilevel, contextualized framework and by investigating theoretically motivated cross-level interactions (Xie and Baumer 2019a).

The analyses presented here are not without limitations. First, although we leverage the longitudinal nature of our dataset and use a novel hybrid method to correctly estimate the "within-effect" component of our cross-level interactions (Giesselmann and Schmidt-Catran 2019), which are more appropriate for drawing causal inferences (Allison 2009), our estimates are not impervious to sources of bias. We have striven to account for external environmental conditions that are demonstrated or theorized to be related to both help-seeking behavior (Baumer 2002; Felson et al. 2002; Gutierrez and Kirk 2017; Xie and Baumer 2019b) and the passage of sanctuary policies (Collingwood and Gonzalez O'Brien 2019; Gonzalez et al. 2017; Lyons et al. 2013), but it is possible that some other unobserved time-varying condition has led us to over or underestimate the extent to which sanctuary policies condition the probability that Latino victims notify law enforcement. Our supplemental analysis presented in Table 6 provides additional evidence that Latinos' probabilities of reporting crime victimization are systematically higher in the years after sanctuary policies have been adopted, which suggests a unique effect due to or parallel to the adoption of immigrant sanctuary policies. Still, it is important that future scholars attempt to replicate our findings using alternative methods for causal inference.

Second, our study is also limited by the scope of the NCVS: MSA Data, which only provides geographic-identified information of crime victimization up to 2004. Our analyses do not consider the more recent wave of sanctuary policies (Collingwood, Gonzalez O'Brien, and Tafoya 2020). Future research should attempt to replicate our analyses with more recent victimization data that include measures of victims' subnational, geographic locations within the United States. Doing so will expand our understanding of immigrant

sanctuary policy contexts beyond the 25-year period we consider. The continued criminalization of immigration and the devolution of immigration enforcement over the past decade, as well as the recent anti-immigrant positions of the Trump administration, may have undermined local-level attempts to integrate immigrant communities. In addition, the NCVS: MSA Data does not contain information on respondents' immigration status. Immigrants—particularly non-naturalized and unauthorized immigrants—should be most sensitive to sanctuary policies given their precarious status. Due to data limitations, we are only able to model patterns in Latino crime-reporting behavior in accordance with changing policy contexts. If sanctuary policies have no effect on the behavior of Latino *citizens* but do affect noncitizen Latinos, then our effect estimates are likely understated. Alternatively, our results may imply that sanctuary policy contexts operate beyond their theorized influence on immigrant behavior. The effects of immigrant sanctuary policy contexts on help-seeking behavior may extend more broadly to members of immigrant communities, regardless of victims' specific immigration status.

Finally, we only consider the role of sanctuary policy contexts in shaping crime victims' help-seeking behavior in terms of their willingness to call the police. As mentioned, federal immigration enforcement efforts have led to a chilling effect whereby members of immigrant communities, especially people in mixed-status families, are hesitant to engage with other social institutions and access public benefits, in spite of their eligibility (Fix and Zimmerman 2001; Pedraza and Zhu 2015; Vargas 2015; Vargas and Pirog 2016). Future research may consider how, if at all, sanctuary policies or other external environmental conditions structure trust in institutions beyond law enforcement as measured through attitudinal and other behavioral measures.

APPENDIX

Table A1. Two-Way, Fixed-Effects Logistic Regression Models of Victims' Log Odds of Reporting Violent Crime Victimization to Law Enforcement, 1980 to 2004

	Model A1		Model A2	
	Coef.	SE	Coef.	SE
Victimization and harm				
Injury sustained: yes	.609***	.027	.609***	.027
Weapon present: gun	1.275***	.038	1.274***	.038
Weapon present: other	.541***	.032	.541***	.032
Personal characteristics				
Age	.017***	.001	.017***	.001
Edu. attainment: college grad.	.184***	.050	.184***	.050
Edu. attainment: high school	.308***	.034	.309***	.034
Ethnicity/race: Latino	.053	.790	.047	.055
Ethnicity/race: black	.160***	.038	.160***	.038
Ethnicity/race: other	−.080	.066	−.081	.066
Home ownership: yes	−.034	.030	−.033	.030
Married: yes	.332***	.030	.332***	.030
Sex: female	.391***	.035	.390***	.035
Offender: don't know	.217*	.101	.217*	.101
Offender: stranger	.117***	.035	.116***	.034
MSA characteristics				
Sanctuary policy[a]	−.030	.067	.024	.061
% Black	.013	.024	.011	.024
% College graduates	−.014	.020	−.015	.020
% Crime-prone population	.044	.072	.048	.073
% Democratic voters	−.001	.006	−.002	.006
% Immigrant	−.010	.013	−.010	.013
% Latino	.012	.022	.014	.022
% Owner-occ. homes	−.005	.028	−.004	.028
Disadvantage index	−.005	.111	−.016	.110
MSA population[b]	−.722*	.322	−.732*	.329
Latino × sanctuary policy	.533***	.149		
Other × sanctuary policy			.004	.244
MSA fixed effects ($j-1$ dummies)	Yes		Yes	
Year fixed effects (1980 to 2004)	Yes		Yes	
Constant	9.389	4.946	9.472	5.070
Level-1, incidents	35,329		35,329	

[a]Policy adoption lagged one year.
[b]Log-value.
Note: Victimization and harm and personal characteristics are mean centered. *Weapon present: none, edu: less than high school, ethnicity/race: white,* and *offender: known* are reference categories. Models include control for NCVS survey redesign, and interactions account for effect heterogeneity in *ethnicity/race: Latino* and *ethnicity/race: other.*
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table A2. Two-Way, Fixed-Effects Logistic Regression Model of Victims' Log Odds of Reporting Property Crime Victimization to Law Enforcement, 1980 to 2004

	Model A3	
	Coef.	SE
Victimization and harm		
Value of property lost[a]	.543***	.008
Personal characteristics		
Age	.013***	.001
Edu. attainment: college grad.	.397***	.026
Edu. attainment: high school	.284***	.026
Ethnicity/race: Latino	−.116***	.020
Ethnicity/race: black	−.029	.031
Ethnicity/race: other	−.018	.032
Home ownership: yes	−.011	.016
Married: yes	.099***	.017
Sex: female	.186***	.017
MSA characteristics		
Sanctuary policy[b]	.065	.037
% Black	.005	.012
% College graduates	.007	.008
% Crime-prone population	−.011	.047
% Democratic voters	−.006	.004
% Immigrant	.003	.009
% Latino	−.022*	.009
% Owner-occ. homes	−.011	.013
Disadvantage index	.149*	.064
MSA population[a]	.035	.252
Latino × sanctuary policy	.023	.073
MSA fixed effects ($j-1$ dummies)	Yes	
Year fixed effects (1980 to 2004)	Yes	
Constant	−3.298	3.760
Level-1, incidents	136,053	

[a]Log-value.
[b]Policy adoption lagged one year.
Note: Victimization and harm and personal characteristics are mean centered. *Edu: less than high school* and *ethnicity/race: white* are reference categories. Models include control for NCVS survey redesign, and interactions account for effect heterogeneity in *ethnicity/race: Latino*.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

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ORCID iDs

Ricardo D. Martínez-Schuldt https://orcid.org/0000-0002-2672-466X
Daniel E. Martínez https://orcid.org/0000-0001-6773-4269

Notes

1. Note that morphology can also refer to levels of intimacy between victims and offenders (Black 1976). Law is more likely to be mobilized when there is greater relational distance between people. So, we would expect people to be more likely to report victimization to law enforcement when crime incidents involve strangers relative to incidents when persons know each other.
2. Although we focus on police notifications of victimization, Xie and Baumer (2019a) call attention to the fact that crime victims may also seek assistance

and support from family members, friends, or other institutions such as social services agencies.

3. See http://www.ailadownloads.org/advo/NILC-LocalLawsResolutionsAndPoliciesLimitingImmEnforcement.pdf.
4. See San Francisco's Due Process for All and Sanctuary 96-16 ordinance.
5. A recent study of Texas residents found that public opinions of sanctuary policies are correlated with county-level demographic changes related to the Latino population, rather than actual exposure to crime (Collingwood et al. 2019).
6. We use the term "mixed-status family" to refer to family structures composed of at least two individuals with differing immigration statuses (e.g., a U.S. citizen child living with an unauthorized parent).
7. In 1992, a redesign in the NCVS survey affected respondents' reporting of some violent crime victimizations (Kindermann, Lynch, and Cantor 1997). To account for this, we include a dichotomous indicator for whether the survey took place before or after the redesign (see also Xie, Lauritsen, and Heimer 2012).
8. NCVS: MSA Data records respondents' ages categorically. We construct a continuous measure by using midpoint scoring.
9. Again, see http://www.ailadownloads.org/advo/NILC-LocalLawsResolutionsAndPoliciesLimitingImmEnforcement.pdf.
10. See, for example, https://cis.org/Map-Sanctuary-Cities-Counties-and-States.
11. The following MSAs (as defined in the NCSV: MSA Data) adopted a sanctuary policy within at least one of their core counties during our study period: (1) Baltimore, (2) Chicago, (3) Cleveland-Loraine-Elyria, (4) Houston, (5) Los Angeles, (6) New York, (7) Philadelphia, (8) Portland, (9) San Diego, (10) San Francisco, (12) Seattle, and (13) Washington, DC.
12. The *percent Democratic voters* measure was derived from David Leip's atlas of U.S. Presidential elections (see https://uselectionatlas.org/). The database includes county-level voting records for 1980 through 2016. In addition to overall votes cast per county, the database registers the number of votes cast for the Democratic and Republican candidates. We record the total number of votes cast for each core county in the NCVS: MSA Data as well as the total number of votes cast for the Democratic candidate. We aggregated the county-level voting data to the MSA-level, and we calculate the percent of votes cast during each election for the Democratic candidate. Finally, consistent with prior research that analyzes the macro-level correlates of crime and victimization, we use linear interpolation to account for inter-election years (Sharkey, Torrats-Espinosa, and Takyar 2017; Xie et al. 2012).
13. We replicated the models presented in Tables 3 and 8 using standard logistic regression with maximum likelihood estimation. The estimates, which are available upon request, result in the same substantive conclusions.
14. In addition to the models presented in Tables 3 and 4, we also followed the methodological corrections outlined by Giesselmann and Schmidt-Catran (2019) to estimate within-unit interaction terms using a two-way fixed-effects analytic approach (i.e., logistic regression models including MSA and year indicator variables). The models, which are presented as Models A1, A2, and A3 in the Appendix, lead to substantive conclusions identical to those drawn from Models 2 and 3 in Table 3 and Model 5 in Table 7. The similarity in the results is not surprising given that the hybrid modeling approach and fixed-effects models are both estimating within-unit effects (Allison 2009).
15. Linear probability models (available upon request) produced substantively similar results.

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Ricardo D. Martínez-Schuldt is an Assistant Professor of Sociology and a Faculty Fellow in the Institute for Latino Studies at the University of Notre Dame. His research examines processes of international migration in the North American migratory system, with a focus on Mexican immigration to the United States. Additionally, he conducts research on the neighborhood and city-level correlates of crime, crime reporting behavior, and officer-involved shootings.

Daniel E. Martínez is an Associate Professor in the School of Sociology and a co-director of the Binational Migration at the University of Arizona. His research focuses on undocumented immigration, deportation, the criminalization of immigration, and ethnoracial identification among Latinos.

CENTER FOR IMMIGRATION STUDIES OCTOBER 2021

Are Immigrants Less Willing to Report Crime?

Data from the National Crime Victimization Survey says "No."

By Jessica M. Vaughan, Steven A. Camarota, and Karen Zeigler

This report uses new data from the Department of Justice's National Crime Victimization Survey (NCVS), the most authoritative survey on crime reporting, to examine the claim that immigrants are less willing than others to report victimizations to authorities. This theory is one of the most frequently cited justifications for sanctuary policies. We find that the most recent data from the NCVS, which previously did not include information on citizenship and foreign birth of victims, does not support the assertion. The combined results of three years of data from this survey collected from 2017 to 2019 show that immigrant victimizations are just as likely, and in some cases more likely, to be reported to police than crimes against the native-born. This was true even though the data was collected in the first three years of the Trump administration, during which time the media and advocacy groups routinely asserted that immigration enforcement had been dramatically expanded. The data also provides insight on why some immigrants and non-citizens might not report crimes, showing that only a tiny number — about 1 percent — said that the reason for not reporting was because of fear of the authorities. **There is no evidence in the NCVS data that crimes against immigrants are reported to police at lower rates than crimes against the native-born, indicating that the routine, even active, cooperation between local law enforcement and federal immigration authorities that takes place in most jurisdictions does not suppress crime reporting by immigrants.**

Among the findings:

- Sixty-two percent of all **serious** crimes against all immigrants (citizen and non-citizen) were reported to police, compared with 53 percent of all serious crimes against native-born Americans — a statistically significant difference.
- Fifty-seven percent of **serious** crimes against *non-citizen* immigrants (and against Hispanic non-citizens) were reported to police, which is statistically equal to the 53 percent reporting rate for native-born Americans.
- Sixty-one percent of **serious violent** crimes against all immigrants in the survey were reported to police, compared with 49 percent against the native-born — a statistically significant difference. Fifty-nine percent of all non-citizen victimizations and 62 percent of all Hispanic non-citizen victimizations were reported to police.
- Of **serious violent** crimes against immigrant **women**, 65 percent were reported to police, as were 67 percent against non-citizen women, compared with 48 percent for the native-born women — a statistically significant difference.
- Reporting of serious and violent crimes against **Hispanic non-citizens**, a group that includes many illegal aliens, also generally matches or exceeds reporting of crimes against the native-born. If there were a major difference in crime reporting rates among illegal immigrants in particular, it would most likely show up in the reporting rates for this group, but the survey reveals no such meaningful difference.

Jessica M. Vaughan is the director of policy studies, Steven A. Camarota is the director of research, and Karen Zeigler is a demographer at the Center.

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- **Property** crimes (the most common type of crime) against immigrants are reported at rates similar to those against native-born victims — 34 percent. Among non-citizens, the rate was 32 percent, which is not statistically different from the other groups.

- Turning to **all crime** (violent and property), the NCVS shows 39 percent of all immigrant victimizations and 38 percent of all non-citizen victimizations were reported to police, compared with 37 percent of all crimes against the native-born — not a statistically significant difference.

- For every major category of crime, only about 1 percent of immigrant victims who did not report the crime said that the reason they did not report it was because the police would be biased, would harass or cause them trouble, or because they were advised not to report it. These are the survey responses that are most likely to indicate a fear of deportation.

- More than 80 percent of all victimizations reported to police were reported by the victim or by a member of the victim's household. This is true for all groups analyzed: immigrants, non-citizens, and the native-born.

- We find no evidence in the NCVS data to support the "chilling effect" theory that immigrants are more reluctant to report crimes, generally, or in the parts of the country where local authorities routinely cooperate with ICE, such as the South.

Introduction

Discussion of crime reporting by immigrants has increased in recent years, as cooperation and communication between state and local law enforcement agencies has been significantly improved by the implementation of various information and resource sharing programs to facilitate the arrest and removal of criminal aliens, including the Secure Communities and 287(g) programs. The Secure Communities program enables the federal Immigration and Customs Enforcement agency (ICE) to access and review biometric information on all those arrested or booked into jails nationwide, and the 287(g) program allows state and local law enforcement officers to arrest and process criminal aliens.[1]

Opposition to the implementation of these programs led to an increase in state and local sanctuary policies, which aim to prohibit or limit state and local law enforcement agencies from sharing information or cooperating with federal immigration agencies, especially ICE. Proponents of sanctuary policies often claim that they are necessary to ensure that immigrants, and especially illegal immigrants, will report crimes. Police cooperation with ICE, the argument goes, has a "chilling effect" on the willingness of immigrants, or those they live with, to report crimes to police because doing so will expose them to immigration enforcement and deportation.

For example, the sponsor of a perennial Massachusetts bill that would outlaw cooperation with ICE recently said:

> *In a city like Lowell* [with a large foreign-born population]*, those immigrants think that the local police officer is engaged in enforcing immigration law, they're not going to report a crime or domestic violence. They won't have that trust with police. That's a main mission of this bill is to restore that trust."*[2]

Similarly, the Colorado speaker of the house said in justifying the need for a sanctuary bill in 2019: "The immigrant population in Colorado [is] a big part of our community and right now with the current administration, people don't always feel safe. They don't feel safe reporting crimes, they don't feel safe going to public spaces."[3]

President Biden's nominee for the position of commissioner of Customs and Border Protection (CBP), Chris Magnus, who is chief of police in Tucson, Ariz., also has promoted this theory. In a 2017 op-ed in the New York Times, Magnus wrote: "If people are afraid of the police, if they fear they may become separated from their families or harshly interrogated based on their immigration status, they won't report crimes or come forward as witnesses."[4]

On the non-partisan "explainer" website Britannica Procon.org, which presents brief summary arguments from prominent officials or experts on each side of major contemporary issues, four out of the five statements by top advocates selected to

represent the pro-sanctuary position mentioned concerns about immigrant crime reporting as a justification for sanctuary policies.[5]

The text of the sanctuary ordinance for San Francisco County, Calif., one of the strictest in the nation, cites the case of an illegal alien who was arrested by ICE in 2016 after a routine interaction with a San Francisco police officer.[6] Advocates often point to the 2017 arrest of an illegal alien (with several criminal convictions and five prior deportations), outside of an El Paso courthouse, where she had been seeking a restraining order on her boyfriend, as an illustration of the alleged risk illegal immigrants face if they report victimizations to local police.[7]

Law enforcement agencies contend that these examples are not at all representative of most interactions immigrants — legal or illegal — have with police, and are skeptical that their cooperation with ICE discourages immigrants from reporting crimes. They maintain that immigrants are more afraid of criminals in the community than they are of authorities.[8]

Other Research on Immigrant Crime Reporting

There is a sizeable body of recent academic research on the topic of immigrant crime reporting that reflects the ongoing debate on the "chilling effect" theory. Yet many of the published studies that we have reviewed have significant limitations, which we explore below. None of the academic studies we have seen are as comprehensive and authoritative as the federal NCVS, which is a nationally representative set of data that surveys actual victims about the victimizations, whether they were or were not reported to police, who reported them, and reasons why crimes were not reported. The 2017 public-use NCVS is the first year that citizenship is available in the NCVS, enabling us to examine reporting rates for self-reported immigrants (foreign-born), native-born, citizens, and non-citizens, as well as by ethnicity. To the best of our knowledge, ours is the first report to use this new data to measure the propensity of immigrant victims to report crimes to police.

Some of the most frequently cited studies on this topic rely on relatively narrowly drawn surveys of immigrants who are asked to respond to hypothetical situations. For example, a 2013 report by a University of Illinois researcher asked immigrants and natives in four U.S. cities if they would be less likely to report crimes after being told that there was increased police involvement in immigration enforcement.[9] Others have surveyed law enforcement officers, courts, or advocacy groups about what they have observed or suspect about crime reporting by immigrants.[10] In contrast, the NCVS surveys real crime victims about their actual response after a victimization.

Still other studies report and compare the results of surveys of immigrants in particular local jurisdictions, which may or may not be representative of or similar to other parts of the country.[11] One of the more comprehensive and authoritative of these local studies was conducted by University of Virginia and Police Executive Research Foundation researchers under contract to the police department of Prince William County, Va.[12] On the specific issue of crime reporting, the researchers found that "the rates of reporting were nearly identical for Hispanics and non-Hispanics, and were statistically indistinguishable," but their survey did not identify immigrants and citizens. And it is unclear to what extent the findings would be applicable to the country as a whole.

A new study, published in January 2021, claims to show conclusively that immigration enforcement, and particularly immigration enforcement in cooperation with local law enforcement, discourages illegal aliens and Hispanics in general from reporting crime. The title of the paper is "Immigration Policies and Access to the Justice System: The Effect of Enforcement Escalations on Undocumented Immigrants and Their Communities", by Reva Dhingra, Mitchell Kilborn, and Olivia Woldemikael.[13] Because this paper also uses the NCVS to test its theory, and because it has appeared in major news media outlets, including the Washington Post, and been cited by other researchers, we believe it merits a closer look.

We find a number of significant problems with their analysis. Using the FBI's Uniform Crime Reporting (UCR) Program, Dhingra et al. found that incidents of crime declined more in counties with larger Hispanic populations (immigrant and native-born) from 2016 to 2017. Over the same time period, they also found declines in crime in those counties around the country that cooperate more with ICE. The authors conclude that the observed decline in crime was caused by Hispanics reporting fewer crimes to police out of fear of deportation due to the Trump administration's intensification of immigration enforcement, assisted by an increase in cooperation between local police and ICE. They do not believe that there was an actual decline in crime (which could have occurred due to a variety of reasons that have nothing to do with immigration

enforcement, such as police budgets, changing policing strategies, the economy, and even the weather), or believe that other possible explanations could account for the decline in crime, such as the possibility that removing criminal aliens helps reduce crime, especially in immigrant communities.

To test their theory, the authors used the NCVS data on the crime reporting rates of Hispanic populations. However, they present the data on Hispanics in isolation — without showing how it compares to the crime reporting rates of other population groups, including the native-born, a vital source of context. Dhingra et al. find that the share of crimes reported against all Hispanics (immigrant and U.S.-born together) dropped by 12 percentage points between 2016 and 2017. They do not report that in the NCVS, Hispanic victimizations in 2017 were still slightly more likely to be reported to police than non-Hispanic victimizations — 44.9 percent vs. 44.6 percent. (See Figure A1 in the Appendix for crime reporting by Hispanics and non-Hispanics from 2007 to 2019 using the public-use NCVS.) A fair interpretation of the NCVS data is that Hispanics were more likely to report crime than non-Hispanics in 2016 (54.2 percent vs 41.9 percent), and then were equally likely in 2017 (44.9 percent vs 44.6 percent).[14]

Further, the new NCVS data, which Dhingra et al. may not have been aware of, shows that the share of all Hispanic victimizations reported fell further in 2018 to 37.2 percent, which might be seen as buttressing the author's arguments, but it rebounded dramatically in 2019 to 47.8 percent, while the share of crimes against non-Hispanics that were reported dropped to 39.1 percent in 2019.[15] What this shows is that there is considerable annual variation in the NCVS data from year to year. We believe this reflects the relatively small sample size of Hispanic victims in the survey when using a single year of data, which creates large margins of error.[16] In the years the authors focus on, the margin of error for Hispanic victims is roughly ±8 percentage points.[17] Regardless, if one thinks that the change from 2016 to 2017 represented a meaningful decline in crime reporting by Hispanics, then the 2019 rebound should be seen as representing a meaningful increase, and that the initial decline was likely temporary.[18]

As we show in this report, newer NCVS data on the citizenship of victims also calls into question the idea that the change from 2016 to 2017 in Hispanic crime reporting was due to a fear of deportation. (See Figure A2 in the Appendix for crime reporting by citizenship.) Each of the last three years of NCVS data shows higher rates of crime reporting for immigrant Hispanics (naturalized citizen and non-citizen) than for native-born non-Hispanics; and, in some years, the difference is statistically significant. Further, in all three years, the reporting rates for Hispanic non-citizen victimizations (a reasonable proxy for illegal aliens and their family members) reported to police were higher than the reporting rates for non-Hispanic, native-born Americans, with a statistically significant difference in 2019.[19]

For all these reasons — failure to consider the many reasons for fluctuations in crime rates, failure to consider Hispanic crime reporting rates in the context of other groups, reliance on too brief a period of analysis of crime reporting rates, and, most importantly, the availability of new data on citizenship — we believe the findings of the study by Dhingra et al. are not persuasive.

Data and Methods

This analysis examines the reporting of crimes against immigrants and the native-born using the annual National Crime Victimization Survey, conducted by the U.S. Census Bureau for the Bureau of Justice Statistics (BJS), which is within the U.S. Department of Justice. The survey is specifically designed to measure the number and nature of criminal victimizations, whether they are reported to police, and, if not reported, the reasons for not reporting.[20] Beginning in 2017, the survey began to include questions on U.S. or foreign birth and citizenship status, among other updates.

We classify crimes using the same categories that the BJS uses in its publications about the NCVS. The sidebar summarizes the crimes included in each category. In its publication, BJS tends to focus on serious crimes, which are those generally prosecuted as felonies, and violent crime.[21] We follow this example, but also include figures for less serious crimes, property crimes, and all crimes.

We combine the three years of data (2017, 2018, and 2019) to allow for more statistically robust estimates, as the sample sizes of crime victims in individual years for many of the sub-populations of interest are quite small.[22] The survey reports

incidents of victimization; some individuals can be a victim more than once during the year.[23]

Calculating Unbiased Standard Errors. All standard errors from the NCVS used in this analysis to calculate confidence intervals and statistical significance are based on the parameter estimates and formulas provided by the BJS.[24] We use the year-to-year correlation coefficients from the BJS for pooled data to calculate standard errors as we are using the 2017 to 2019 NCVS. The use of parameter estimates and coefficients accounts for the complex design of the NCVS, plus the fact that it is a panel survey.[25] We use 90 and 95 percent confidence levels to determine statistical significance when comparing crime reporting rates between groups.

Citizenship. The 2016 redesign of the NCVS included the addition of a citizenship question. This question was first asked in July 2016 but was not available in the public-use files of the survey until 2017. The inclusion of this question allows researchers to examine all immigrants (naturalized citizens and non-citizens) together or separately.[26] For the purposes of this analysis, immigrants are defined as all those residing in the United States who were not U.S. citizens at birth, including naturalized citizens, lawful permanent residents, temporary workers, foreign students, and illegal aliens.[27] The U.S. Census Bureau and the BJS, as well as other government agencies, typically refer to this population as the "foreign-born". Here we use the terms "immigrant" and "foreign-born" synonymously.[28] We also use the terms "native-born", "native", and "U.S.-born" interchangeably for all those who were U.S. citizens at birth.

Illegal Immigrants. It is well established that illegal immigrants do respond to Census Bureau-administered surveys.[29] To the best of our knowledge, there has been no specific research measuring the inclusion of illegal immigrants in the NCVS. But in comparing the Census Bureau surveys that capture illegal immigrants, we see that the number of foreign-born individuals in the Census surveys is similar to the number in the NCVS. Though there may be some undercount of illegal immigrants in the NCVS, as with the other surveys, illegal immigrants should show up in the NCVS as non-citizens.[30] Confining the analysis to Hispanic non-citizens should provide a good deal of insight into the willingness of illegal immigrants, or those who live with them, to report crimes because a significant share of Hispanic non-citizens are illegal immigrants or live with illegal immigrants.[31]

Types of Crimes

Serious Crime: Serious violent and serious property crimes.

Serious Violent Crime: Rape, sexual assault, robbery, and aggravated assault.

All Violent Crime: All serious violent crime, plus simple assault, threats, including those of sexual and non-sexual nature and sexual contact.

Serious Property Crime: Completed burglaries and motor-vehicle thefts.

All Property Crime: All serious property crimes, plus attempted burglary, residential trespassing, thefts of money or property of any amount.

All Crime: Includes all violent and all property crimes and several types of more minor crimes against persons, such as pick-pocketing and purse snatching.

See end note 21 for more detail.

Purpose of This Analysis. The primary goal of this analysis is to measure the crime reporting rates of immigrants in comparison to other groups to determine if immigrants have significantly different crime reporting rates than others. We compare the reporting of crimes against immigrants (both the foreign-born and non-citizens) to the reporting of crimes against the native-born. A secondary goal of this analysis is to explore whether cooperation between Immigration and Customs Enforcement (ICE) and local police has a measurable impact on the reporting of crimes by the foreign-born. While the NCVS is not ideal to examine this question because it does not identify specific states or cities where the respondents reported their victimizations, it is possible to compare crime reporting across different regions of the country and in communities of different sizes to shed some light on this question.

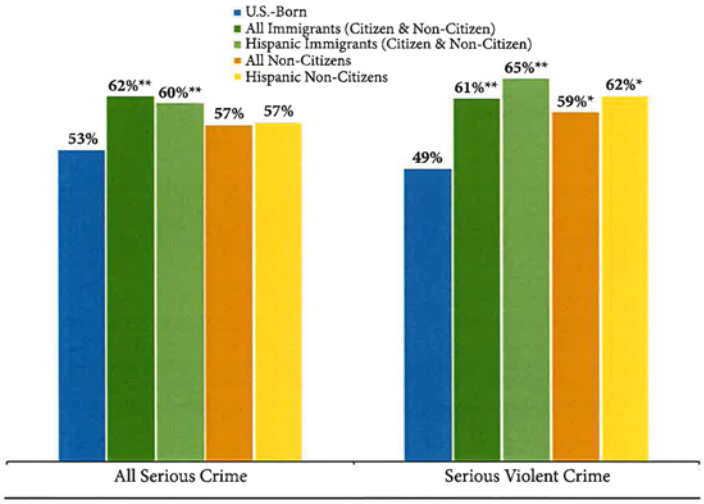
Findings

Immigrant Victim Crime Reporting Rates Are Similar to or Higher than the U.S.-Born

Serious Crimes. The Department of Justice defines serious crimes as those that are generally prosecuted as felonies (see sidebar). These include most completed or attempted violent crimes, apart from simple assault, and completed burglaries and motor-vehicle thefts. The left side of Figure 1 shows the share of victimizations reported to police for all serious crimes

by the nativity and citizenship of the victim, with a separate breakdown of Hispanics in these classifications. (Table A1 in the Appendix provides additional information on the various types of crimes reported in Figures 1 and 2.)

Figure 1. Percentage of Serious Crime and Serious Violent Crime Victimizations Reported to Police by Nativity and Citizenship, 2017-2019



Source: 2017- 2019 NCVS, which measures reported victimizations for persons ages 12 and older.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.
See text for definition of types of crimes.
Table A1 contains more detailed information.

In the combined 2017 to 2019 sample of the NCVS, 62 percent of serious crimes against immigrants were reported to police, compared to 53 percent of such crimes against the U.S.-born. The higher rate of reporting for foreign-born victimizations is statistically significant at a 95 percent confidence level, which means that we can be 95 percent certain that an actual difference exists in the population. The same is true for Hispanic immigrants. The reporting rate for all serious crimes also is higher for non-citizens than for the native-born, but the differences are not statistically significant.[32]

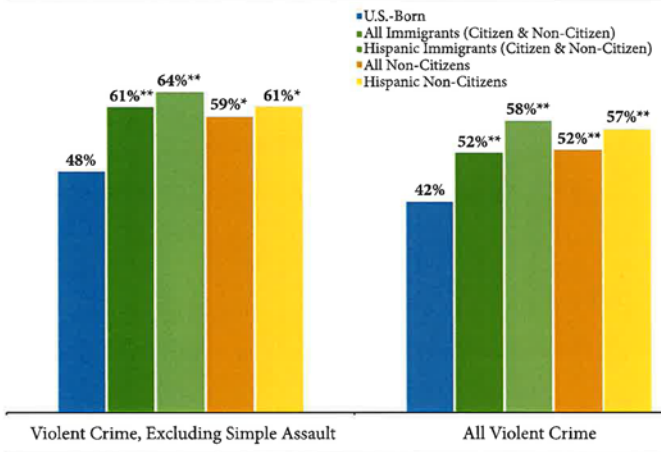
Figure 1 also shows that crimes against the three sub-groups of immigrants (Hispanic immigrants, all non-citizens, and Hispanic non-citizens) are reported to police at rates that are at least as high as the reporting of crimes against the U.S.-born.

Serious Violent Crime. Serious violent crime is defined by the BJS to include rape, sexual assault, robbery, and aggravated assault. This is the category of crime that is most impactful on victims and public safety, and the type of crime that the police, prosecutors, and community leaders are most interested in having reported to them so that offenders can be prosecuted. The right side of Figure 1 shows the share of *serious violent crimes* reported to police.

Figure 1 shows that serious violent crimes against the foreign-born are more likely to be reported to police than crimes against native-born victims. Hispanic immigrant, non-citizen, and Hispanic non-citizen victimizations for serious violence are reported to police at rates that match or exceed those of the native-born, and the differences are statistically significant. At 61 percent, the share of serious violent crimes against all immigrants (citizen and non-citizen) reported to police is much higher than the 49 percent for the native-born, and the difference is statistically significant. Similarly, 65 percent of serious violence against foreign-born Hispanics was reported to police, compared to the 49 percent of such crimes against the U.S.-born — a statistically significant difference. At 59 percent and 62 percent respectively, the share of serious violent crimes against all non-citizens and Hispanic non-citizens reported to police is higher than the 49 percent of serious violent crimes against native-born Americans.

Other Types of Violent Crime. The BJS data also includes a breakdown for the broader category of violent crime, both including and excluding simple assault.[33] The results are shown in Figure 2. This category includes all crimes of a sexual nature (including sexual threats), aggravated assault, and all types of robbery. The left side of Figure 2 shows the share of violent crimes, excluding simple assault, reported to police. As is the case with serious violent crime, immigrant victimizations are at least as likely to be reported as victimizations of the native-born when the crime is violent. In the category of violent crime, excluding simple assault, crimes against all immigrants (61 percent) and Hispanic immigrants (64 percent) are more likely to be reported than violence against natives (48 percent) and the difference is statistically significant. The reporting rates for all non-citizens and Hispanic non-citizens is also significantly higher than the rate for natives.

Figure 2. Percentage of Violent Crime Victimizations Reported to Police by Nativity and Citizenship, 2017-2019



Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.
See text for definition of types of crimes.
Table A1 contains more detailed information.

The right side of Figure 2 reports all violent crime, including simple assault. Simple assault is the most common type of violence, comprising more than half of all violent victimizations. It also tends to be the least reported and is generally the least serious. Figure 2 shows that when we consider all violent crime, including simple assault, we again see no evidence that immigrant victimizations are less likely to be reported to the police than crimes against the U.S.-born. Violent crimes, broadly defined, against immigrants, Hispanic immigrants, non-citizens, and non-citizen Hispanics are all reported to police at significantly higher rates than crimes against the native-born.

Taken together, Figures 1 and 2 show that, contrary to what is commonly claimed, for serious crimes and violent crimes, foreign-born victimizations are reported to police at the same or even slightly higher rates than victimizations of the U.S.-born. This finding is consistent across different categorizations of immigrants and different sets of crimes.

Most Immigrant and Native Victimizations Are Reported by the Victim

Who Reported the Crimes? The NCVS also asks victims about who reported the crimes to police. Possible responses include the victim, another household member, an official, police at the scene, or someone else. Table 1 shows the survey responses. Of serious crimes reported to police when the victim was U.S.-born, 73 percent were reported to police by the victim, and 10 percent were reported by a member of the victim's household. Of serious crimes reported to police when the victim was an

Table 1. Persons Reporting Crimes to Police, 2017-2019

Crime	Nativity	Respondent (Victim)	Other Household Member	Some Official[1]	Someone Else	Police Were at the Scene	All Others[2]
All Serious	U.S.-Born	73.3%	9.5%	4.3%	9.7%	1.9%	1.3%
	All Immigrant	76.2%	10.6%	4.9%	6.9%	0.8%	0.6%
	Non-Citizen	74.0%	10.3%	7.2%	7.6%	0.8%	0.1%
Serious Violent	U.S.-Born	65.2%	8.7%	8.2%	13.3%	3.0%	1.6%
	All Immigrant	72.8%	9.4%	7.8%	8.5%	1.4%	0.0%
	Non-Citizen	70.5%	7.4%	11.0%	9.9%	1.2%	0.0%
Violent (Excluding Simple Assault)	U.S.-Born	63.8%	8.5%	8.8%	12.7%	4.6%	1.5%
	All Immigrant	73.4%	9.2%	7.6%	8.3%	1.4%	0.0%
	Non-Citizen	71.5%	7.2%	10.6%	9.5%	1.1%	0.0%
All Violent	U.S.-Born	62.6%	7.4%	9.1%	12.4%	7.1%	1.5%
	All Immigrant	66.8%	7.2%	6.3%	9.2%	8.8%	1.7%
	Non-Citizen	72.6%	5.8%	9.9%	9.1%	1.3%	1.3%
Serious Property	U.S.-Born	79.3%	10.0%	1.4%	7.1%	1.0%	1.2%
	All Immigrant	78.7%	11.4%	2.8%	5.6%	0.4%	1.1%
	Non-Citizen	77.9%	13.5%	2.9%	5.0%	0.5%	0.2%
All Property	U.S.-Born	78.7%	7.8%	3.1%	7.8%	1.1%	1.5%
	All Immigrant	77.1%	10.6%	4.1%	6.7%	0.6%	0.9%
	Non-Citizen	77.4%	10.4%	3.8%	7.0%	0.7%	0.6%
All Crime	U.S.-Born	72.9%	7.6%	5.3%	9.5%	3.2%	1.5%
	All Immigrant	73.7%	9.4%	5.0%	7.5%	3.3%	1.2%
	Non-Citizen	75.4%	8.7%	6.4%	7.8%	0.9%	0.8%

Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
[1] Examples include guards, apartment managers, and school officials.
[2] Includes other victims or the offender was a police officer or not recorded.

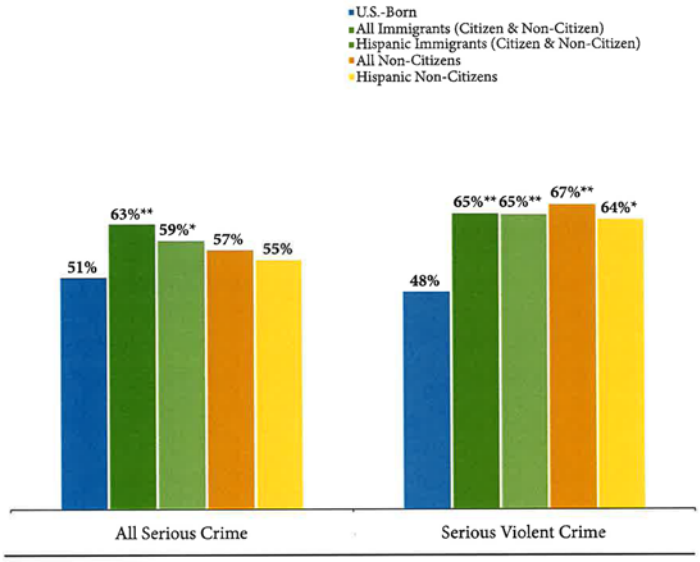
immigrant, 76 percent of the time the victim reported it and 11 percent of the time a member of the immigrant's household reported it. For every type of crime, it is typically the victim or a member of the victim's household who reported it to police, and this is true for both native-born Americans, the foreign-born, and non-citizens.

The responses shown in Table 1 indicate that the relatively high rates of crime reporting associated with immigrants are not due to someone other than the immigrant victim or a household member reporting the crimes. Immigrant victims are at least as willing as U.S.-born victims to report crimes to police.

Reporting of Violence Against Immigrant and Native-Born Women Is Similar

Violence Against Women. There is a long-standing concern that immigrant women are especially reluctant to report violent crimes, particularly those of a sexual nature or those committed by an intimate partner. The NCVS does identify crimes committed by domestic partners as a subset of violent crime, but the number of female victimizations is relatively small, at least as captured in the NCVS. As a result, the sample of female immigrant victimizations for domestic partners is too small to provide reliable results for reporting of this specific crime. However, we can look at female victimizations for serious crime or serious violent crimes using the survey.

Figure 3. Percentage of Serious Victimizations of Females Reported to the Police by Nativity and Citizenship, 2017-2019



Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.
See text for definition of types of crimes.
Table A2 contains more detailed information.

The left side of Figure 3 shows the percentage of serious crimes reported to police when the victim is a woman. Serious crimes against all immigrant women and Hispanic immigrant women are significantly more likely to be reported than those against U.S.-born women.

As for serious violent crimes, the right side of Figure 3 shows that, relative to U.S.-born women, serious violence against immigrant women (including Hispanics) and against all non-citizens and Hispanic non-citizens is significantly more likely to be reported to police than are serious violent crimes against native-born women. Figure 3 also shows that police are being informed of the larger category of all serious crimes against foreign-born women at higher rates than for native-born women. Table A2 in the Appendix reports additional categories of violence against women as well as sample sizes and confidence intervals. Like Figure 2, Table A2 shows that violent crimes against immigrant women are reported to police at rates that either match or are higher than reporting rates for U.S.-born women.

As with reporting for all crimes, the survey reveals that violence against women is reported primarily by the victim herself or, less frequently, by members of her household, as shown in Table 2. This is true both for immigrant and U.S.-born female victims.

Table 2. Persons Reporting Crimes Against Women to the Police, 2017-2019

Crime	Nativity	Respondent (Victim)	Other Household Member	Some Official[1]	Someone Else	Police Were at the Scene	All Others[2]
All Serious	U.S.-Born	73.3%	9.6%	4.0%	10.2%	1.7%	1.3%
	All Immigrant	72.3%	14.7%	3.7%	7.8%	0.8%	0.7%
	Non-Citizen	73.3%	16.1%	4.7%	5.2%	0.5%	0.2%
Serious Violent	U.S.-Born	68.2%	7.4%	7.4%	13.0%	2.8%	1.2%
	All Immigrant	75.9%	10.8%	4.7%	7.0%	1.6%	
	Non-Citizen	79.4%	6.7%	7.3%	6.5%		
All Violent	U.S.-Born	67.2%	5.8%	8.5%	12.4%	4.9%	1.2%
	All Immigrant	63.9%	9.0%	2.8%	8.7%	13.6%	1.9%
	Non-Citizen	81.2%	6.2%	4.4%	8.2%		
All Crime	U.S.-Born	74.0%	7.4%	5.0%	9.6%	2.5%	1.5%
	All Immigrant	68.8%	12.9%	4.2%	7.6%	4.9%	1.5%
	Non-Citizen	73.1%	13.1%	4.8%	7.9%	0.4%	0.8%

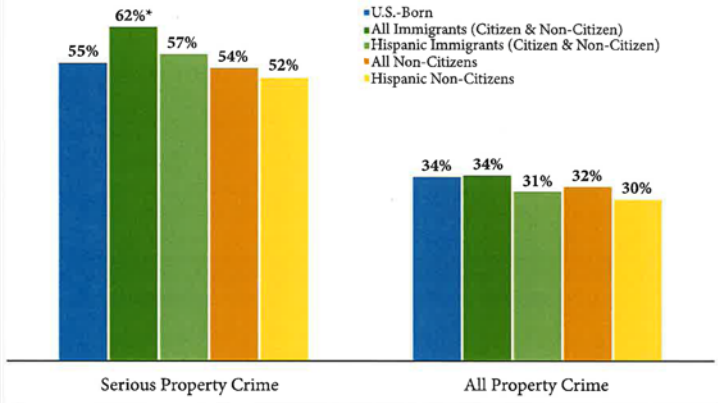
Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
[1] Examples include guards, apartment managers, and school officials.
[2] Includes other victims or the offender was a police officer or not recorded.

This data should not be interpreted to mean that reporting of crime and violence against women, immigrant or native-born, is not an issue of concern. Rather, it shows that assertions that immigrant women in particular are more reluctant to report victimizations than other women are not supported by the NCVS data.

Immigrants and the Native-Born Report Property Crimes at Similar Rates

Property Crime. Property crime includes burglary (unlawful or forcible entry involving theft), attempted forced entry, motor vehicle theft (attempted or completed), and thefts of money or property of any amount. Serious property crime includes burglary and completed motor-vehicle theft. Figure 4 illustrates that 62 percent of all serious property crimes against immigrants in the NCVS survey were reported to police, which is higher than the 55 percent of serious property crimes reported against the U.S.-born in the survey. The difference is statistically significant at the 90 percent confidence level.

Figure 4. Percentage of Property Crimes Reported to Police by Nativity and Citizenship, 2017-2019



Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
See text for definition of types of crimes.
Table A3 contains more detailed information.

Figure 4 also shows that the reporting rates for serious property crimes in the survey were higher for Hispanic immigrants than for the native-born, but the difference is not statistically significant. In fact, none of the other differences in Figure 4 are statistically significant for serious property crimes except when the native-born are compared to all immigrants.

The right side of Figure 4 shows reporting rates for all property crimes, which are the same for immigrants and the native-born — 34 percent. Hispanic immigrants and non-citizens have reporting rates that are slightly lower than natives, but these modest differences are not statistically significant. It is worth noting that the category of all property crimes includes a relatively large number of relatively small incidents. The number of serious property crimes is a relatively small share of the total. Thefts are by far the largest number of property crimes, and about one-quarter of all thefts in the NCVS are valued at less than $50 by the victim. So the relatively low reporting rates shown on the right side of Figure 4 reflect the fact that many victims do not consider the crime to be very serious or worth reporting to police. Still, the survey data shows no meaningful difference between the share of property crimes reported to police when the victim is foreign-born or U.S.-born. Tables A1, A2, and A3 in the Appendix provide additional information about property crimes against immigrants and the U.S.-born.

No Indication of Lower Reporting Rates for Illegal Immigrants

As described above, advocates for illegal immigrants and some officials frequently express concern that illegal immigrants refrain from reporting crimes because they fear that local police will turn them over to federal immigration authorities. This is despite long-standing ICE policy to generally refrain from initiating deportation proceedings in the case of crime victims and witnesses, and despite near-universal policies of local law enforcement agencies not to question victims and witnesses about their immigration status.

The NCVS does not collect information on immigration status, so it is impossible to identify definitively respondents who are illegal aliens. However, it is possible to construct a proxy for this population based on other reliable data. As discussed in the Methods section above, we can be confident that illegal immigrants participate in the NCVS. According to the Census Bureau's 2018 American Community Survey (ACS), which is often used by researchers to estimate illegal immigration, we find that 46 percent of the non-citizen Hispanics aged 12 and older and non-institutionalized are residing in the country illegally, and that another 23 percent are legal immigrants who live in the same household as an illegal alien.[34] (All NCVS respondents are age 12 and older and all are non-institutionalized, so our selected ACS population matches the NCVS.)

If we assume that the shares in the NCVS are roughly similar, then about two-thirds of non-citizen Hispanics in the NCVS (69 percent) are either illegal immigrants themselves or live with an illegal immigrant — meaning that this is the group that would be most likely to refrain from reporting crimes to police according to the "chilling effect" theory. If fear of deportation truly causes illegal immigrants or their family members to be significantly less willing to report victimizations, then the share of crimes against non-citizen Hispanics in the NCVS that were reported to police should be quite low relative to the native-born. But this is not the case.

The results of the NCVS presented in the figures and tables above consistently show that Hispanic non-citizens in the survey reported victimizations to police at rates that match or exceed the crime reporting rates of the U.S.-born — for serious crimes, violent crimes, and all crimes together. In particular, violence against Hispanics who are not citizens tends to be reported to police at rates that are significantly higher than the native-born. The reporting of property crimes against non-citizen Hispanics tends to be somewhat lower than the U.S.-born, but the differences with natives are not statistically significant.

Hispanic non-citizens are not a perfect proxy for illegal immigrants and their family members; about one-fourth of illegal immigrants are not Hispanic, and about one-third of Hispanic non-citizens are neither illegal aliens nor residing with them. Nevertheless, to the best we can measure in the NCVS, there is no indication that crimes against Hispanic illegal immigrants are reported to police at significantly lower rates than the native-born.

Younger Immigrants Report Crimes at Similar Rates as Natives

A number of academic studies over the years also have found that immigrants generally, including non-citizens, are just as likely as the native-born to report crimes to police, but that recently arrived immigrants may be more reluctant to report victimizations to police.[35] Unfortunately, we cannot examine the reporting rates of new arrivals directly because, unlike some other government surveys, the NCVS does not ask the foreign-born what year they came to live in the United States.

However, in the same way that the category of Hispanic non-citizens can be used as a proxy for illegal immigrants, an analysis of the reporting rates of these younger immigrant victims could offer insight into whether or not recent arrivals are less likely to report crimes. The Census Bureau's American Community Survey (ACS) does ask about year of arrival, and it shows that about half of all immigrants who are age 12 to 34 arrived during the previous 10 years. Of non-citizens in particular, the ACS shows that roughly two-thirds of those under age 35 came during the last 10 years.

Table 3 shows that about 54 percent of serious crimes against immigrants under age 35 and 51 percent of serious crimes against the U.S.-born under age 35 were reported to the police. For young non-citizens in this age group, about 50 percent of serious crimes were reported.

As for serious violent crimes against those under age 35, about 54 percent were reported to police when the victim was an immigrant or a non-citizen, compared to 46 percent for native-born victimizations. None of these differences are statistically

Table 3. Percentage of Victimizations of Persons Under 35 Reported to Police by Nativity and Citizenship, 2017-2019

Crime	Nativity	Share of Victims Who Reported Crime to Police	95 % C.I.	Sample Size
All Serious Crime	U.S.-Born	50.6%	2.6	2,096
	All Immigrants (Citizen & Non-Citizen)	54.2%	7.6	195
	All Non-Citizens	49.8%	8.9	142
All Serious Violent Crime	U.S.-Born	45.6%	4.6	1,033
	All Immigrants (Citizen & Non-Citizen)	54.1%	11.8	91
	All Non-Citizens	54.2%	13.3	65
Violent Crime (Not Assault)	U.S.-Born	44.1%	4.8	1,127
	All Immigrants (Citizen & Non-Citizen)	54.1%	12.4	97
	All Non-Citizens	54.4%	13.8	70
All Violent Crimes	U.S.-Born	36.9%	3.3	2,877
	All Immigrants (Citizen & Non-Citizen)	43.2%	8.9	203
	All Non-Citizens*	46.7%	10.7	137
Serious Property Crime	U.S.-Born	58.7%	4.7	1,063
	All Immigrants (Citizen & Non-Citizen)	54.4%	13.5	104
	All Non-Citizens*	43.6%	16.1	77
All Crimes	U.S.-Born	34.3%	1.4	10,542
	All Immigrants (Citizen & Non-Citizen)	35.9%	4.4	970
	All Non-Citizens	36.1%	5.4	648

Source: Combined public-use sample of the 2017-2019 National Crime Victimization Surveys, persons 12 to 34.
See text for definition of types of crimes.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.

significant. There are only two statistically significant results in Table 3. First, 47 percent of all violent crimes against young non-citizen victims were reported, which is significantly higher (90 percent confidence level) than the 37 percent share of violent crimes against young natives. Second, the lower reporting rate of crimes against non-citizens (44 percent) relative to the native-born (59 percent) for property crimes is also statistically significant at the 90 percent confidence level. But overall there is no clear evidence in this data for the theory that newcomers (using the proxy of younger immigrants) are less likely to report crimes than the native-born.

Fear of Police, Deportation Not Major Hindrances to Immigrant Crime Reporting

Advocates for sanctuary policies frequently assert that immigrants are wary of reporting victimizations out of concern that local authorities will turn them over to immigration authorities. The responses of immigrant victims in the NCVS who say they did not report crimes do not support this theory. The survey asks those who did not report their victimizations for the most important reason they did not come forward about their victimization. The responses, further broken down according to the category of crime, are shown in Table A4. Because most serious and violent crimes *are* reported by immigrants, the sample sizes for victims who did not report are very small. Moreover, there are 21 different possible responses in the survey. Therefore, Table A4 shows the responses for all the sub-groups in the immigrant population only in the category of "All Crimes".

In Table A4, the reasons are sorted from the most to least common answer given by immigrants for not reporting a serious crime. The top reasons given by immigrants for each category of crime are highlighted in yellow and the top reasons given by the native-born are highlighted in green. The top two responses given by immigrant victims of all categories of crimes that were not reported to police were: "Police wouldn't think it was important enough, wouldn't want to be bothered or get involved" and that there was a "Minor or unsuccessful crime, small or no loss, recovered property".

While "fear of deportation" or "fear of referral to immigration authorities" is not included specifically in the list of 21 different possible responses, there are other possible responses that plausibly could include or represent such concerns. The two specific responses most likely to indicate a fear of deportation are "Police would be biased, harass/insult respondent, or cause respondent trouble" and "Was advised not to report to police". Together, only about 1 percent of immigrant victims gave these two responses as the reason they did not report the crime. This is also the case for non-citizen Hispanics, shown on the right side of Table A4. Fear of immigration enforcement does not seem to be much of a factor when immigrants do not report crimes.

A significant number of immigrant respondents answered "other" in several of the categories of crime. While it is possible that this could indicate fear of deportation, in fact this response is very difficult to interpret and could be unrelated to a fear of deportation.[36] First, there is little consistency in the share of immigrants who gave this answer from year to year. Moreover, less than 5 percent of all non-citizen Hispanic victims offered this response for all crimes.[37] Finally, academic research on reasons that immigrants may not feel comfortable reporting crimes suggests a number of different factors that also could be represented in the "other" category. These include language barriers, not knowing how to report the crime, fear of authorities based on experiences in their home country, and lack of awareness that a crime had occurred.[38]

Analysis of Crime Reporting by Region and Community Size

This analysis so far has shown that crimes committed against the foreign-born were not less likely to be reported to the police than crimes against the U.S.-born during the 2017-2019 time period, which is generally considered to have been a time of relatively robust immigration enforcement, assisted by cooperation with local law enforcement agencies. It does not directly answer the related and much-debated question of whether local policies and practices of police engagement with federal immigration enforcement suppress crime reporting by immigrants in those communities, a claim that is routinely asserted by advocates for sanctuary policies. Because the NCVS includes basic information on victim responses according to broad geographic region and community size, it is possible to do a rudimentary examination of this question. We can compare crime reporting rates between large regions of the country that are generally more likely to have law enforcement agencies that routinely engage with ICE and those that have more sanctuary jurisdictions. In addition, we can compare reporting rates in larger and smaller U.S. communities.

Variation in Crime Reporting Across Regions Is Generally Not Significant

While most state and local law enforcement agencies routinely cooperate and engage with federal immigration authorities, there are dozens of jurisdictions around the country that have adopted laws, ordinances, or other types of sanctuary policies that limit such cooperation in a variety of ways. A few jurisdictions, such as San Francisco and New York City, have had sanctuary policies for many years (at least since the 1990s), although the majority of the roughly 300 non-cooperation policies were established more recently, after a new local-federal criminal information-sharing program known as Secure Communities was fully implemented in 2012.

In response to the growing movement to adopt sanctuary policies, several states, including Texas, Florida, Tennessee, and Indiana, enacted laws to require all law enforcement agencies in the state to cooperate with ICE. In addition, 146 law enforcement agencies around the country have joined ICE's 287(g) partnership program, established by Congress to enable agencies to voluntarily participate in immigration enforcement.[39]

The Center for Immigration Studies monitors the adoption and reversal of sanctuary, anti-sanctuary, and other engagement policies nationwide, enabling us to identify the regions of the country where these policies might affect crime reporting by immigrants.[40]

Unfortunately, the level of specificity of geographic locations of NCVS respondents is not as detailed as the information available on sanctuary and ICE engagement policies. The NCVS does not provide details on the geographic locations of the respondents beyond the same four broad geographic divisions used by the Census Bureau: Northeast, Midwest, South, and West. Individual states or cities are not identified in the survey. Even if they were, there would not be enough immigrant respondents in the sample to allow for reliable analysis of crime reporting rates in most states, and certainly not for localities, even specific large cities.

While there are jurisdictions with sanctuary policies in all four broad regions of the country, the greatest concentrations of these are in the West and the Northeast. Of 11 states with sanctuary laws, five are in the West (California, Colorado, Oregon, Washington, and New Mexico), five are in the Northeast (Massachusetts, New Jersey, New York, Vermont, and Connecticut), and one is in the Midwest (Illinois). None are in the South, which is the region with the most state anti-sanctuary laws and 287(g) jurisdictions. The Midwest has one very large sanctuary jurisdiction (the state of Illinois and especially Chicago and Cook County within it), but also includes large non-sanctuary states such as Ohio, Indiana, and Missouri, and states with a patchwork of policies, like Wisconsin and Michigan.

Figure 5 reveals some slight regional differences in crime reporting by immigrant victims, non-citizens, and the U.S.-born. Yet even with the larger sample size available for victims of all crimes, only one of the regional differences is statistically significant. The share of immigrant victimizations for all crimes reported to police in the South (44 percent) is significantly higher than the share reported in the West (36 percent).

The division of the NCVS sample by the four geographic regions necessarily reduces the size of each sample. As shown in Table A5 in the Appendix, the largest geographic samples are for the West and the South, while the sample for the Northeast is about one-third the size of the three other regions. This makes it unworkable to further break down the data by types of crimes or for Hispanic non-citizens as a proxy for illegal immigrants. For this reason, the analysis in Figure 5 (and Table A5) showing crime reporting by region is limited to serious crimes and all crimes and only for U.S.-born, all immigrants, and all non-citizens. (Table A5 reports sample sizes.)

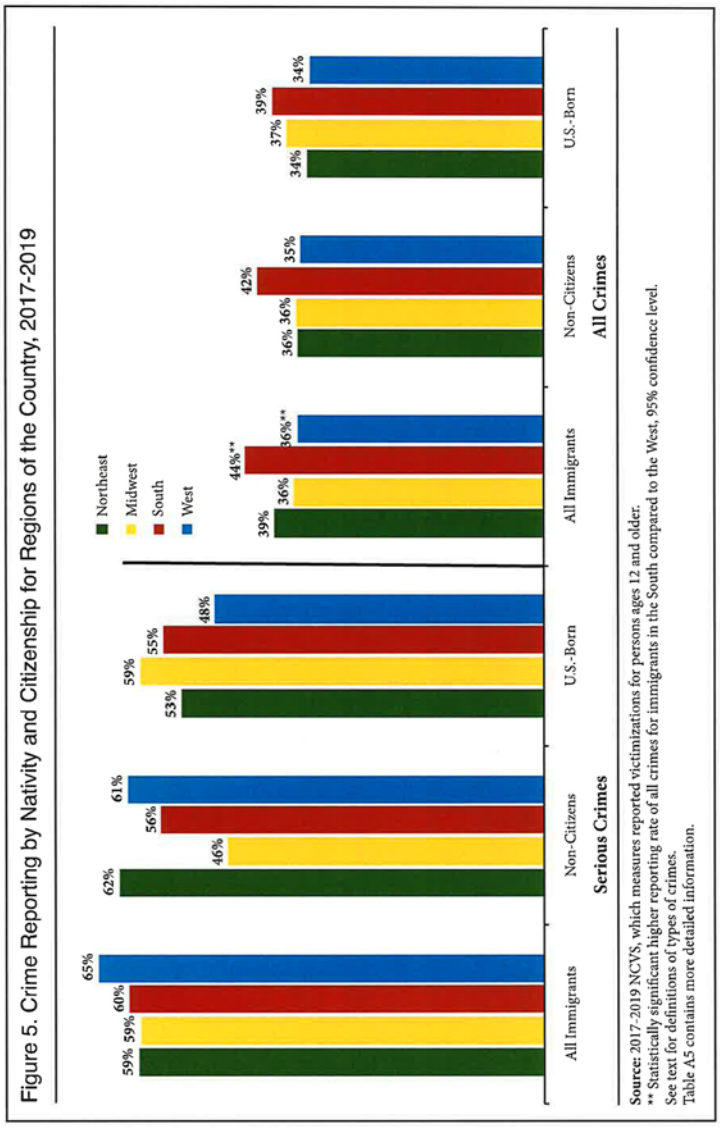
These results call into question the theory that routine engagement and cooperation between local law enforcement agencies and ICE creates distrust and suppresses crime reporting by immigrants. If the sanctuary policies that are so common in the West actually did help boost immigrant crime reporting, then the West should have higher reporting rates than the South, where cooperation is more common — but the NCVS data indicates the opposite.

Variation in Crime Reporting By Community Size Is Not Significant

The "chilling effect" theory also can be tested by examining another geographic variable included in the NCVS — the size of the community in which respondents live. The NCVS breaks down the responses according to whether the respondent lives in a place with a population greater than or less than 100,000. In general, sanctuary policies are more likely to be adopted in larger jurisdictions than in the suburbs and rural areas. Out of 181 localities with sanctuary policies documented by the Center, 112 are large communities (greater than 100,000) and 69 are in small communities (fewer than 100,000 people). And many of the nation's largest cities, including New York, Los Angeles, Chicago, and Philadelphia have sanctuary policies. Therefore, comparing reporting rates in larger communities with smaller communities might provide some insight into whether engagement with federal immigration enforcement affects immigrant crime reporting.

Figure 6 shows the share of serious, violent, and all crimes against the native- and foreign-born reported to police in cities of 100,000 or more compared with communities with fewer than 100,000 residents.[41] As it turns out, the data show that there are no statistically significant differences in crime reporting rates for immigrants and non-citizens according to community size. Based on a simple comparison by community size, crimes against immigrants are not reported at higher rates in larger communities even though, in general, these areas are more likely to have sanctuary policies.

Table A6 in the appendix provides additional information, including sample sizes and reporting rates by community size. There are some significant differences between natives and immigrants for some types of crimes living in similarly sized communities. In every category, it is the immigrants who have the higher reporting rates. Consistently, there are more statistically significant differences between natives and immigrants in smaller communities. We believe that the most meaningful



Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
** Statistically significant higher reporting rate of all crimes for immigrants in the South compared to the West, 95% confidence level.
See text for definitions of types of crimes.
Table A5 contains more detailed information.

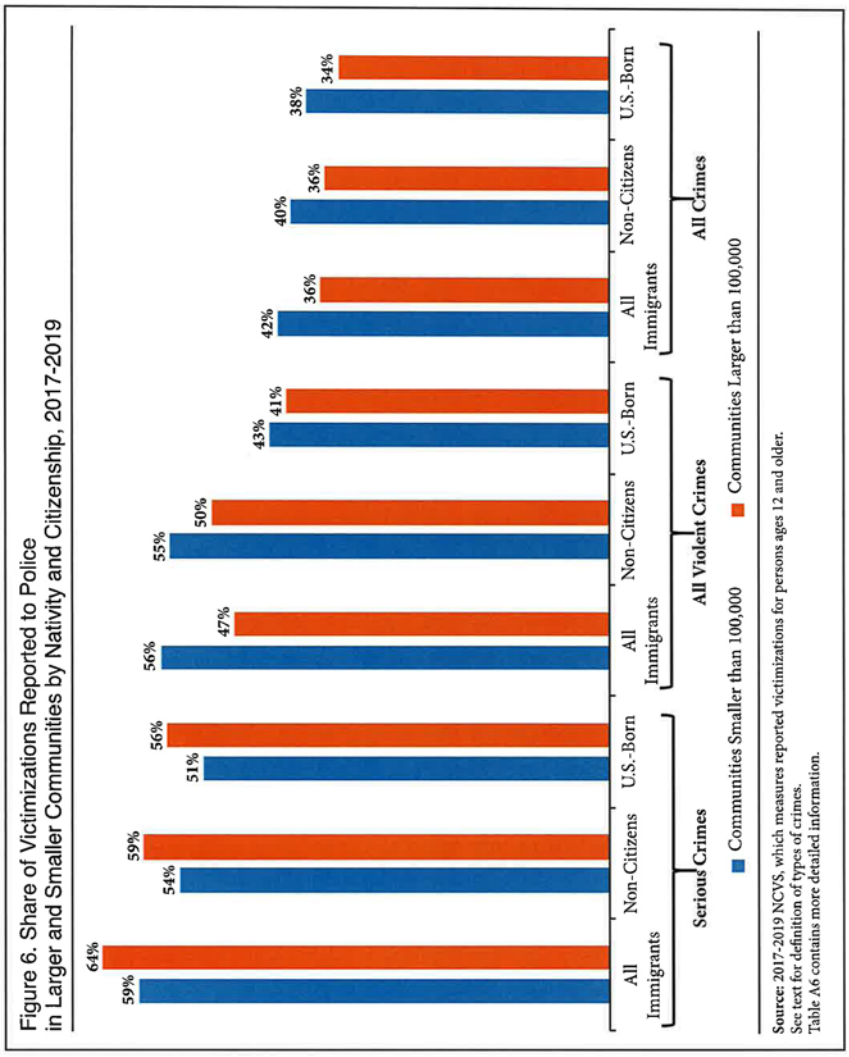


Figure 6. Share of Victimizations Reported to Police
in Larger and Smaller Communities by Nativity and Citizenship, 2017-2019
64%
59%
59%
54%
56%
51%
56%
47%
55%
50%
43%
41%
42%
36%
40%
36%
38%
34%
All Immigrants
Non-Citizens
U.S.-Born
All Immigrants
Non-Citizens
U.S.-Born
All Immigrants
Non-Citizens
U.S.-Born
Serious Crimes
All Violent Crimes
All Crimes
Communities Smaller than 100,000
Communities Larger than 100,000
Source: 2017-2019 NCVS, which measures reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
Table A6 contains more detailed information.

comparison is between immigrants in larger and smaller communities, as shown in Figure 6, but the data showing that immigrants report crimes at relatively high rates in small communities also supports the idea that immigrants report crimes at relatively high rates, even where local police tend to cooperate more with ICE.

Conclusion

Advocates for immigrants have long claimed that state and local law enforcement agencies should be prohibited from cooperating with federal immigration authorities because immigrant victims fear that contact with local police could result in their deportation, leading them to avoid reporting crimes. Data from the NCVS, the most authoritative national source of statistics on crime reporting, shows that crimes against immigrants (naturalized citizens and non-citizens) are reported to police at rates that match or exceed those of the U.S.-born. We find that this is consistently true for various types of crimes, and it is true for female victims, for Hispanic non-citizens (a group that includes many illegal aliens), for younger and presumably more recently arrived immigrants, across most geographic regions, and in both small and large communities. This is the case even though law enforcement in most jurisdictions routinely cooperates with federal immigration enforcement authorities. Our analysis also shows that in the overwhelming majority of victimizations, the immigrants themselves reported the crime to police. Of immigrant victims who did not report a crime, very few gave reasons stating or suggesting that they feared police or immigration enforcement.

Policy Recommendations

1. While it is encouraging that immigrants apparently report victimizations to police at the same or higher rates as the native-born, federal immigration authorities and state and local law enforcement agencies should continue to emphasize to each other and to the public that victims and witnesses of crime are not targets of immigration enforcement when they report them. Continuous reinforcement of this message will help ensure that immigrants and their friends and families continue to feel comfortable reporting crimes.

2. Local law enforcement agencies should not prohibit or discourage officers from cooperating with ICE based on the concern that immigrants are disproportionately less likely to report crimes.

3. Local law enforcement agencies should continue to encourage immigrants to report crimes. There are many successful models for this, some of which have been supported through grants from the Department of Justice's Community Oriented Policing programs. The NCVS data on reasons immigrants and natives may not report crimes suggest some appropriate initiatives, including:

    - Conduct outreach to immigrant communities to help immigrants understand why it is important to report crimes and how they can do so, including appearances in media for non-English speakers;
    - Cultivate a relationship with foreign consuls to enlist their help in explaining our laws and justice system to their citizens;
    - Use existing programs like faith community jail ministry programs to forge connections with leaders in immigrant communities;
    - Educate immigrants on how to prevent victimization;
    - Reduce language barriers by hiring staff who speak the languages of the community;
    - Consider hiring non-uniform personnel to staff sub-stations, take crime reports, and interview certain victims;
    - Offer anonymous tip lines for reporting crimes; and
    - Conduct outreach to help the entire community understand the law enforcement priorities of the agency.

4. Local law enforcement agencies should make judicious use of the visa programs available to help illegal immigrants who are victims of serious crimes and who are assisting in the prosecution of the offender.[42] The availability of these visas can be helpful in gaining the cooperation of illegal immigrants in prosecuting crimes, but the U visa program in particular has become bogged down with rampant fraud and misuse to the point where cooperating victims of serious crime cannot access the benefits promptly.[43] To improve the U visa program, Congress should narrow the eligibility criteria to cover victims of recent, serious crimes, among other reforms. In the meantime, state and local agencies should ensure certifying officers are properly applying the eligibility criteria to reduce abuse.

5. ICE should continue to provide services to victims of crime, including information for victims on the detention status and outcome of immigration proceedings of perpetrators. In particular, Congress should earmark funding for ICE's victim services office.[44]

Appendix

Table A1. Reporting Rates and Confidence Intervals for Crime Victims by Nativity and Citizenship, 2017-2019

Nativity and Type of Crime	Share of Victims Reporting Crime	95% C.I.	Sample Size	Average Annual Number of Crimes (Reported & Unreported)
Pct. Reporting All Serious Crime				
U.S.-Born	52.6%	1.71	6,249	3,902,239
All Immigrants (Citizen & Non-Citizen)**	61.6%	4.76	579	410,080
Hispanic Immigrants (Citizen & Non-Citizen)**	60.5%	6.34	325	227,947
All Non-Citizens	56.8%	6.33	319	235,134
Hispanic Non-Citizens	57.2%	7.84	209	150,926
Pct. Reporting Serious Violent Crime				
U.S.-Born	49.4%	3.84	2,178	1,777,511
All Immigrants (Citizen & Non-Citizen)**	61.3%	8.87	191	176,537
Hispanic Immigrants (Citizen & Non-Citizen)**	64.7%	10.76	117	105,696
All Non-Citizens*	59.1%	10.45	122	120,038
Hispanic Non-Citizens*	61.8%	12.49	84	76,657
Pct. Reporting Violent (Excl. Simple Assault) Crime				
U.S.-Born	48.2%	3.98	2,341	1,928,114
All Immigrants (Citizen & Non-Citizen)**	61.0%	9.40	198	181,604
Hispanic Immigrants (Citizen & Non-Citizen)**	64.1%	11.50	119	106,743
All Non-Citizens*	59.1%	11.02	127	124,510
Hispanic Non-Citizens*	61.2%	13.35	85	77,373
Pct. Reporting All Violent Crimes				
U.S.-Born	42.0%	2.78	6,291	5,434,371
All Immigrants (Citizen & Non-Citizen)**	51.9%	6.63	460	462,903
Hispanic Immigrants (Citizen & Non-Citizen)**	58.3%	8.58	243	233,345
All Non-Citizens**	52.5%	8.52	252	241,453
Hispanic Non-Citizens**	56.7%	10.52	151	141,474
Pct. Reporting All Crimes				
U.S.-Born	36.6%	1.02	29,203	17,287,544
All Immigrants (Citizen & Non-Citizen)	38.8%	2.74	2,826	1,862,109
Hispanic Immigrants (Citizen & Non-Citizen)	38.0%	3.75	1,432	947,752
All Non-Citizens	37.6%	3.81	1,411	909,802
Hispanic Non-Citizens	37.0%	4.88	844	536,872

Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.

Table A2. Reporting Rates and Confidence Intervals for Female Crime Victims by Nativity and Citizenship, 2017-2019

Nativity and Type of Crime	Share of Victims Reporting Crime	95% C.I.	Sample Size	Average Annual Number of Crimes (Reported & Unreported)
Pct. Reporting All Serious Crime				
U.S.-Born	51.0%	2.3	3,362	2,084,216
All Immigrants (Citizen & Non-Citizen)**	62.6%	6.9	294	185,749
Hispanic Immigrants (Citizen & Non-Citizen)*	58.9%	9.1	167	108,676
All Non-Citizens	56.9%	9.7	149	97,846
Hispanic Non-Citizens	54.7%	11.4	106	70,973
Pct. Reporting Serious Violent Crime				
U.S.-Born	47.8%	4.8	1,192	936,941
All Immigrants (Citizen & Non-Citizen)**	64.8%	13.0	86	66,954
Hispanic Immigrants (Citizen & Non-Citizen)**	64.7%	15.0	60	48,296
All Non-Citizens**	66.8%	15.8	52	41,620
Hispanic Non-Citizens*	63.7%	17.8	41	33,072
Pct. Reporting Violent (Excl. Simple Assault) Crime				
U.S.-Born	46.3%	4.8	1,330	1,065,496
All Immigrants (Citizen & Non-Citizen)**	65.2%	13.6	91	70,517
Hispanic Immigrants (Citizen & Non-Citizen)**	63.8%	16.0	61	49,012
All Non-Citizens**	67.7%	16.3	56	44,917
Hispanic Non-Citizens	62.3%	19.0	42	33,788
Pct. Reporting All Violent Crimes				
U.S.-Born	43.7%	3.4	3,383	2,849,426
All Immigrants (Citizen & Non-Citizen)**	56.0%	8.7	230	226,508
Hispanic Immigrants (Citizen & Non-Citizen)**	61.8%	11.0	136	120,991
All Non-Citizens**	64.8%	11.9	120	98,431
Hispanic Non-Citizens**	64.4%	13.6	83	71,394
Pct. Reporting All Crimes				
U.S.-Born	37.1%	1.3	15,773	9,286,452
All Immigrants (Citizen & Non-Citizen)	38.5%	3.8	1,472	950,575
Hispanic Immigrants (Citizen & Non-Citizen)	37.3%	5.1	771	494,524
All Non-Citizens	37.7%	5.5	691	422,601
Hispanic Non-Citizens	37.5%	6.7	454	276,658

Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.

Table A3. Reporting Rates and Confidence Intervals for Victims of Property Crimes by Nativity and Citizenship, 2017-2019

Nativity and Type of Crime	Share of Victims Who Reported Crime to Police	95% C.I.	Sample Size	Average Annual Number of Crimes (Reported & Unreported)
Pct. Reporting Serious Property Crime				
U.S.-Born	55.2%	2.66	4,071	2,124,727
All Immigrants (Citizen & Non-Citizen)*	61.8%	7.37	388	233,543
Hispanic Immigrants (Citizen & Non-Citizen)	56.8%	10.28	208	122,251
All Non-Citizens	54.3%	10.65	197	115,096
Hispanic Non-Citizens	52.5%	13.22	125	74,270
Pct. Reporting All Property Crime				
U.S.-Born	34.1%	1.17	22,766	11,749,453
All Immigrants (Citizen & Non-Citizen)	34.4%	3.06	2,352	1,386,769
Hispanic Immigrants (Citizen & Non-Citizen)	31.4%	4.11	1,181	705,676
All Non-Citizens	32.2%	4.26	1,155	665,251
Hispanic Non-Citizens	29.8%	5.36	689	392,299

Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.

Table A4. Reasons Crimes Were Not Reported, 2017-2019

	All Serious Crime		Serious Violent		All Violent		Serious Property		All Property		All Crime				
Most Important Reason Not Reported	**Imm.**	**U.S.-Born**	**Imm.**	**U.S.-Born**	**Imm.**	**U.S.-Born**	**Imm.**	**U.S.-Born**	**Imm.**	**U.S.-Born**	**Imm.**	**Non-Cit.**	**Hisp. Imm.**	**Hisp. Non-Cit.**	**U.S.-Born**
Police wouldn't think it was important enough, wouldn't want to be bothered or get involved	17.9%	9.7%	16.9%	8.1%	10.6%	7.6%	18.7%	11.3%	15.1%	14.4%	14.3%	15.4%	17.4%	19.4%	12.4%
Minor or unsuccessful crime, small or no loss, recovered property	13.3%	14.5%	10.3%	8.5%	13.5%	12.0%	15.6%	20.0%	27.9%	29.3%	24.9%	23.1%	23.1%	22.0%	24.5%
Other	13.1%	7.3%	21.7%	10.7%	10.8%	7.5%	6.4%	4.1%	3.5%	3.2%	5.0%	6.0%	4.0%	4.5%	4.5%
Afraid of reprisal by offenders or others	7.4%	5.1%	14.6%	8.2%	9.9%	6.5%	1.7%	2.3%	1.2%	1.0%	2.9%	3.5%	3.0%	2.9%	2.6%
Police would be inefficient, ineffective (they'd arrive late or not at all, wouldn't do a good job, etc.)	6.6%	7.1%	3.9%	7.0%	5.0%	3.8%	8.7%	7.3%	4.2%	4.6%	4.4%	5.3%	5.6%	7.0%	4.3%
Private or personal matter or took care of it myself or informally; told offender's parents	6.4%	13.2%	8.7%	17.5%	8.3%	17.9%	4.6%	9.1%	4.6%	6.7%	5.3%	4.2%	4.7%	3.0%	9.9%
Could not find or identify offender, lack of proof	5.2%	7.0%	0.7%	3.1%	3.5%	2.1%	8.7%	10.7%	7.7%	8.7%	6.8%	7.4%	6.8%	7.0%	6.9%
Did not want to get offender in trouble with the law	5.1%	5.9%	7.8%	7.5%	5.6%	5.4%	2.9%	4.4%	1.3%	2.5%	2.1%	2.6%	2.5%	2.9%	3.3%
Reported to another official (guard, apartment manager, school official, etc.)	4.9%	7.3%	4.3%	10.9%	18.6%	20.9%	5.4%	3.9%	9.5%	7.9%	11.2%	9.8%	8.9%	7.5%	11.6%
Respondent not present or doesn't know why it wasn't reported	4.1%	0.9%	0.9%	0.3%	4.6%	0.3%	6.6%	1.4%	2.2%	0.8%	2.7%	1.6%	1.6%	1.6%	0.6%
Did not want to or could not take time - too inconvenient	3.9%	5.6%	1.2%	6.6%	2.7%	3.9%	6.1%	4.6%	5.6%	4.3%	5.0%	6.3%	7.0%	8.1%	4.2%
Didn't find out until too late	3.9%	3.5%	-	0.3%	-	0.1%	6.9%	6.6%	6.0%	4.7%	4.9%	4.6%	5.8%	5.0%	3.4%
Unknown	2.5%	3.1%	4.5%	3.7%	3.0%	4.2%	1.0%	2.6%	3.6%	3.2%	3.4%	3.8%	2.8%	2.8%	3.5%
Could not recover or identify property	1.4%	1.9%	-	0.1%	-	0.1%	2.6%	3.6%	3.8%	3.9%	3.0%	3.2%	2.8%	2.9%	2.8%
Not clear was a crime or that harm was intended	1.2%	3.0%	1.1%	3.4%	2.0%	3.8%	1.2%	2.6%	1.5%	1.5%	1.6%	1.4%	1.2%	1.1%	2.2%
Child offender(s),"kid stuff"	1.1%	0.9%	2.4%	1.2%	0.7%	1.7%	0.1%	0.6%	0.6%	0.8%	0.6%	0.3%	0.5%	0.4%	1.1%
No insurance, loss less than deductible, etc.	1.0%	0.9%	-	-	-	-	1.7%	1.8%	1.2%	1.3%	0.9%	1.2%	1.4%	1.5%	1.0%
Police would be biased, would harass/insult respondent, cause respondent trouble, etc.*	0.6%	2.8%	-	2.4%	0.7%	1.4%	1.0%	3.2%	-0.5%	0.9%	0.5%	0.1%	0.3%	0.2%	1.1%
Was advised not to report to police*	0.5%	0.3%	1.1%	0.6%	0.4%	0.8%	-	0.1%	0.1%	0.1%	0.3%	0.2%	0.5%	0.4%	0.3%
Offender was police officer	-	-	-	-	-	0.0%	-	-	-	0.0%	0.0%	-	-	-	0.0%
No one reason was more important	-	-	-	-	-	-	-	-	-	0.0%	0.0%	-	-	-	0.0%
Sample Size	215	2,824	70	1,019	208	3,391	145	1,805	1,533	14,898	1,749	886	909	543	18,377

Source: 2017-2019 NCVS, which measures reported victimizations for person ages 12 and older.
* This response may be an indication of fear that police would report victim to immigration agencies.
The reasons are sorted from the most to least common answer given by immigrants for not reporting a serious crime. The top reasons given by immigrants for each category of crime are highlighted in yellow and the top reasons given by the native-born are highlighted in green.

Table A5. Reporting Rates and Confidence Intervals for Crime Victims by Region, Nativity, and Citizenship, 2017-2019

	All Crime			Serious Crime		
Region, Nativity, Citizenship	Share of Victims Who Reported Crime to Police	95% C.I.	Sample Size	Share of Victims Who Reported Crime to Police	95% C.I.	Sample Size
Northeast						
U.S.-Born	34.3%	2.6	2,551	52.7%	4.9	472
All Immigrants (Citizen & Non-Citizen)	39.2%	6.3	411	58.9%	12.2	80
All Non-Citizens	35.8%	10.0	158	61.8%	19.3	30
Midwest						
U.S.-Born	37.4%	2.0	8,268	54.2%	3.3	1,908
All Immigrants (Citizen & Non-Citizen)	36.4%	8.4	393	58.8%	15.0	82
All Non-Citizens	35.9%	11.0	225	46.0%	19.0	53
South						
U.S.-Born	39.3%	1.6	9,085	55.4%	2.7	2,075
All Immigrants (Citizen & Non-Citizen)	43.5%[1]	4.9	797	60.3%	7.5	191
All Non-Citizens	41.6%	6.4	440	55.7%	9.1	125
West						
U.S.-Born	34.0%	1.6	9,299	47.9%	3.0	1,794
All Immigrants (Citizen & Non-Citizen)	35.7%[1]	4.0	1,225	64.8%[2]	7.6	226
All Non-Citizens	35.3%	5.8	588	60.5%[2]	10.9	111

Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
[1] Statistically significant higher rate in the South compared to West at 95% confidence level.
[2] For serious crime only statistically significant difference with U.S.-born reporting at 95% confidence level.

Table A6. Share of Victimizations Reported by Community Size, Type of Crime, & Nativity, 2017-2019

Places with Fewer than 100,000 Residents

Type of Crime and Nativity	Share of Victims Who Reported Crime to Police	95% C.I.	Sample Size
Pct. Reporting All Serious Crime			
U.S.-Born	51.0	2.1	4,271
All Immigrants (Citizen & Non-Citizen)**	59.2	6.9	273
All Non-Citizens	54.1	9.6	137
Pct. Reporting All Serious Violent Crime			
U.S.-Born	48.8	4.4	1,435
All Immigrants (Citizen & Non-Citizen)**	62.4	12.1	84
All Non-Citizens	55.3	15.2	48
Pct. Reporting Serious Property Crime			
U.S.-Born	52.9	3.2	2,836
All Immigrants (Citizen & Non-Citizen)	56.9	10.7	189
All Non-Citizens	52.9	16.1	89
Pct. Reporting Violent (Excl. Assault)Crime			
U.S.-Born	47.9	4.6	1,519
All Immigrants (Citizen & Non-Citizen)*	61.0	12.9	86
All Non-Citizens	53.3	16.0	50
Pct. Reporting All Violent Crime			
U.S.-Born	42.7	3.2	4,245
All Immigrants (Citizen & Non-Citizen)**	56.4	8.6	223
All Non-Citizens	55.3	11.6	113
Pct. Reporting All Crime			
U.S.-Born	38.1	1.3	18,917
All Immigrants (Citizen & Non-Citizen)	41.6	4.0	1,328
All Non-Citizens	40.2	5.8	625

Places with 100,000 or More Residents

Type of Crime and Nativity	Share of Victims Who Reported Crime to Police	95% C.I.	Sample Size
Pct. Reporting All Serious Crime			
U.S.-Born	55.6	2.8	1,978
All Immigrants (Citizen & Non-Citizen)*	63.8	6.4	306
All Non-Citizens	58.7	8.2	182
Pct. Reporting All Serious Violent Crime			
U.S.-Born	50.6	5.6	743
All Immigrants (Citizen & Non-Citizen)	60.4	11.5	107
All Non-Citizens	61.9	13.0	74
Pct. Reporting Serious Property Crime			
U.S.-Born	60.1	4.4	1,235
All Immigrants (Citizen & Non-Citizen)	66.4	9.9	199
All Non-Citizens	55.3	14.0	108
Pct. Reporting Violent (Excl. Assault)Crime			
U.S.-Born	48.8	5.7	822
All Immigrants (Citizen & Non-Citizen)	61.1	12.1	112
All Non-Citizens	63.3	13.6	77
Pct. Reporting All Violent Crime			
U.S.-Born	40.7	3.9	2,046
All Immigrants (Citizen & Non-Citizen)	47.1	8.7	237
All Non-Citizens	50.0	10.9	139
Pct. Reporting All Crime			
U.S.-Born	34.1	1.5	10,286
All Immigrants (Citizen & Non-Citizen)	36.4	3.6	1,498
All Non-Citizens	35.8	4.9	786

Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.

Table A7. Crime Victimization per 1,000 U.S. Residents by Nativity and Citizenship, 2017-2019 NCVS

Type of Crime and Nativity	Victimizations per 1,000 Residents	95% C.I.	Sample Size
Victimization Rate All Serious Crime			
U.S.-Born	17	0.6	640,884
All Immigrants (Citizen & Non-Citizen)**	11	1.1	86,291
Hispanic Immigrants (Citizen & Non-Citizen)**	13	1.8	36,521
All Non-Citizens	15	2.0	34,627
Hispanic Non-Citizens	18	2.9	18,474
Victimization Rate Serious Violent Crime			
U.S.-Born	8	0.8	640,884
All Immigrants (Citizen & Non-Citizen)**	5	1.0	86,291
Hispanic Immigrants (Citizen & Non-Citizen)	6	1.6	36,521
All Non-Citizens	8	1.9	34,627
Hispanic Non-Citizens	9	2.7	18,474
Victimization Rate Violent Crime (Excl. Simple Assault)			
U.S.-Born	8	0.9	640,884
All Immigrants (Citizen & Non-Citizen)**	5	1.1	86,291
Hispanic Immigrants (Citizen & Non-Citizen)*	6	1.8	36,521
All Non-Citizens	8	2.1	34,627
Hispanic Non-Citizens	9	2.9	18,474
Victimization Rate All Violent Crime			
U.S.-Born	23	1.8	640,884
All Immigrants (Citizen & Non-Citizen)**	12	2.0	86,291
Hispanic Immigrants (Citizen & Non-Citizen)**	14	2.9	36,521
All Non-Citizens**	15	3.2	34,627
Hispanic Non-Citizens**	17	4.2	18,474
Victimization Rate Serious Property Crime			
U.S.-Born	9	0.5	640,884
All Immigrants (Citizen & Non-Citizen)**	6	1.0	86,291
Hispanic Immigrants (Citizen & Non-Citizen)**	7	1.5	36,521
All Non-Citizens**	7	1.6	34,627
Hispanic Non-Citizens	9	2.4	18,474
Victimization Rate All Property Crime			
U.S.-Born	50	1.4	640,884
All Immigrants (Citizen & Non-Citizen)**	36	2.5	86,291
Hispanic Immigrants (Citizen & Non-Citizen)**	42	3.8	36,521
All Non-Citizens**	43	4.0	34,627
Hispanic Non-Citizens	47	5.6	18,474
Victimization Rate All Crime			
U.S.-Born	74	1.8	640,884
All Immigrants (Citizen & Non-Citizen)**	49	2.9	86,291
Hispanic Immigrants (Citizen & Non-Citizen)**	56	4.5	36,521
All Non-Citizens**	58	4.7	34,627
Hispanic Non-Citizens**	64	6.5	18,474

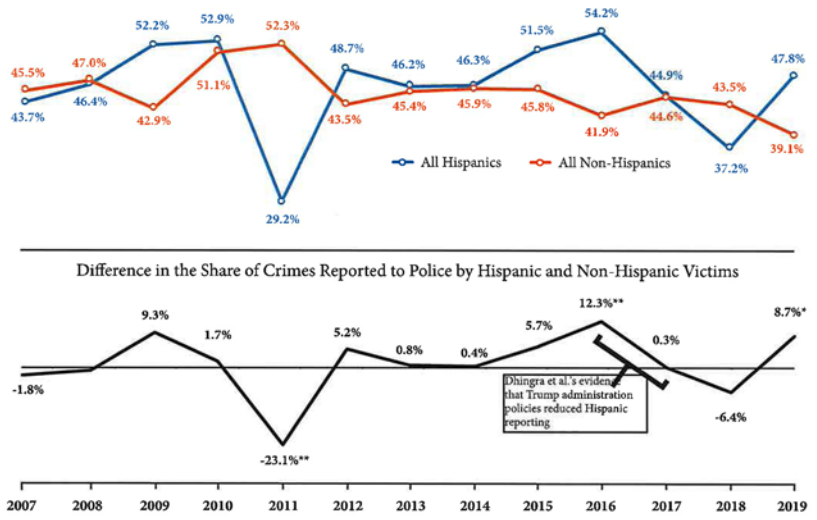
Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.

Table A8. Share of Victimizations Reported to Police by Nativity and Citizenship for Individual Years of the NCVS: 2017, 2018, and 2019

	2017		2018		2019	
Type of Crime and Nativity	Percent Reporting	Sample Size	Percent Reporting	Sample Size	Percent Reporting	Sample Size
All Serious Crime						
U.S.-Born	54.1	2,102	52.7	2,268	50.9	1,879
All Immigrants (Citizen & Non-Citizen)	58.6	210	62.3	186	64.4	183
Hispanic Immigrants (Citizen & Non-Citizen)	60.9	114	57.8	105	62.5	106
All Non-Citizens	48.4	122	64.4	100	61.1	97
Hispanic Non-Citizens	55.0	76	54.8	61	61.8	72
Serious Violent Crime						
U.S.-Born	50.2	708	51.2	775	46.6	695
All Immigrants (Citizen & Non-Citizen)	55.6	58	69.5	63	60.0	70
Hispanic Immigrants (Citizen & Non-Citizen)	64.9	32	72.2	35	59.1	50
All Non-Citizens	47.4	37	75.6	38	60.4	47
Hispanic Non-Citizens	58.1	25	73.3	22	58.7	37
Violent Crime (Excl. Simple Assault)						
U.S.-Born	51.2	757	48.5	854	45.0	730
All Immigrants (Citizen & Non-Citizen)	55.2	61	68.9	67	60.0	70
Hispanic Immigrants (Citizen & Non-Citizen)	64.9	32	69.8	37	59.1	50
All Non-Citizens	47.6	39	75.0	41	60.4	47
Hispanic Non-Citizens	58.1	25	70.4	23	58.7	37
All Violent Crime						
U.S.-Born	44.5	2,071	42.4	2,205	39.2	2,015
All Immigrants (Citizen & Non-Citizen)	48.9	152	47.8	149	58.6	159
Hispanic Immigrants (Citizen & Non-Citizen)	62.6	78	48.6	74	61.8	91
All Non-Citizens	43.7	80	54.9	80	59.3	92
Hispanic Non-Citizens	54.6	45	54.2	42	59.4	64
Serious Property Crime						
U.S.-Born	57.0	1,394	53.9	1,493	54.8	1,184
All Immigrants (Citizen & Non-Citizen)	60.7	152	57.0	123	68.1	113
Hispanic Immigrants (Citizen & Non-Citizen)	58.0	82	46.9	70	66.7	56
All Non-Citizens	49.4	85	54.2	62	61.9	50
Hispanic Non-Citizens	52.0	51	40.6	39	66.1	35
All Property Crime						
U.S.-Born	35.6	7,731	34.2	7,754	32.4	7,281
All Immigrants (Citizen & Non-Citizen)	36.3	843	33.8	761	32.9	748
Hispanic Immigrants (Citizen & Non-Citizen)	34.6	437	28.5	401	30.5	343
All Non-Citizens	32.8	423	33.1	377	30.9	355
Hispanic Non-Citizens	29.9	264	28.2	232	31.5	193
All Crime						
U.S.-Born	38.3	9,851	36.9	10,015	34.6	9,337
All Immigrants (Citizen & Non-Citizen)	39.4	1,002	37.5	914	39.3	910
Hispanic Immigrants (Citizen & Non-Citizen)	40.9	519	32.8	477	39.8	436
All Non-Citizens	35.8	505	38.3	458	39.0	448
Hispanic Non-Citizens	35.8	311	33.4	275	41.5	258

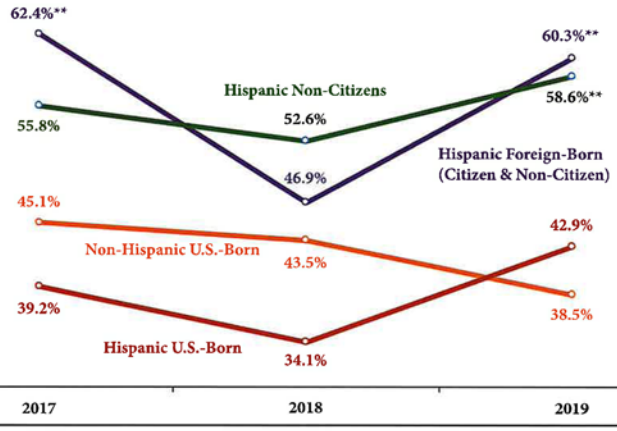
Source: Combined 2017, 2018, 2019 National Crime Victimization Surveys, which measure reported victimizations for persons ages 12 and older.
See text for definition of types of crimes.

Figure A1. The share of crimes against Hispanics & Non-Hispanics reported to police has fluctuated over the years in the NCVS data (2007-2019).



Source: All personal crime reporting from the NCVS. Household crimes are excluded to match Dhingra et al. 2021.
Hispanics include both immigrants and the U.S.-born.
* Statistically significant difference at 90% confidence level with U.S.-born for the same type of crime.
** Statistically significant difference at 95% confidence level with U.S.-born for the same type of crime.

Figure A2. In 2017, 2018, and 2019, crimes against foreign-born Hispanics and even non-citizen Hispanics were statistically as likely, and in some years more likely, to be reported than crimes against U.S.-born non-Hispanics.



Source: All personal crime reporting from the NCVS. Household crimes are excluded to match Dhingra et al., 2021.
** Statistically significant difference at 95 percent confidence level with non-Hispanic U.S.-born.

End Notes

[1] For more information, see ICE's topical web pages on "Secure Communities", undated, and "Updated Facts on ICE's 287(g) Program", updated September 17, 2021; and various CIS publications, including "Remarks of Jessica M. Vaughan before the Homeland Security Advisory Council's Task Force on Secure Communities", Prepared for the Homeland Security Advisory Council's Task Force on Secure Communities, August 9, 2011, and Jessica M. Vaughan and James R. Edwards, Jr, "The 287(g) Program: Protecting Home Towns and Homeland", October 26, 2009.

[2] Jamie Eldridge, state senator from Acton, Mass, quoted in 2018 in Samantha J. Gross, "Deal Struck on Immigrant", the Lowell Sun, February 22, 2018.

[3] KC Becker, speaker of the House in Colorado, quoted in Allison Sherry, "Polis: ICE Is 'A Legitimate Agency' And The State Shouldn't Step On Local Cooperation", CPR News, March 26, 2019.

[4] Chris Magnus, "Sessions' Anti-Immigrant Policies Will Make Cities More Dangerous", New York Times, December 6, 2017.

[5] "Are Sanctuary Jurisdictions Good Policy?", Britannica Procon.org, March 15, 2018.

[6] Pedro Figueroa, an illegal alien from El Salvador, was arrested by ICE outside the San Francisco police department in February 2016 attempting to claim his stolen car after it was recovered by police. The police ran a background check on Figueroa before releasing the vehicle and discovered an ICE warrant for his arrest. Figueroa had entered illegally in 2005 and asked for asylum, but failed to appear for his immigration hearing and was ordered deported by an immigration judge. In addition, he had a misdemeanor conviction for drunk driving in 2012. He was held by ICE for two months and eventually an immigration judge allowed him to reopen his asylum case, setting a hearing in November 2019. Figueroa sued the city of San Francisco for violating its sanctuary policy, and was awarded $190,000. Reportedly, he planned to apply for a U visa available for illegal alien crime victims, saying he was the victim of "false imprisonment" by ICE. See Richard Gonzales, "Man Reports Car Stolen, Ends Up In Deportation Limbo", NPR, February 16, 2016.

[7] Aaron Martinez, "Transgender woman gets time served, stays jailed", El Paso Times, April 13, 2017.

[8] See Jessica M. Vaughan, "Top Justice Official Scoffs at Immigrant Crime Reporting 'Chilling Effect'", June 26, 2017, and "Sheriffs Skeptical of "Chilling Effect' from Secure Communities", July 11, 2012.

[9] See, for example, Nik Theodore, "Insecure communities: Latino perceptions of police involvement in immigration enforcement", Department of Urban Planning and Policy: University of Illinois at Chicago, 2013.

[10] See, for example, Robert C. Davis et al., "Access to Justice for Immigrants Who Are Victimized: The Perspectives of Police and Prosecutors", Criminal Justice Policy Review, 12:3, September 2001.

[11] See, for example, Cecilia Menjivar et al., "Immigration enforcement, the racialization of legal status, and perceptions of the police: Latinos in Chicago, Los Angeles, Houston and Phoenix in comparative perspective", Du Bois Review: Social Science Research on Race, 15(1), 2018.

[12] Thomas M. Guterbock et al., "Evaluation Study of Prince William County Police Illegal Immigration Enforcement Policy", January 2010.

[13] Reva Dhingra, et al., "Immigration Policies and Access to the Justice System: The Effect of Enforcement Escalations on Undocumented Immigrants and Their Communities", Springer Science+Business Media, January 2, 2021.

[14] The higher rate for Hispanics was statistically significant in 2016, but not 2017.

[15] The much higher rate for Hispanics relative to natives in 2019 is statistically significant. In itself, this seems to contradict the notion that Trump enforcement efforts reduced Hispanic reporting of crime, as the very same policies that Dhingra et al. refer to as chilling Hispanic crime reporting (Secure Communities, 287(g) and Executive Order 13768 on immigration enforcement priorities) were still in effect in 2019.

[16] Figure A1 shows that, since 2007, the difference between Hispanics and non-Hispanics was only statistically significant in 2011, 2016, and 2019, with Hispanics in 2016 and 2019 being significantly more likely to report crime.

[17] The sample size of Hispanic victims in the NCVS was only 289 in 2016 and 349 in 2017. The margin of error for Hispanics in 2016 using a 95 percent confidence is 54.2% ±8%, and for non-Hispanics 41.9% ±4.4%. The margin of error for Hispanics in the 2017 NCVS is 44.9% ±7.8%, and for non-Hispanics 44.6% ±4.5%.

[18] This interpretation makes sense since, as Figure A1 shows, the difference in reporting rates in 2016 and 2019 for Hispanics was higher than non-Hispanics and the difference was statistically significant, while the differences in 2017 and 2018 were not.

[19] In the Census Bureau's 2018 American Community Survey (ACS), which is often used by researchers to estimate illegal immigration, we find that 46 percent of non-citizen Hispanics ages 12 and older not in institutions are in the country illegally, and that 23 percent are legal immigrants who live in the same household as an illegal alien. It is likely that a similar share of non-citizen Hispanics are illegal immigrants or live with those in the country illegally in the NCVS. For an explanation of how we identify and weight illegal immigrants in the ACS see the methods section in Steven A. Camarota, Karen Zeigler, and Jason Richwine, "How Much Would It Cost to Provide Health Insurance to Illegal Immigrants?", Center for Immigration Studies, October 10, 2019.

[20] The NCVS is a panel survey of individuals 12 and older, in which the same household is interviewed every six months over a 3.5-year period. As the survey measures crime victimization, murder is excluded from the analysis. Participants are asked if they were victims of crime in the prior six months. The public-use data and most publications from the Bureau of Justice Statistics (BJS) using the survey are based on the year the data was collected, so in some cases the crime may have occurred in the prior calendar year.

[21] Serious crime includes most violent crime: rape, attempted rape, sexual assault, completed forced sexual assault without injury, completed robbery, completed robbery without injury, attempted robbery with injury, attempted robbery without injury, completed aggravated assault with injury, attempted aggravated assault with a weapon, and threatened aggravated assault with a weapon. All violent crime includes all serious crimes, plus simple assault, threatened rape or sexual assault, and unwanted sexual contact (not rape) without force. Serious property crime includes completed burglary and completed motor-vehicle theft. Completed burglary includes unlawful or forcible entry, including a permanent residence or other residence or structure, involving a theft or attempted theft. All property crimes include all serious property crimes, plus attempted burglary, residential trespassing, and all other property crimes.

See p. 17 in Rachel E. Morgan and Barbara A. Oudekerk, "Criminal Victimizations, 2018", U.S. Department of Justice Office of Justice Programs Bureau of Justice Statistics, September 2019, NCJ 253043.

[32] Tables A1 and A3 in the Appendix show the total sample sizes for different categories of crime using the 2017 to 2019 NCVS. Table A2 shows the sample size for female victimizations. Table A8 shows the sample size for each individual year for different categories of crime. While Table A8 shows that the immigrant sample is small for many types of crimes in individual years, the basic pattern exists in the individual years as the combined three-year sample.

[23] The number of victimizations reflects how many criminal acts were committed against those in the NCVS. See p. 18 of the 2018 NCVS code book provided by ICPSR. Some crimes, like burglary, are not committed against individuals. In that case, all persons 12 and older in the household are considered victims.

[24] Parameters for the NCVS are found in Table 2 of the NCVS code book provided by ICPSR.

[25] Year-to-Year Correlation Coefficients can be found in Table 15 of the NCVS code book provided by ICPSR.

[26] Like other Census Bureau-administered surveys, the NCVS asks respondents about their citizenship with five possible responses: 1) U.S. citizen, born in the United States; 2) U.S. citizen, born abroad of U.S. citizen parents; 3) U.S. citizen, born in Puerto Rico, Guam, the U.S. Virgin Islands, or the Northern Marianas; 4) U.S. citizen by naturalization; 5) Not a U.S. citizen. Responses 1 through 3 are the natives or the U.S.-born. Responses 4 and 5 are immigrants, or the foreign-born.

[27] The NCVS source and accuracy statement indicates that among those excluded from the data are, "foreign visitors to the United States". However, the NCVS Interviewing Manual for field representatives states that individuals are included in the survey if they are temporarily living (not visiting or traveling) in the United States, the sample address is not an embassy, consulate, chancellery, or any other type of office for a foreign country, and the sample address is their usual place of residence while residing in the United States. This means that like other surveys collected by the Census Bureau, long-term temporary visitors (e.g. guestworkers and foreign students) who live in the United States and are not institutionalized should be captured in the survey. This was confirmed in an email dated December 7, 2020, from Dr. Jennifer L. Truman a statistician at the BJS.

[28] The term "immigrant" has a specific meaning in immigration law, which is all those inspected and admitted as lawful permanent residents of the United States. Here, we use the term "immigrant" in the non-technical sense, to include all foreign-born persons, legal or illegal.

[29] Prior research indicates that most illegal immigrants are included in surveys collected by the Census Bureau, including the American Community Survey (ACS) and the Current Population Survey (CPS). The Center for Migration Studies' latest estimate based on the 2019 ACS data can be found here. This presentation from the Pew Research Center explains how they estimate the illegal population using the ACS. The Department of Homeland Security's newest estimate of the illegal population using the ACS can be found here.

[30] In recent years, DHS, Pew, and CMS have all estimated that there are roughly 9.5 to 10 million illegal immigrants captured by the ACS. Estimates derived from the ACS are then adjusted upward by these researchers to account for undercount in the survey. The number of immigrants age 12 and older who are not in institutions is 11 to 12 percent smaller in the 2017-2019 NCVS than the ACS from 2017-2019. (The NCVS survey includes only the non-institutionalized population ages 12 and older.) The smaller total immigrant population in the NCVS relative to ACS may reflect a larger undercount of immigrants, generally, or of illegal immigrants in particular in the NCVS. But the two surveys have different designs, which might explain the difference. Still, the number of immigrants in the NCVS is not very different from the ACS, indicating that the crime survey must capture some significant share of the illegal population. It is reasonable to expect that the illegal population in the NCVS would be represented in the category of non-citizens.

[31] The federal government estimates that some 70 percent of illegal immigrants come from Latin America. The Center for Migration Studies estimates a similar share.

[32] The lack of significance between values that seem quite different in the NCVS is typically due to the small sample size of the sub-populations of immigrants.

[33] This category is for all violent crime, except for the categories of simple assault with injury, assault without a weapon and without injury, and verbal threat of assault. In contrast to simple assault, aggravated assault often includes a weapon, and typically results in significant injury and/or is related to the intent of the crime or characteristics of the victim.

[34] To determine which respondents are most likely to be illegal aliens in the ACS, we first exclude immigrant respondents who are almost certainly not illegal aliens — for example, spouses of native-born citizens, veterans, government employees, Cubans (because of special rules for that country), etc. The remaining candidates are weighted to replicate known characteristics of the illegal population (population size, age, gender, region or country of origin, state of residence, and length of residence in the United States). We use the 2018 estimates from the Center for Migration Studies (CMS), which are based on the ACS. The resulting illegal population is designed to match CMS on the characteristics listed above. See "State-Level Unauthorized Population and Eligible-to-Naturalize Estimates", Center for Migration Studies. For some additional explanation of how we identify and weight illegal immigrants in the ACS, see the methods section in Steven A. Camarota, Karen

Zeigler, and Jason Richwine, "How Much Would It Cost to Provide Health Insurance to Illegal Immigrants?", Center for Immigration Studies, October 10, 2019.

[35] See, for example, Robert C. Davis and Nicole J. Henderson, "Willingness to Report Crimes: The Role of Ethnic Group Membership and Community Efficacy", in Crime and Delinquency, October 2003; and Cecilia Menjivar and Cynthia L. Bejarano, "Latino immigrants' perceptions of crime and police authorities in the United States: A case study from the Phoenix Metropolitan area", in Ethnic and Racial Studies, January 2004.

[36] The NCVS "Interviewing Manual for Field Representatives Manual" indicates that those administering the survey can ask various probes to see if they can get the respondent to give one of the specific answers to this question. According to the manual, among the reasons a response might be coded as "other" is that the respondent does not know the reason why the crime was unreported.

[37] Only in the 2017 NCVS did a large share of immigrants pick "other" as a response. In 2018 and 2019, of immigrant victims of serious crime who did not report it, just 5 percent in both years gave "other" as the reason they did not come forward. Of immigrant victims of serious violent crime in 2018, none gave "other" as the reason for not reporting it and just 3 percent gave it as a reason in 2019. And the sample is very small in individual years for these two categories of crime. In 2017, there were only nine unweighted cases in which an immigrant victim of serious crimes gave "other" as the reason it was not reported. For unreported serious violent crimes, only four immigrants gave "other" as the reason it was not reported. There is no obvious reason why a large number of immigrant victims would pick this specific response in 2017, but not in other years. Also, for other types of crimes shown in Table A4, very few immigrants gave "other" as the reason for not reporting a crime. It is possible that perhaps the larger number who chose "other" in 2017 reflects some kind of problem or coding error for these types of crimes in that one year.

[38] See Robert C. Davis and Nicole J. Henderson, "Willingness to Report Crimes: The Role of Ethnic Group Membership and Community Efficacy", in Crime and Delinquency, October 2003.

[39] "Delegation of Immigration Authority Section 287(g) Immigration and Nationality Act", U.S. Immigration and Customs Enforcement, undated.

[40] See Jessica M. Vaughan and Bryan Griffith, "Map: Sanctuary Cities, Counties, and States", Center for Immigration Studies, March 22, 2021.

[41] Dividing communities between those with more or fewer than 100,000 residents is common convention. It is also useful in this analysis because it distributes the sample population equally for most types of crime, creating more statistically robust estimates across the categories of crime.

[42] Copies of training materials are available upon request.

[43] See Jessica M. Vaughan, "Visas for Victims: A Look at the U Visa Program", Center for Immigration Studies, March 30, 2020.

[44] "DHS announces launch of new office for victims of illegal immigrant crime: Office built with input from victims impacted by crime DHS Launches New Office for Victims of Illegal Immigrant Crime", U.S. Immigration and Customs Enforcement, April 26, 2017.

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ORIGINAL ARTICLE

CONTEMPORARY ECONOMIC POLICY

The local effects of federal law enforcement policies: Evidence from sanctuary jurisdictions and crime

Dale T. Manning | **Jesse Burkhardt**

Department of Agricultural and Resource Economics, Colorado State University, Fort Collins, Colorado, USA

Correspondence
Jesse Burkhardt, Department of Agricultural and Resource Economics, Colorado State University, 1200 Center Ave. Mall, Fort Collins, CO 80523, USA.
Email: jesse.burkhardt@colostate.edu

Abstract

This study estimates the association between sanctuary policies and crime in the United States by exploiting an increase in state and local sanctuary policy adoption in 2014. Counties that adopted sanctuary policies in 2014 experienced a decrease of 17.9 violent crimes per 100,000 inhabitants per year (0.02 percentage points) compared to counties that continued to cooperate with Immigration and Customs Enforcement Agency (ICE), with the relationship driven by decreases in robberies and assaults. This result implies that sanctuary policies avoid $101 million per year in crime costs. Conversely, ICE cooperation increases crime costs in local communities by $3.28 billion per year.

KEYWORDS
crime, immigration policy, law enforcement policy, sanctuary city

JEL CLASSIFICATION
J15, K37, K14

1 | INTRODUCTION

American attitudes on immigration policy have become polarized in recent decades. A significant response at the Federal level, under both Republican and Democratic presidents, has been to create detainer policies. The Immigration and Customs Enforcement Agency (ICE) implements detainer policies by ordering local law enforcement offices to detain suspected illegal immigrants when arrested for nonimmigration related crimes (TRAC Immigration, 2017). While detainer policies are applied broadly, today, nearly 250 local jurisdictions, including five states, have established sanctuary policies indicating they will not honor ICE detainer requests (CIS, 2018). Local sanctuary policies are thus in conflict with federal detainer policies, which has sparked debate about their legality and impacts. Both detainers and sanctuary policies are thought to have multifaceted impacts on public safety and policing effectiveness, and as a result, they have received substantial criticism, including from the law enforcement community and public officials. In this study, we exploit a wave of sanctuary policy adoption to provide rigorous empirical evidence of the monetary impacts of sanctuary jurisdictions (SJs) on crime, and by reflection, the effects of detainer policies on crime costs.

Often referred to as "sanctuary cities," counties are the most common SJs in our analysis, though several states have also adopted sanctuary policies. The formation of SJs has sparked debate about their impacts on communities, and particularly on local violent and nonviolent crime. While Hausman (2020) finds that SJs did not reduce deportations of

Abbreviations: ICE, Immigration and customs enforcement agency; SJ, sanctuary jurisdiction; UCR, Uniform crime reporting program.

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people with violent convictions, many argue that SJs encourage the release of criminals from incarceration and lead to an increase in local crime. Others advocate for SJ policies because the threat of deportation for minor offenses can erode trust in authority and lead to less effective policing. For example, Martínez-Schuldt and Martínez (2021) find that immigrants increase crime reporting rates after SJ policies are implemented. This implies that SJs can enhance the effectiveness of policing and can thus better deter potential criminals. These mechanisms suggest that SJs could either increase or decrease the incidence of crime.

We use data from the Federal Bureau of Investigation Uniform Crime Reporting Program (UCR) to estimate the relationship between SJ policies and a variety of categories of crime at the county-annual level from 2010 to 2016. This dataset, previously used to investigate questions such as the impact of available cash (Wright et al., 2017) and marijuana legalization (Maier et al., 2017; Morris et al., 2014) on crime rates, provides a standardized database on crimes by category (e.g., assault, rape, etc.) within each US county over time. Although there are several known issues with the UCR that we discuss in our Data section, it has become a widely used dataset for law enforcement professionals as well as academic researchers (UCR website).[1]

We employ a difference-in-differences strategy combined with a rich set of fixed effects and control variables to control for time-invariant county-specific unobservables and time-varying state-and county-specific factors that might bias our estimates. Our statistical strategy takes advantage of a significant increase in 2014 in the number of counties that adopted sanctuary policies (Miranda, 2014). This wave of policy adoption was preceded by years of increased federal enforcement and an unanticipated court case that suggested that complying with detainers may be unconstitutional. Our baseline finding is that SJ adoption is associated with a reduction in violent crime at the county level. The result is robust to a variety of modeling assumptions and is further corroborated through a placebo test. While we cannot disentangle the mechanisms through which SJ policies impact crime, our findings represent a rigorous attempt to establish a plausible association between sanctuary policies and crime.

We have several important findings. First, our results suggest that SJ adoption decreases violent crime incidents by 17.9 per 100,000 residents per year per county, or 0.02 percentage points. This reduction is driven primarily by decreases in aggravated assaults (11.7 per 100,000 residents), supporting the argument that increased trust in local law enforcement outweighs the cost of releasing potential repeat offenders. Moreover, it follows that policy that obliges cooperation between local law enforcement and ICE—detainer policies—leads to increases in crime rates. Using estimates of the cost of crime from McCollister et al. (2010), our results imply that SJ policies have decreased local annual crime costs by $1.1 million per county, or $101 million across the 88 counties that adopted SJ policies in 2014 (this excludes counties in states that adopted policies in 2014). This suggests that SJ policies have had economically significant impacts where implemented. Equivalently, federal detainer policies that require cooperation between local jurisdictions and ICE increase local crime costs by $3.28 billion per year across all counties that honor ICE requests.[2]

In contrast to the effects on violent crimes, we find no evidence that SJ policies impact nonviolent property crime rates. However, we find that violent crime effects are not uniform across the US population. For instance, the effects are strongest in urban counties, counties with larger populations of foreign born residents, and counties with low unemployment rates. Also, reductions in crime are driven by county-level sanctuary policies, with no clear effect in states that adopt state-level policies.

Our findings provide important empirical evidence on the socioeconomic impacts of SJ and detainer policies, and highlight the degree to which federal crime policies can impose costs on local communities, governments, and authorities. We find that local law enforcement agency cooperation with ICE imposes an economically important cost on local communities. These costs include higher crime rates, suggestive of less effective policing. The increase in crime could result because local cooperation with federal immigration authorities decreases crime reporting among Latino communities (Martínez-Schuldt & Martínez, 2021). In general, accounting for interactions between federal policy and local governments can improve the efficiency and effectiveness of public policies. Our results show that there are substantial ancillary benefits to SJ policies, and they offer important food for thought as many states and local jurisdictions continue to contemplate federal detainer policies.

2 | BACKGROUND AND RELEVANT LITERATURE

In this section, we provide background on the origin and development of ICE, the history of SJs in the United States, including a court case that is part of our identification strategy, and predicted impacts of SJs on crime. We also place our paper into several strands of relevant literature.

2.1 | A brief history of ICE

The Immigration and Customs Enforcement Agency, or ICE, originated as part of the Department of Homeland Security, which emerged from the Homeland Security Act of 2002 (www.ice.gov). The agency's objective was to protect national and public safety in the aftermath of the September 11, 2001 attacks on the World Trade Center and Pentagon. As of 2018, the agency had more than 20,000 employees and a budget of approximately $6 billion per year. A key function of the agency includes Enforcement and Removal Operations, which focuses on the deportation of individuals in the US illegally.

One tool through which ICE identifies and pursues undocumented immigrants is through the use of "detainers." Local police departments are asked to inform ICE when they interact with undocumented immigrants. When this occurs, ICE issues a formal detainer, allowing local jurisdictions to hold immigrants for up to 48 h, and for ICE to begin deportation procedures.[3] Detainers are issued when ICE has probable cause for suspecting that the detainee is removable from the United States. They allow ICE to take custody of the immigrant after release from local authorities.

Issuing detainers is controversial and has led many local law enforcement agencies to refuse cooperation with ICE. Refusal to cooperate with this policy most often means not honoring ICE detainer requests, or requiring a warrant from a judge in order to detain an immigrant for additional time.

2.2 | Sanctuary policies and crime

While sanctuary policies date back to the early 1980s (Paik, 2017), there has been an increase in the frequency of SJs in response to the use of detainers by ICE. Colbern et al. (2019) argue that the increase in SJs resulted from an increase in harsh federal policies that denied immigrants a pathway to citizenship. In particular, the Secure Communities program, which automatically shares fingerprint data from local jails with the Department of Homeland Security, faced significant resistance from pro-immigrant movements. In 2014, federal judges in several states, including Pennsylvania, Rhode Island, and Oregon issued rulings that questioned the legality of detainer orders under the constitution. Most notably, in a case involving Clackamas County, Oregon (Miranda-Olivares v. Clackamas County), a judge ruled that detaining an individual beyond the time needed for local charges represented a violation of the fourth Amendment guarantee of freedom from unreasonable seizure. In that case, Miranda-Olivares was granted bail, but Clackamas county jail officials informed her she could not leave because of an ICE detainer. Instead, she remained in jail roughly 2 weeks while waiting on the case. Clackamas county argued that it had to keep Miranda-Olivares detained because of the ICE order. Instead, the ruling suggested that ICE detainers resemble a request instead of an order, and that Clackamas county should not have prevented Miranda-Olivares from posting bail. As a consequence, the county was ordered to pay Miranda-Olivares nearly $130,000 for damages and court fees.

The Clackamas county court decision created local liability for violating immigrants' constitutional rights. This shifted local SJ policy decisions from political to economic. As a result, many local jurisdictions implemented sanctuary policies. Within a week of the Clackamas county ruling, eight other counties in Oregon adopted SJ policies.[4]

In total, 88 counties and 4 states implemented SJ policies in 2014 (see Table 10 in the Supporting Information S2 for a list of adoptions by year). As of September 2018, SJs covered nearly 250 counties across the US, including counties in five states that adopted state level policies.[5] In many cases, a detainer request is not honored at all. In others, additional requirements exist for the local jurisdictions to keep immigrants in custody. For example, Clackamas County now only honors ICE requests with a court order or warrant.

As SJs have become more prevalent, competing hypotheses exist about the impact of SJ policies on crime in local communities. For instance, SJs reduce the punishment to undocumented immigrants for committing crimes because the threat of deportation is reduced (Hausman, 2020). This poses the additional risk that criminals who are released could become repeat offenders (Vaughn, 2015). An often cited case for this is the murder of Kate Steinle in San Francisco after local authorities released an undocumented immigrant from county jail.

Alternatively, SJs may lead to decreases in violent crime (Lederman et al., 2002; Sampson et al., 1997). For instance, trust between law enforcement and local communities can lead to more cooperation with investigations, which may increase the probability of solving crimes when they occur (Carr et al., 2007; Hagan et al., 2018). Martínez-Schuldt and Martínez (2021) find that Latinos are more likely to report crimes after the establishment of an SJ. This would increase the likelihood of an arrest after a crime. Given evidence that criminals respond significantly to changes in expected punishment (Drago et al., 2009), SJ policies could decrease crime rates. Related research on reports of gunshots after an

incident of police violence (Ang et al., 2021) provides additional evidence that trust in law enforcement can increase community engagement. Importantly, this mechanism works against all would-be criminals, not just those who are undocumented immigrants. Furthermore, recent research suggests that immigrant communities commit fewer crimes than native-born populations (Ferraro, 2013; Martinez, 2006). The impact of SJ laws on crime therefore depends on the net effect of these various opposing mechanisms.

The literature on the economics of crime has revealed relevant insights into the factors that contribute to crime (Levitt, 2017). According to Levitt (2004), more police, larger prison populations, legalized abortion, and the end of the crack epidemic all contributed to lower crime rates across most regions of the US. Similar to SJs, Miles and Cox (2014) find that "Secure Community" policies that allow local law enforcement to check the immigration status of all people arrested lead to no measurable effect on crime rates. Investigating a related policy in Italy, Fasani (2018) finds that amnesty programs for undocumented immigrants have a small and transient negative impact on crime committed by non-EU immigrants. Using a regression discontinuity design, Pinotti (2017) finds that legalization in Italy decreased an immigrant's probability of committing a crime by 0.6 percentage points.

Studies that examine the impact of SJs on crime include Wong (2017), Hernandez (2016), Martínez et al. (2018a), Martínez-Schuldt and Martínez (2017), Hausman (2020), and O'Brien et al. (2019). Generally, empirical studies find that SJ policies have no effect (Hausman, 2020; O'Brien et al., 2019) or a negative effect on local crime (see Martínez et al., 2018b, for a review). Using data on policies implemented between 2010 and 2015, Hausman (2020) finds that SJ policies decrease the deportation of nonviolent offenders, while not significantly decreasing the deportation of violent offenders. He also finds no evidence that crime rates change after SJs are passed using a sample of 296 counties. Wong (2017) uses matching techniques to compare outcomes in SJs with other areas that have similar characteristics. He finds that cities with SJ policies have higher incomes, less poverty, and lower crime rates, but relies on cross-sectional comparisons of areas with and without SJ policies. This suggests the potential for omitted variables (or reverse causality). Also using a matching method, O'Brien et al. (2019) find evidence that the impacts of sanctuary policies on crime are minimal. Hernandez (2016) investigates the impact of SJs on crime using a comparison between cities that do and do not have SJ policies. He finds that policy impacts depend on the type of SJ policy, and that some policies lead to decreases in property crime while other policies can cause increases. Related to this, Lyons et al. (2013) find that favorable local immigration policies can strengthen the decrease in crime associated with larger immigrant shares in a community.

Martínez-Schuldt and Martínez (2017) in the sociology literature is the most similar to our analysis. They use 107 US cities that implemented related policies between 1990 and 2010 to examine the impact of sanctuary policies, using a broader definition than we use here. Results suggest no change in homicides and a decrease in robberies. Importantly, the SJ policies in our analysis were implemented during the wave of policy adoption in 2014. Therefore, we expand on previous work (O'Brien et al., 2019) by including data through 2016, which captures the time period in which a large number of SJ policies were implemented, and by using well established statistical methods to control for confounding effects. Our analysis builds on Hausman (2020) and O'Brien et al. (2019) by finding significant negative effects when including a larger dataset of treatment and control counties across the country. We confirm that the results of Wong (2017) hold when using an identification strategy that is conducive to controlling for unobserved time-invariant county level characteristics that could influence the adoption of SJs. Finally, given that we use a different dataset to define SJ, our results demonstrate that the negative effects found in the literature are not driven by the choice of SJ definition.

In summary, 2014 saw a stark increase in the number of SJ policies adopted, representing one of three policy waves since the late 1990s (Colbern et al., 2019). The 2014 wave was motivated by resistance to policies in Washington, DC, and was further incentivized by a landmark court case in Clackamas County, Oregon. Our estimation strategy utilizes this sudden increase in county-level adoption. We discuss potential threats to identification and robustness checks in the following sections. Finally, as more counties adopt policies, information about their impacts can facilitate adoption in other counties (Boehmke & Witmer, 2004).

3 | DATA

The data for this analysis come from three main sources. First, we requested and were granted access to monthly crime count data from the Federal Bureau of Investigation Uniform Crime Reporting Program (UCR) from 2010 to 2016 (FBI, 2018). Crimes are reported at the county level for nearly all counties in the US. The UCR tracks several major categories

of violent crimes, including assaults, murder, forced and attempted rape, robbery, and several major categories of property crimes, including burglary, larceny/theft, and motor vehicle theft. The sum of each of the subcategories produces counts of aggregate violent crimes and aggregate property crimes. In our analysis, we focus first on the aggregate violent and property crimes but use the disaggregated categories to shed light on the mechanisms behind our primary result.[6]

Second, we downloaded demographic data, including median household income, the fraction of the population that is white, the fraction of the population that is between the ages of 18–64, the fraction of the population that is older than 64, county population, and the number of foreign born residents from the American Community Survey (ACS) (ACS, 2018). We also gathered county level unemployment rates from the Bureau of Labor Statistics (BLS, 2018). We use the designation of urban and rural status for each county from the Center for Disease Control (CDC, 2018). We perform our analysis at the annual level because the Census data are reported at the annual level. We also begin our panel in 2010 because the ACS does not report annual estimates prior to 2010.[7] Finally, we downloaded county-level presidential voting outcomes for the 2016 election from the MIT Election Data Science Lab (https://electionlab.mit.edu/).

Third, we manually recorded information on SJ status by state, county, and municipality from the Center for Immigration Studies (CIS, 2018). Figure 1 illustrates the counties used in our analysis, and whether they have an SJ policy, according to CIS (2018), as of 2016. In some cases, SJ laws are passed by municipalities. As such, we assign the SJ status to each county that overlaps a municipality with an SJ policy. In 2014, 88 counties adopted SJ laws, as well as four states (CA, CO, RI, and NM). Treatment is assigned to 2014 and all subsequent years. For example, if a county adopted in May 2014, the policy variable is equal to 1 for that county in 2014 and all years thereafter. If a county adopted an SJ policy in a year other than 2014, we drop that county from our analysis (this drops 18 counties). Table 10 in the Supporting Information S2 lists the number of counties that adopted in 2014 relative to other years. The data indicate that 86.6% of counties with SJ policies by 2016 adopted in 2014 after the Clackamas County ruling that places liability on local jurisdictions for damages caused by potentially unconstitutional detainment. Our primary specification uses all other nonadopting counties as controls, but we explore alternative definitions of control groups in subsequent analyses.

Our sample of SJs differs from previous work on the impact of SJs (e.g., Martínez-Schuldt & Martínez, 2017). The CIS definition of SJ requires evidence that the jurisdiction does not cooperate with ICE detainer requests. This is a relatively narrow definition of SJ compared to, for example, the National Immigration Law Center (NILC),[8] which includes other policies and focuses on cities. One such example is San Rafael, California, which opposed amendments

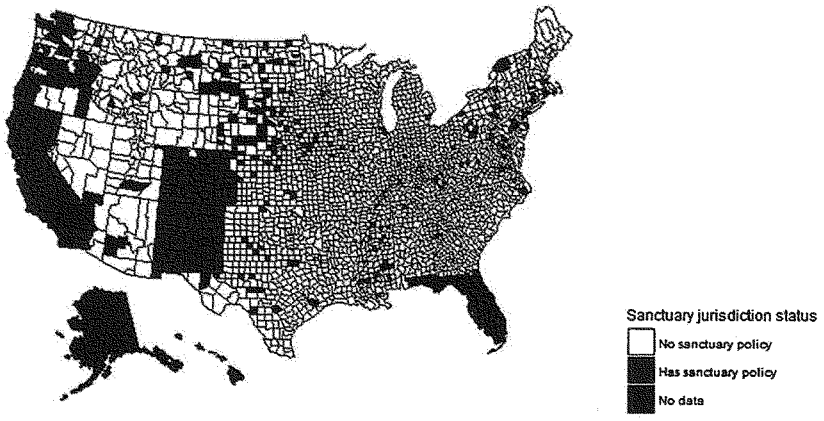

FIGURE 1 Map of sanctuary jurisdictions in the United States

TABLE 1 Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	*N*
County-yearly crime rates per 100,000 population					
Violent	347.232	1696.085	0	53,073	18,373
Aggravated assault	215.079	969.987	0	31,791	18,373
Murder	4.465	22.48	0	835	18,373
Rape	58.759	183.95	0	4752	18,373
Robbery	97.502	644.544	0	20,245	18,373
Property	2469.807	8479.788	0	1,92,143	18,373
Burglary	544.611	1819.503	0	46,982	18,373
Motor vehicle theft	184.584	911.184	0	22,872	18,373
Control variables					
County population	92,029	2,43,355.196	428	52,53,756	18,373
Foreign born population	8870.342	46,117.256	0	11,38,017	18,373
Median income	53,545.333	7674.659	33,321	77,216	18,373
Unemployment rate	6.682	2.019	2.675	13.5	18,373
% White	0.797	0.095	0.256	0.956	18,373
% ≥65	0.141	0.017	0.078	0.194	18,373
% 18–64	0.622	0.011	0.589	0.719	18,373
% male	0.493	0.006	0.472	0.524	18,373
Urban	0.389	0.488	0	1	18,373
Republican vote share	0.637	0.15	0.041	0.918	2714

Note: The primary sample contains 2940 counties. Several categories of crime have 2 observations with negative values. We drop all variables for those county-years. Republican vote share is the fraction of votes for the Republican candidate in the 2016 presidential election. This is only observed in 2016 and not for all counties.

to the Patriot Act in 2003 that would allow cooperation with local law enforcement. This placed them on the NILC list of SJs but would not place them on the CIS list. Furthermore, our more recent data allow us to evaluate the impact of SJ policies implemented in the 2014 wave of policy adoption.

Summary statistics on crime and county controls are provided in Table 1 for the time period of our study (2010–2016). Annually, there are 347.2 violent crimes per 100,000 residents on average per year, the majority of which (215) are aggravated assaults. This translates into a crime rate of 0.35%. There are substantially fewer murders, rapes (attempted and forced), and robberies than aggravated assaults, with averages of 4.5, 59, and 98 per 100,000 people, respectively. There are 2469 property crimes per 100,000 residents per year, and roughly 22% of those are burglaries, on average. Demographic variable summary statistics show that the population is approximately 80% white across treatment and control counties, with an average unemployment rate over the study period of 6.7%. The average number of foreign born residents per county is 8870, or 9.5% of the population. As expected, the county populations are roughly half male.

4 | METHODS

4.1 | Empirical model

We employ a difference-in-differences design to estimate the causal impact of SJ policies in each year after the treatment. This specification has been used in many settings, for instance, to evaluate the impact of medical marijuana laws on traffic fatalities (Anderson et al., 2013), crime (Morris et al., 2014), and opioid mortality (Powell et al., 2018). Our

primary specification at the county-year level is as follows, although we also present results from a range of specifications to demonstrate the robustness of our results:

$$c_{ct}^{j} = \alpha_{c}^{j} + \gamma_{t}^{j} + X_{ct}'\beta^{j} + \sum_{\tau=2008}^{\tau=2016} (SJ_{c} * y_{c\tau})\delta_{\tau}^{j} + \varepsilon_{ct}^{j}, \quad (1)$$

where c_{ct}^{j} is the crime rate per 100,000 residents of crime type j in county c in year t, α_{c}^{j} is a county fixed effect, γ_{t}^{j} is a year fixed effect, X_{ct}' is a vector of control variables, including median income, percentage of the population that is white, percentage of the population that is between the ages of 18–64, percentage of the population that is older than 64, and the number of foreign born residents.[9] The variable SJ_{c} is an indicator equal to one if a county adopted an SJ policy in 2014 and $y_{c\tau}$ is a year dummy variable equal to 1 if $\tau = t$ and 0 otherwise. The omitted year is 2013. Thus, the coefficients of interest are δ_{τ}^{j}, which provide the effect of SJ adoption on crime category j in year τ relative to 2013 and relative to counties that did not adopt policies. All models are estimated via ordinary least squares and standard errors are clustered at the state-level.[10] As an additional robustness check, we present Poisson estimates using violent crime *counts* in Section 6.

4.2 | Identification

The primary assumption for causal identification in our model is that pre-treatment crime trends are statistically similar in control and treated states and counties, that is, the parallel trends assumption. We evaluate this econometrically in our main specification.[11]

The policy wave of 2014, instigated partly by the Clackamas county court case, provides us with a set of treated and untreated counties. A relatively benign assumption is that neither the outcome of the Clackamas County court case nor federal level policy in the years prior to 2014 were spurred by the actions of individual counties.[12] Therefore, the precise timing of the adoption wave can be considered exogenous.

If counties were randomly assigned SJ policies in response to the court case, fixed effects and control variables would not be unnecessary. However, not all counties adopted SJ policies in 2014 and thus, the counties that did adopt SJ policies likely selected to adopt based on observable and unobservable (to the researcher) characteristics. Therefore, we first include county fixed effects to control for time-invariant county-specific characteristics that might be correlated with the probability of SJ adoption and crime rates such as urban status. Second, we include year fixed effects to control for time-varying nationwide characteristics that might be correlated with the probability of SJ adoption and crime rates, such as general macroeconomic trends and national news coverage of crime and immigration. Last, counties may choose to adopt policies based on time-varying county-specific characteristics, such as local crime, population, or demographic trends. To address this concern, we include additional time-varying county-level control variables that could influence crime rates including the percentage of the population that is male, the percentage of the population that is 18–64, the percentage of the population that is older than 64, the percentage of the population that is White, the number of foreign born residents, median income, and the unemployment rate.

Despite the 2014 policy wave, fixed effects, and control variables, one might still be concerned about selection and omitted variable bias (confounders). With respect to omitted variable bias, robustness to the inclusion or exclusion of time-varying controls suggest that the remaining omitted variables would have to be uncorrelated with the control variables to lead to biased estimates (Schlenker et al., 2007).

To more formally test for selection in our data, we regress the adoption dummy, SJ_{c}, on 2013 or 2014 aggregate violent and property crime rates and control variables. The results are presented in Table 2.[13] Columns 1 and 3 use control variable data from 2014 but lagged crime rates (crime rates from 2013). Columns 2 and 4 use control variable data from 2013 and use contemporaneous crime rates (crime rates from 2013). Columns 1 and 2 include all counties that adopted policies, including states, while columns 3 and 4 omit state-level adopters, similar to our primary specification. We find that when we include all counties that adopted, including state adopters (columns 1 and 2), 2013 violent and property crimes are moderately correlated with the probability of adoption. Perhaps unsurprisingly, the number of foreign born residents and republican vote share are strongly correlated with the probability of adoption. However, when we limit the sample to only county adopters (state-level adopters are omitted), then the probability of adoption is moderately correlated with the fraction of the population that is older than 65 and the republican vote share (columns 3 and 4). These findings, along with findings in the following section lead us to drop state-level adopters. To address selection issues, we control for each of the variables in Table 2 either directly or via our fixed effects, and we perform several robustness checks, including a matching routine, in Section 6.

TABLE 2 Selection on observables

	(1)	(2)	(3)	(4)
Lagged violent crime rate	−0.000* (0.000)		−0.000 (0.000)	
Lagged property crime rate	0.000 (0.000)		0.000 (0.000)	
Violent crime rate		−0.000* (0.000)		−0.000 (0.000)
Property crime rate		0.000* (0.000)		0.000 (0.000)
Foreign born (1,000s)	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)
Median income	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Unemployment rate	0.029 (0.024)	0.014 (0.011)	0.030 (0.030)	0.011 (0.013)
% White	0.211 (0.154)	0.173 (0.161)	0.149 (0.142)	0.134 (0.157)
% Between 18 and 64	1.063 (1.262)	1.695 (1.610)	0.112 (1.154)	1.250 (1.662)
% ≥ 65	0.952 (0.779)	1.024 (0.784)	1.302* (0.686)	1.303* (0.686)
% Male	5.521 (3.487)	5.325 (3.587)	4.268 (4.403)	3.631 (4.257)
Urban status	0.017 (0.013)	0.018 (0.013)	0.013 (0.013)	0.014 (0.012)
Republican vote share	−0.164*** (0.056)	−0.183*** (0.064)	−0.102** (0.046)	−0.124** (0.061)
R^2	0.118	0.112	0.081	0.068
N	2751	2745	2604	2598

Note: This table presents estimates of potential sample selection bias. The dependent variable in each column is the indicator for whether or not a county adopted a sanctuary policy in 2014. Columns 1 and 3 use data from 2014 and include lagged crime rates. Columns 2 and 4 use data from 2013 and use contemporaneous crime rates. Columns 1 and 2 include all counties that adopted policies, including states, while columns 3 and 4 omit state-level adopters, similar to our primary specification. Standard errors are clustered at the state-level. Within R^2 reported. ***Indicates 1% significance, **indicates 5% significance, *indicates 10% significance.

5 | RESULTS

5.1 | Primary results

Table 3 presents the results of estimating Equation 1, with violent crime rates as the outcome variable in columns 1–3, robbery rates as the outcome variable in columns 4–6, and aggravated assaults as the outcome variable in columns 7–9. Columns 1, 4, and 7 present estimates using all county and state-level adopters, columns 2, 5, and 8 present estimates using state-level adopters only, and columns 3, 6, and 9 present estimates using county-level adopters only. The coefficients demonstrate the association between the 2014 SJ policies and crime rates between treated and control counties (δ_t^j) after 2014 relative to before 2014. Several observations become apparent. First, crime rates are not statistically different between treated and control counties prior to 2014 for all treatment groups except state-level adopters (columns 2, 5, and 8). This provides statistical support for parallel pre-treatment trends using county-level adopters (conditional on controls), which is necessary for our identification strategy. Next, in 2014 or 2015, county-level adopters experienced a decrease in violent crime rates, robbery rates, and aggravated assault rates, relative to control counties (columns 3, 6, and 9). County level policies appear to have the largest and most persistent impact on violent crime, with statistically significant effects in all years after policy implementation. On the other hand, the effect of state-level SJs is less clear. This would be expected, for example, if state level policies were implemented to varying degrees by local authorities.

5.2 | Impact by crime type

In the previous section, we found that the introduction of an SJ policy is associated with a decrease in violent crime, robbery, and aggravated assault rates, and that this effect is particularly strong for policies implemented at the county level. Therefore, in this section we focus only on 2014 county-level SJ adopters (omitting state-level adopters), and we explore the crimes that drive our primary result. We use a simple difference-in-differences specification, with controls

and fixed effects, and treatment occurring in 2014 and all years thereafter.[14] Table 4 presents the average impact of county level policies on two aggregate categories (violent and property) as well as the specific crimes of murder, robbery, rape, aggravated assault, aggregate property crimes, burglary, and motor vehicle thefts.[15] Table 4 suggests that the decrease in violent crime is driven primarily by an 11.7 crime per 100,000 resident reduction in aggravated assault. All other crimes have statistically insignificant treatment effects.

TABLE 3 Primary results on violent crimes

	(1) Violent All	(2) Violent State	(3) Violent County	(4) Robbery All	(5) Robbery State	(6) Robbery County	(7) Assault All	(8) Assault State	(9) Assault County
Treatment effect in 2010	1.328	20.290**	−5.111	−1.708	−7.817***	0.431	1.460	30.961***	−8.682
	(11.588)	(9.155)	(13.616)	(2.042)	(2.296)	(2.606)	(11.431)	(9.566)	(11.926)
Treatment effect in 2011	−2.226	8.536	−5.914	−3.799*	−8.490	−2.235	−0.871	19.372*	−7.777
	(8.706)	(10.530)	(9.988)	(2.143)	(5.764)	(1.919)	(9.265)	(10.253)	(9.590)
Treatment effect in 2012	−8.336	−0.971	−10.427	−1.638	−3.987*	−0.684	−6.789	8.125	−11.617
	(6.766)	(6.652)	(7.780)	(1.036)	(1.995)	(1.079)	(7.011)	(5.572)	(7.448)
Treatment effect in 2014	−6.096	7.336	−10.862***	−3.096	−8.050	−1.732	−3.062	15.799**	−9.278***
	(3.857)	(8.349)	(3.768)	(2.499)	(7.870)	(1.223)	(4.300)	(7.060)	(3.082)
Treatment effect in 2015	−17.411**	14.756	−28.384***	−4.383*	−0.352	−5.942***	−11.832*	15.861	−21.070***
	(8.398)	(14.078)	(7.459)	(2.281)	(6.366)	(2.111)	(6.979)	(11.096)	(6.354)
Treatment effect in 2016	−17.438*	22.469	−30.869***	−3.868	0.047	−5.341*	−14.227	20.765**	−25.826***
	(10.196)	(16.609)	(8.828)	(3.094)	(10.460)	(2.795)	(8.864)	(10.187)	(7.567)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Within R^2	0.008	0.008	0.009	0.012	0.009	0.013	0.007	0.008	0.008
N	19,404	18,598	18,373	19,404	18,598	18,373	19,404	18,598	18,373

Note: The dependent variables are crime rates per 100,000 residents. Each model includes year and county fixed effects and control variables listed in the Methods Section. Columns 1, 4, and 7 include all state and county SJ adopters in 2014 and control counties. Columns 2, 5, and 8 include only state-level adopters in 2014 and control counties. Columns 3, 6, and 9 include only county-level adopters in 2014 and control counties. Standard errors are clustered at the state-level. Within R^2 reported. ***Indicates 1% significance, **indicates 5% significance, *indicates 10% significance.

TABLE 4 Difference-in-differences table

	(1) Violent	(2) Murder	(3) Robbery	(4) Rape	(5) Assault	(6) Property	(7) Burglary	(8) MVT	(9) Larceny
SJ	−17.897**	−0.025	−3.685	−4.950	−11.669*	9.234	−8.721	5.669	12.285
	(7.879)	(0.188)	(2.652)	(3.533)	(6.715)	(40.842)	(17.540)	(8.214)	(27.031)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.009	0.003	0.012	0.042	0.008	0.151	0.166	0.020	0.113
N	18,373	18,373	18,373	18,373	18,373	18,373	18,373	18,373	18,373

Note: The dependent variables are the crime rates per 100,000 residents of each category of crime. Each model includes year and county fixed effects and control variables listed in the methods section. Assault is aggravated assaults and MVT is motor vehicle theft. The models are simple difference-in-differences estimates for each category of crime, where before and after is defined by pre- and post-2014. The models do not include the states that adopted SJ policies as the state-level results are not significant. Standard errors are clustered at the state-level. Within R^2 reported. **indicates 5% significance, *indicates 10% significance.

Our results indicate that SJ policies are not associated with statistically significant increases in any crimes rates, but are associated with statistically significant decreases in aggravated assaults and robberies (see Table 3). This suggests that better policing and/or trust between law enforcement and immigrant communities dominates increases in criminals on the street, and that the improved policing has especially strong effects on particular types of crime.

Notably, we a find that the reduction in violent crime is driven largely by a negative impact on assaults and robberies. One possible explanation for our findings is that the reporting and prosecution of assaults and robberies depends on the willingness of victims to cooperate with police. This is particularly true for domestic violence cases (Dawson & Dinovitzer, 2001). It is also the case that victims' perceptions of the law enforcement community affect the likelihood that they cooperate with authorities (Koster, 2017). Since a commonly presented benefit of SJs is improved trust in law enforcement (e.g., addressing New Jersey's new law restricting cooperation with federal officials, the chief of New Jersey's state policy emphasized the importance of trust[16]), victims may be more likely to report crimes and cooperate during the prosecution process. This means that potential offenders would have a higher probability of being caught, creating a greater deterrent effect. This effect may be particularly strong for assault and robbery cases. Also, the frequency of murder is much lower than other crimes (with zeros for many county-years). Therefore, there is more room for decreasing assault and robbery rates through improved policing and trust within the community.

6 | ADDITIONAL SPECIFICATIONS, ROBUSTNESS CHECKS, AND TREATMENT HETEROGENEITY

One might be concerned that SJ adopting counties are dissimilar to counties that do not adopt SJ policies (Section 4.2) or the results depend on the control group or inclusion of control variables. To address these concerns, we test the robustness of our results to alternative specifications.

For example, the additional control variables could be endogenous, which would lead to biased estimates of our primary treatment effect. To alleviate this concern, we present four robustness checks in Table 5. We focus on the impact of SJ policies on violent crime in county level adopters. First, we estimate our primary model without control variables (column 1). Second, one might be concerned that the control variables are endogenous due to reverse causality (e.g., crime rates affect demographics, which in turn affects the probability of SJ adoption). To address this concern, we estimate our model with lagged control variables (column 2). Third, we estimate a nearest neighbor matching routine, matching on all of our control variables (data from 2013, the year prior to treatment) as well as urban status, republican vote share, and violent crime trends (the change in violent crime rates between 2013 and 2014).[17]

Based on the discussion in Section 4.2, if selection were a concern, we might think that urban counties and counties with higher numbers of foreign born residents were more likely to adopt. This relationship is demonstrated in columns 1 and 2 of Table 2. Column 4 of Table 5 limits the sample to urban counties and counties with higher than the median number of foreign born residents. Finally, column 5 limits the sample to counties with republican vote shares in the

TABLE 5 Alternative specifications

	(1) No controls	(2) Lagged controls	(3) Matching	(4) Urban/Foreign	(5) Democrat
SJ	−14.164* (7.635)	−85.302*** (30.357)	−26.486** (11.041)	−25.115* (14.020)	−32.091** (14.065)
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
R^2	0.003	0.009	0.019	0.057	0.012
N	18,373	18,373	1166	3626	7262

Note: This table replicates the simple difference-in-differences results reported in Table 4 for violent crimes with several alternative specifications. Column 1 estimates the primary model (column 1 of Table 4 without control variables. Column 2 estimates the primary model (column 1 of Table 4) with lagged control variables. Column 3 uses a 1-1 nearest neighboring matching approach. Matching variables include urban status, the change in crime rates, the percentage of the population that is white, between the ages of 18–64, older than 64, male, number of foreign born residents, median income, the unemployment rate, and republican vote share. The dependent variable in each model is the violent crime rate. Column 4 limits the sample to urban counties and counties with above median foreign born residents. Column 5 limits the sample to below 50% republican vote shares (Democrat counties). Standard errors are clustered at the state-level. Within R^2 reported. ***Indicates 1% significance, **indicates 5% significance, *indicates 10% significance.

2016 presidential election below 50%, that is, Democrat leaning counties. The results show that the association between SJ adoption and violent crime rates is robust across this range of specifications.

We focus our primary analysis on the counties that implemented SJ policies. In Table 6, we present the results for all counties that adopted an SJ policy including state and county level policies (column 1) and for only state-level adopters (column 2). This table supports the observation that impacts are driven by county level policies.

To further test the robustness of our results, Table 7 replicates Table 4, replacing crime rates with crime counts and using Poisson regression. The table supports our previous conclusion that overall, violent crimes decreased in response to SJ policies and that the results are driven by robbery and assault.

Last, we examine treatment heterogeneity. The results are displayed in Table 8. Column 1 interacts the treatment dummy with the unemployment rate. Column 2 interacts the treatment dummy with the number of foreign born residents. Column 3 interacts the treatment dummy with urban status. Column 4 interacts the treatment dummy with the fraction of votes that were for the republican candidate in the 2016 presidential election.[18] The treatment dummy and the associated interaction terms are jointly significant at the 5% significance level in all models. Examining interaction terms, these results provide suggestive evidence that the association between SJ adoption and violent crime rates is smaller in counties with higher unemployment rates but larger in counties with higher numbers of foreign born residents, urban counties, and democratic leaning counties.

6.1 | Placebo check

To confirm that our results are not driven by spurious correlation, we implement a placebo check by randomly assigning treatment status to 3.4% of counties in the sample for 2014 and repeating 1000 times. We assign treatment

TABLE 6 Full sample and state-level adopters only

	(1)	(2)
SJ	−11.180* (6.104)	7.953 (11.106)
County FE	Y	Y
Year FE	Y	Y
Controls	Y	Y
R^2	0.008	0.008
N	19,404	18,598

Note: This table replicates the simple difference-in-differences results reported in Table 4 for violent crimes with several alternative samples. Column 1 estimates the primary model (column 1 of Table 4) on the full sample of data including states that adopted SJ policies and counties that adopted SJ policies. Column 2 limits the sample to only states that adopted SJ policies. The dependent variable in each model is the violent crime rate. Standard errors clustered are at the state-level. Within R^2 reported. *indicates 10% significance.

TABLE 7 Robustness: Poisson regression replication of Table 4

	(1) Violent	(2) Murder	(3) Robbery	(4) Rape	(5) Assault	(6) Property	(7) Burglary	(8) MVT
SJ	−0.073** (0.029)	−0.005 (0.063)	−0.074* (0.039)	−0.052 (0.049)	−0.070* (0.038)	0.028 (0.024)	−0.003 (0.032)	−0.066 (0.073)
County FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
N	18,320	15,774	17,291	18,065	18,318	18,320	18,311	18,279

Note: The dependent variables are the crime *counts* of each category of crime. Each model includes year and county fixed effects and control variables listed in the Methods Section. The models are simple difference-in-differences estimates for each category of crime, where before and after is defined by pre- and post-2014. The models do not include the states that adopted SJ policies as the state-level results are not significant. Standard errors are clustered at the county-level. **indicates 5% significance. *indicates 10% significance.

status to 3.4% of the counties because 88 counties is roughly 3.4% of the counties in our sample. We then estimate the specification presented in column 1 of Table 4 on these 1000 randomly generated datasets. We find that the placebo treatment has a statistically significant effect on violent crime rates in 1.9% of the simulated adoptions. Importantly, the coefficient on treatment is not uniformly positive or negative (it is positive 49.2% in of the bootstrapped samples). This provides further evidence that our estimated impacts are in fact driven by the introduction of SJ policies. The distribution of the placebo treatment effects is displayed in Figure 2.

TABLE 8 Heterogeneity in effects on violent crimes

	(1)	(2)	(3)	(4)
SJ	−85.302*** (30.357)	−11.139 (8.669)	16.920* (9.642)	−75.125*** (27.085)
Unemployment rate	−2.540 (4.458)	−2.555 (4.445)	−2.564 (4.431)	−3.011 (4.604)
Foreign born (1,000s)	−0.867 (0.635)	−0.725 (0.587)	−0.761 (0.583)	−0.730 (0.583)
SJ × Unemployment rate	12.671** (5.019)			
SJ × Foreign born		−0.197* (0.100)		
SJ × Urban			−55.440*** (18.276)	
SJ × % republican votes				116.324** (51.233)
Jointly significant at 95% level	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
R^2	0.009	0.009	0.009	0.009
N	18,373	18,373	18,373	18,267

Note: This table replicates the simple difference-in-differences results reported in Table 4 for violent crimes with several alternative specifications. Column 1 interacts the treatment variable with unemployment rates. Column 2 interacts the treatment variable with the number of foreign born residents per county in 1,000s. Column 3 interacts the treatment variable with a dummy for urban status. Column 4 interacts the treatment variable with the % of votes in the 2016 election that went republican. Standard errors clustered at the county-level. Within R^2 reported. ***Indicates 1% significance, **indicates 5% significance, *indicates 10% significance.

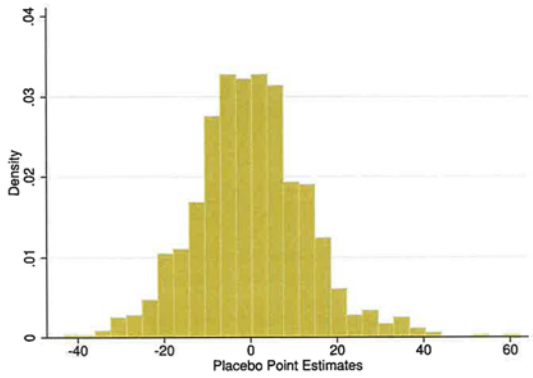

FIGURE 2 Distribution of placebo treatment effects [Colour figure can be viewed at wileyonlinelibrary.com]

7 | DISCUSSION AND CONCLUSIONS

In this study, we present evidence that jurisdictions that introduced sanctuary policies by placing limits on cooperation with ICE experienced decreases in crime relative to counties that continued to comply with ICE detainer requests. The result is driven by county-level policies and is robust to alternative specifications. Results in Table 4 suggest that SJ policy implementation leads to a statistically significant decrease of 17.9 violent crimes per 100,000 residents per county each year. This is a decrease in crime rate of 0.02 percentage points from a base of 0.35%.

Our results are consistent with the existing literature that finds a decrease in crime rates as a result of SJs (Martínez-Schuldt & Martínez, 2017; Wong, 2017). This provides an additional data point using different identification assumptions, an alternative definition of SJs, and a differing time period of analysis. The negative impact on violent crime is most consistent with the hypothesis that SJs can enhance trust in local law enforcement, improve reporting (Martínez-Schuldt & Martínez, 2021), and deter potential criminals from offending. This is consistent with the hypothesis discussed in Lyons et al. (2013) that policies that create political opportunities for immigrants can create a "spiral of trust" between immigrant and nonimmigrant members of a community. This leads to better outcomes across a range of local government responsibilities.

To explore if this result is economically meaningful, we use crime-specific cost estimates from McCollister et al. (2010). These costs include monetary costs to victims, increased government expenditures (including policing), criminal opportunity costs (because of committing crime instead of participating in legal productive activities), and intangible costs, including pain, suffering, and decreased quality of life. We focus on the costs of aggravated assaults since these are the most robust estimates. According to McCollister et al. (2010), an aggravated assault costs society $107,020 in 2008 dollars.

Table 4 suggests that aggravated assaults decrease by 11.7 per 100,000 residents. Multiplying by the average county population of 92,029 and dividing by 100,000, we estimate that there are 10.77 fewer aggravated assaults on average per year in counties with sanctuary policies relative to counties without sanctuary policies. Using cost estimates from McCollister et al. (2010), this translates into a total decreased cost of $1,149,279 per county per year (95% confidence interval equal to $182,778–$2,481,337). Given that there are 88 counties in our sample that adopt an SJ policy in 2014, this becomes a total avoided cost of approximately $101 million per year.

We also examine the impact of SJ policies from an alternative perspective. SJ policies are responses to a federal ICE detainer policy. Thus, we can examine the additional local cost imposed by ICE detainer policies in locations that do not adopt SJ policies, or counties that choose not to offer sanctuary. Our results suggest that the average county would save $1.15 million per year in avoided violent crime costs by passing a sanctuary policy. If ICE no longer issues detainer requests so that all jurisdictions effectively become SJs, this would save a total of $3.28 billion per year across the 2940 non-SJ counties in our sample. Interpreted this way, the ICE detainer policy costs local jurisdictions $3.28 billion in additional crime costs per year. This back-of-the-envelope calculation assumes the average effect of locally adopted SJ policies applies across all counties in our analysis. In reality, ICE cancellation of detainer policies may not be equivalent to implementation of a local SJ policy. For example, SJ policies may include explicit communication with immigrant communities. Our results demonstrate that state policies do not have the same effect. Yet, cancellation of the federal policy could increase trust in law enforcement by even more. Therefore, we view this exercise as illustrative and not a prediction of the impacts of changes to ICE policy.

Of course, focusing on costs may ignore outcomes of ICE's detainer policy. Therefore, we explore what ICE produces through its detainer policy, relative to its crime cost. According to TRAC Immigration (2017), ICE removed an average of 18,022 immigrants per year through detainer requests from 2013 to 2016. On average, this means that the ICE detainer policy produced crime costs of $182,000 ($3.28 billion/18,022) per immigrant removed from the country. This cost represents a lower bound on costs to society because it ignores federal ICE, court, and deportation costs, as well as the loss of additional economic benefits that accrue because of the presence of migrants in a local economy (Jaumotte et al., 2016). Finally, if some deportees were (or could have been) removed through other means, then fewer additional deportations were achieved only as a result of detainers. This would also increase the cost per deportation because of a detainer.

A caveat to our results is that our crime data consist of reported crimes, and therefore our results depend on the quality of reporting. If an SJ policy increases the frequency of reported crimes, then our estimates may provide lower bounds of the true impact of SJ policies on violent crime rates. Alternatively, if SJ policies increase immigrant populations, which may be less likely to report crimes (Gutierrez & Kirk, 2017), then our results could overstate impacts.

While the latter is a possibility, to our knowledge, there is no evidence that sanctuary policies led to population changes on a scale that would substantially move crime rates per population (regression results modeling population changes suggest no impact of SJ policies on county population: see Table 9 in the Supporting Information S1). Therefore, our results provide strong evidence that sanctuary policies led to a decrease in violent crime, with our coefficient estimates representing lower bounds of the true impact.

Overall, our research provides a valuable contribution as the first rigorous attempt to establish the causal impact of SJ policies on local crime costs. It opens many questions about the mechanisms through which the policy works. Disentangling these mechanisms provides a fruitful area for future work on immigration policy and crime.

ACKNOWLEDGMENT

We thank Maggie Haynes for important research assistance.

ENDNOTES

[1] https://www.fbi.gov/services/cjis/ucr

[2] These cost estimates come from estimates of the total cost of an aggravated assault from McCollister et al. (2010): $107,020 per assault.

[3] http://www.msnbc.com/specials/migrant-crisis/sanctuary-cities

[4] https://www.prisonlegalnews.org/news/2016/nov/8/local-jails-increasingly-refuse-comply-ice-detainers/

[5] The 88 counties and four states that adopted in 2014 represent 94% of the 250 counties that have adopted (https://cis.org/Map-Sanctuary-Cities-Counties-and-States).

[6] Maltz and Targonski (2002) show there are two primary issues with the aggregated UCR data. First, the population numbers are incorrect. Some counties or police precincts have zero populations because they want to avoid double counting when precincts overlap within counties. In other circumstances, multiple populations are reported within counties resulting in double counting of populations (e.g., university campuses and municipality police precincts). In our analyses, we use population data from the Census. We do not use population data from the UCR. Second, the FBI and Michigan's National Archive of Criminal Justice Data (NACJD) impute missing crime rates using several potentially flawed methods. We acquired the raw monthly UCR data from the FBI, not the NACJD. We requested the data by emailing crimestatsinfo@fbi.gov. In our data cleaning procedure, we did not use the variable that is used to impute missing crime counts, which is known to be incorrect (the number of months reported). This variable does not show up in the raw monthly data from the FBI. Instead, we dropped counties that only reported violent or property crimes once a year, counties that only reported violent or property crimes twice a year (e.g., in June and December), and counties that only reported violent or property crimes quarterly. We also dropped counties with missing values for all months of the year. We did not replace missing values with zeros. We then aggregated the remaining monthly data to the annual level. While no dataset is perfect, our analysis obviates the criticisms of Maltz and Targonski (2002). However, we must still caveat our findings with regard to imperfections in the data.

[7] In robustness checks, we extend the control variable data to 2008 by applying 2010 demographics to 2008 and 2009 data. The results are qualitatively similar and remain statistically significant.

[8] http://www.ailadownloads.org/advo/NILC-LocalLawsResolutionsAndPoliciesLimitingImmEnforcement.pdf

[9] Section 6 describes the need for control variables and shows robustness to the choice of controls.

[10] While many SJ policies are adopted at the county level, several states also adopt SJ policies. Therefore, to be conservative we cluster standard errors by state.

[11] We note that the parallel trends assumption can never be fully tested.

[12] For instance, Chesterfield County, Virginia adopted an SJ policy in October 2014. It is unlikely that a court in Oregon decided a ruling based on what took place in Chesterfield County, Virginia prior to 2014.

[13] Specifically, these are regressions with one year of data, either 2013 or 2014. The dependent variable in each model is the adoption dummy, SJ_c. The control variables are the variables described in the Data section.

[14] To be clear, we estimate the model: $c_{ct}^j = \alpha_c^j + \gamma_t^j + X_{ct}'\beta^j + SJ_{ct}\delta^j + \epsilon_{ct}^j$, which is identical to Equation 1 except that the variable SJ_{ct} is not multiplied by a year dummy. This specification is akin to a traditional difference-in-differences model in which SJ_{ct} is the interaction between a treatment dummy and a before and after dummy, where before and after are defined as before and after 2014.

[15] Murder, robbery, rape, and aggravated assault are not the only constituents of violent crime but they constitute the majority.

[16] https://www.rollcall.com/news/policy/new-jersey-police-seek-immigrants-trust-get-pushback-from-ice

[17] The results are robust to matching on violent crime rates from 2013.

[18] For the interactions that are time varying, we interact the treatment dummy with the contemporaneous value of each variable.

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Sanctuary Cities and Their Respective Effect on Crime Rates

Adam R. Schutt
Minnesota State University Moorhead, arschutt@gmail.com

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Sanctuary Cities and Their Respective Effect on Crime Rates

Abstract

According to the U.S. Center for Immigration Studies (2017), cities or counties in twenty-four states declare themselves as a place of "sanctuary" for illegal immigrants. This study addresses the following question: Do sanctuary cities experience higher crime rates than those cities that are not? Using publicly available data, this regression analysis investigates the relationship between crime rates in selected cities and independent variables which the research literature or the media has linked to criminal activity. Results of this research reveal that sanctuary cities do not experience higher violent or property crime rates than those cities that are not sanctuary cities.

Keywords

sanctuary cities, violent crime, property crime, immigration

Cover Page Footnote

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I. Introduction

In 2019, both major political parties in the U.S., as well as prominent political figures, continue to debate the creation and/or implementation of sanctuary cities in the U.S. and their effect on those respective cities. Conservatives often declare that sanctuary cities provide a "breeding place" for violent crime (Luhby, 2016), resulting in higher instances of aggravated assault, rape and murder. Liberals, on the other hand, argue that sanctuary cities do not create an environment for more crime, but that they protect those individuals who entered the U.S. illegally in the past from deportation. While this may appear to be a clear-cut topic and simple for people to choose one side or the other, the discussion becomes much more complex as we attempt to define sanctuary cities and determine what being a "sanctuary city" means.

Sanctuary cities' prominence grew during the 2016 U.S. Presidential election amidst the increasing popularity of then-candidate, Donald J. Trump. Sanctuary cities became a popular topic of discussion during primary debates, and many candidates, including Trump, proposed the idea of cutting federal funding to those states and/or jurisdictions that chose to defy federal law to protect undocumented immigrants. According to candidate Trump on August 29th, 2016, "Block funding for sanctuary cities … no more funding. We will end the sanctuary cities that have resulted in so many needless deaths. Cities that refuse to cooperate with federal authorities will not receive taxpayer dollars, and we will work with Congress to pass legislation to protect those jurisdictions that do assist federal authorities" (Luhby, 2016). After candidate Trump was elected President in 2016, sanctuary cities remained an important issue for him and his political base. While President Trump and his political base continue to advocate for sanctuary cities to be eliminated and their federal funding withheld to some extent, many people remain uninformed about sanctuary cities and uncertain of their role in the U.S. moving forward.

Most people do not realize that sanctuary cities actually became popular in the U.S. in the 1980s. During this decade, numerous immigrants in Central America fled from harsh and violent living conditions in countries such as El Salvador and Guatemala. As these immigrants fled to the U.S., churches and synagogues would often provide some sort of refuge or shelter to these undocumented immigrants. "The Sanctuary Movement encompassed a number of religious and faith-based groups around the country, with additional support coming from university campuses, civil rights organizations, lawyers, and a host of other concerned parties" (Gonzalez et al., 2017). At the height of the sanctuary movement, approximately 20,000 to 30,000 church members and more than 100 churches and synagogues participated. This movement was then followed by what some termed the "New Sanctuary Movement" following the September 11, 2001 attacks in the U.S. Some of the new policies following these attacks, including the

U.S. Patriot Act (2001) and the Clear Law Enforcement for Criminal Alien Removal Act (2003), led some to believe that the government was abusing its power by requiring local law officials to enforce federal immigration law. Alternative definitions of sanctuary cities, as evidenced by the prior definitions, make it difficult to provide one single, consistent definition of a sanctuary city over time.

Although we often refer to places that provide some type of protection to immigrants as "sanctuary cities," these locations are not technically always cities. Not only cities provide "sanctuary" to immigrants; other jurisdictions, including entire states and counties, serve as places of "sanctuary" and do not completely comply with the U.S. Immigration and Customs Enforcement's (ICE's) demands. As the Center for Immigration Studies' article *Maps: Sanctuary Cities, Counties, and States* (2017) explains, these cities, counties and states support laws, ordinances, regulations, resolutions, policies or other practices that block immigration enforcement and shield criminals from ICE. These entities refuse to or prohibit agencies from complying with ICE detainers, imposing unreasonable conditions on detainer acceptance, denying ICE access to interview incarcerated aliens or otherwise impeding communication or information exchanges between their personnel and federal immigration officers. Thus, the definition of sanctuary cities and how those cities choose to provide "sanctuary" is multifaceted.

This research considers if labeling a city as a sanctuary city results in higher violent and/or property crime rates for that city when compared to those cities that do not identify as sanctuary cities. Since the lines are slightly blurred when it comes to either cities or counties identifying as an area that provides "sanctuary," I incorporate only cities that define themselves individually as sanctuary cities in this study. I exclude cities within counties that had, for example, their sheriff's office decide that they would provide some sort of "sanctuary" to immigrants.

A review of the literature reveals only a limited amount of research on the relationship between sanctuary cities and crime. One study titled *The Politics of Refuge: Sanctuary Cities, Crime, and Undocumented Immigration* by Gonzalez, Collingwood and Omar El-Khatib (2017) exhibits the greatest amount of similarity with my study. Their study is explained in greater detail in the literature review section, but it is important to note that a few significant differences exist between this study and the one completed by Gonzalez, Collingwood and Omar El-Khatib. This study includes other economic variables, such as income and education, while Gonzalez, Collingwood and Khatib do not. Since the omission of these variables could result in omitted variable bias, I include these socioeconomic variables. Another main difference is the results; Gonzalez et al. analyze different types of violent crime separately, while I analyze the effect of these independent variables on both violent and property crime rates. Although

this was the only study I identified that uses a simple linear regression model to observe the relationship between sanctuary cities and crime, I review several other papers written on the topic of immigration and crime.

My hypothesis for this study is that cities defined as sanctuary cities will not, on average, experience higher violent crime or property crime rates than those cities that are not defined as sanctuary cities. I expect the independent dummy variable (1 if sanctuary city, 0 if not sanctuary city) to be statistically insignificant when it comes to explaining the two dependent variables (property crime rates and violent crime rates) in respective cities. This is the same result as the previous study completed by Gonzalez, Collingwood and Khatib. In the next section, I review the literature related to this topic. The remainder of this paper is ordered as follows: theory, data, results and the conclusion of the study.

Review of Literature

My interest in studying the relationship between sanctuary cities and violent/property crime stems from the fact that limited research exists despite the fact that this topic has been discussed since sanctuary cities emerged in the 1980s. *The Politics of Refuge: Sanctuary Cities, Crime, and Undocumented Immigration* by Gonzalez et al. (2017) is the only study I found that is closely related to this research. Their research addresses the claim that sanctuary cities, defined as cities that expressly forbid city officials or police departments from inquiring into an individual's immigration status, are associated with post hoc increases in crime. My research addresses the same issue: whether cities that define themselves as sanctuary cities experience higher crime rates than those that do not. Their findings provide evidence that sanctuary policies in cities have no effect on crime rates, despite narratives to the contrary portrayed in mass media. They also determine from this result that the potential benefits generated in sanctuary cities, such as better incorporation of the undocumented immigrant community and cooperation with police, result in limited cost for the cities in question in terms of crime. This statement suggests that the possible added benefits that may come from sanctuary cities are increasingly likely to be "more" positive because the evidence does not in any way suggest that sanctuary cities increase violent crime rates.

Although these studies share the same general objective and hypotheses, some important differences exist between the two studies. In my study, I select cities randomly and include some that identify as "sanctuary" and some that do not. In the study by Gonzalez, Collingwood and Khatib (2017), they, "employ a casual inference matching strategy to compare similarly situated cities where key variables are the same across the cities except the sanctuary status of the city." Unlike my study, these researchers do not select cities randomly to compare respective crime rates across different cities. Rather, they choose cities that are "similarly situated" to compare a sanctuary city to another similar city

geographically that is not a sanctuary city. Another key difference from the two studies is that Gonzalez, Collingwood and Khatib investigate each type (violent, property or rape) of crime separately. They then compare those results individually across cities that identify as sanctuary cities and those that do not. My study, on the other hand, analyzes the two separate classifications of crime as two different dependent variables [violent (which includes murder/nonnegligent manslaughter, rape, robbery and aggravated assault) and property (which includes burglary, larceny-theft, motor vehicle theft and arson)] and then compares the regression results associated with the two different classifications of crime. Another key difference between the two studies emerges since Gonzalez, Collingwood and Khatib use both time series and cross-sectional approaches within their study. They use crime data from fifty-five cities that passed sanctuary city laws post-9/11 and compare these crime data with the crime rate in the year preceding the implementation of a sanctuary policy. By doing this, they determine whether sanctuary cities themselves experience an increase in their respective crime rates by comparing the crime rate of the year before they identified as sanctuary cities to the year after identification. Their second analysis employs a "matching causal inference strategy to test the claim that sanctuary cities are associated with more crime than are non-sanctuary cities." My study resembles this part of their study.

In the first part of Gonzalez et al.'s study where they compare crime rates the year before cities became sanctuary cities to the year after, they detect no statistical evidence to suggest that sanctuary cities experience higher crime rates. In the second part of their study in which they compare crime in non-sanctuary cities to crime in sanctuary cities, they also find no statistical evidence that sanctuary cities experience higher crime rates than those cities that do not identify as sanctuary cities. My regression finds no statistically significant correlation between sanctuary cities and crime as well.

Although Gonzalez et al. is the only paper identified in which the author uses regression analysis to analyze the relationship between sanctuary cities and crime, a significant amount of research exists for immigration and crime in general. Camarota and Vaughan (2009) examine academic and government research in the context of the following question: do immigrants, on average, commit more crime than citizens? Although this is slightly different than my research question, I can infer that immigration and sanctuary cities are both intimately related, and therefore reviewing literature on the effects of immigration on crime provides insights as to how sanctuary cities may affect crime. Ultimately, Camarota and Vaughan conclude that there is very little conclusive data to inform the well-entrenched views on *both* sides of the debate. Instead, they highlight that the collection and measurement of illegal immigration data are the greatest challenges to conducting research on this topic.

Camarota and Vaughan identify that, except for federal prisons which only account for a small number of all those incarcerated, state and local correctional institutions generally have not tried to carefully determine whether their prisoners are native or foreign-born. As the report states, many people do not realize that state and local prisons will typically use a "self-reporting" technique when determining whether the inmates were born in the U.S. or were foreign-born. Self-reporting means that they ask the inmates whether they were born in the U.S. or not and take their word for it when collecting these data. However, this introduces its own challenges when relying on self-reported data when conducting research. Smart inmates, who realize that being a non-citizen can lead to deportation, face a much stronger incentive to lie when asked whether they are a citizen or foreign-born. Because of this, the Census or any surveys administered in jails or prisons likely understate the share of inmates who are non-citizens or illegal aliens if the inmate data are not carefully checked against actual immigration records. While one may think that verifying immigration records may address this problem, multiple issues surface in that step of the process as well. No official list of legal U.S. residents or illegal aliens exists. In theory, if someone enters the country illegally and has no prior contact with immigration officials, that person's fingerprints and other information are not present in any immigration database. These issues identified by Camarota and Vaughan make it extremely difficult to measure an exact number of crimes or rates of crimes committed independently by foreign-born individuals with accuracy or confidence. This information is important to this study as well, as I use non-citizen/foreign-born data provided by the U.S. Census Bureau's American Community Survey. As Camarota and Vaughn emphasize, it is difficult to determine an accurate number of foreign-born individuals based upon a self-reporting system, as many illegal immigrants may feel as though the incentives favor lying about their immigration status versus providing the truth to law enforcement officials.

Another study of immigration and crime titled *More Foreigners, Less Crime: Examining the Relationship between Immigrant Inflow and County Crime Rates in 2000* by Gonzalez (2006) addresses the concern that many Americans had back in the early 2000s: are immigrants and foreigners more criminal than native-born citizens? To answer this question, Gonzalez uses 1990 and 2000 U.S. Census and FBI Uniform Crime Report data to analyze the relationship between foreign-born people in the U.S. and crime rates. Gonzalez examines the changes in crime and immigration rates from 1990 to 2000, the nation's immigrant-crime link during the 2000s and the immigrant-crime link in counties that experienced an increase in their foreign-born population. This study relies upon data from the Federal Bureau of Investigation's (FBI) Uniform Crime Report (UCR) and the U.S. Census Bureau's Decennial Reports. The UCR data from the FBI serve as the source of Gonzalez's independent variables of total crime, property crime and

violent crimes, while the U.S. Census Bureau's Decennial Reports offer self-reported data for the independent variables including: population size, mean age, proportion of males in a county, proportion of black people in a county, proportion of Hispanic people in a county, mean education level, proportion of population living in poverty, proportion of county population that is unemployed, proportion of county that is foreign-born, proportion of people in a county that are new immigrants and proportion of people in a county that are established immigrants.

After running the regression analysis with all the variables mentioned, Gonzalez finds that immigrants are less violent than native-born Americans and that communities with greater proportions of immigrants exhibit lower crime rates. The author reaffirms previous researchers' findings that immigrants are less violent than native-born citizens and that, when it comes to creating immigration policies and addressing crime, closing the nation's borders and removing immigrants is not a practical or reasonable solution. This result contradicts some of the mainstream ideas in our nation today, but it is consistent with most of the existing research on this topic. Although Gonzalez's study explores the link between immigration and crime instead of sanctuary cities and crime like my study, I find this research to be relevant to both topics because Gonzalez used FBI crime data and many of the same economic and demographic variables used in my study.

Similar to Gonzalez's work, other researchers have conducted studies to determine if immigration increases crime in U.S. cities. *Does Immigration Increase Homicide? Negative Evidence from Three Border Cities* (Lee et al., 2001) is another example of an attempt by researchers to understand the complex relationship between homicide and immigration. These researchers compare three U.S. border cities (Miami, El Paso and San Diego) in order to determine if an increase in immigration increases the homicide rate. Lee et al. highlight some of the "sociological images" of immigrants and crime that have led some individuals to believe that immigrants are more prone to committing violent crimes than others. Based upon opportunity structure theory, some expect that immigrants are more likely to commit violent crimes simply because legitimate opportunities for wealth and social status are not equally available to all groups. Because of this fact, some immigrants "innovate" by taking advantage of available illegitimate opportunities. Racism and discrimination may make it difficult for immigrants to obtain higher education or secure a stable job, which as a result makes it difficult for them to achieve "culturally prescribed success and goals," otherwise known by some in the U.S. as the "American Dream" or a typical "middle-class lifestyle." Even though this sociological theory makes theoretical sense, Lee et al. find that the cities of Miami, El Paso and San Diego do not experience increases in homicide rates as a result of the increase in immigration to these cities,

respectively. This study relates closely to mine, as these researchers attempt to conclude if immigrants possess greater incentives than natural-born citizens to commit homicide.

One final paper that I find to be important regarding the topic of crime and immigration is one titled *Immigration and Crime: What's the Connection?* (Mears, 2002). Unlike the papers reviewed thus far, Mears analyzes the link between immigration and crime in a way that incorporates topics from psychology and sociology instead of only using data and regression analysis to explain the link between the two. Similar to the papers reviewed previously, Mears states that immigrants are less, not more, criminal than non-immigrants. Mears also suggests that immigration rates are largely unassociated with crime rates. This article discusses current research on the immigration-crime connection and also identifies key issues relevant to understanding both the limitations of existing data/studies and opportunities for future research.

Mears points out that, despite almost a century of research attempting to explain a possible connection between immigration and crime, well-developed theoretical and empirical studies remain rare. From a psychological/sociological standpoint, theory would in some ways suggest that immigrants should be more prone to engagement in criminal behavior than non-immigrants. Mears mentions the idea of social disorganization theory. This theory advocates that in highly disorganized areas, with high rates of residential mobility, residents do not develop a sense of shared values and thus become more likely to engage in crimes. Two other notable sociology theories, known as social strain theory and opportunity theory, suggest that individuals who face few prospects to achieve social goals legitimately turn to illegitimate means, such as criminal behavior, to do so.

The sociological and psychological theories mentioned suggest that immigrants are more likely to be involved in crime than natural-born citizens. As Mears points out, immigrants typically move into areas thought to be more highly disorganized, and they face many more cultural and social barriers as they attempt to assimilate into U.S. society. Further, these factors suggest that immigrants are more likely to involve themselves in different types of crime. However, the research completed over past years does not provide support for these theories. Mears suggests that, contrary to popular belief, most studies offer a more paradoxical finding than what many expected; immigrants are less likely to engage in crime than those who are natural-born citizens.

Although limited literature exists in which researchers study the relationship between sanctuary cities and crime, a significant amount of research on the topic of immigration and crime exists. Presuming that some, if not a majority, of those individuals in sanctuary cities that these entities are trying to protect are indeed immigrants, previous literature and studies completed on

immigration and crime provide valuable insights in relation to sanctuary city policies and their effects on crime rates.

Theory

Literature reviewed in the previous section highlighted the dependent and independent variables used in this research; I relied especially upon the study completed by Gonzalez et al. (2017) to decide which variables would be important to include in this study. Table 1 summarizes the two alternative dependent variables (violent crime rate and property crime rate) and seven independent variables included in this study with detailed explanations of each offered to conclude this section.

The violent crime rate and property crime rate each serve as separate dependent variables for the respective cities studied. I obtain these data for 2016 from the FBI's website. To obtain the actual crime rates, I take the number of violent crimes reported in the cities chosen for my research and divide each city's number of violent crimes by its respective population. I then repeat this same method with property crimes in the same cities to obtain a similar ratio. This provides the number of violent and property crimes per person in each city. I multiply each result by one thousand to define the two dependent variables as the number of violent crimes per one thousand people in each respective city, and the number of property crimes per one thousand people in each respective city. In the following section, I explain the expected signs for each of the seven independent variables used in this study. It is also important to note that use of the word "crime" refers to both violent and property crimes. I anticipate the same expected signs, irrespective of whether the dependent variable is measured as violent crime or property crime.

The inclusion of the percentage of Hispanic/Latino individuals in each city originates from the idea that when most people think of sanctuary cities, they think of immigrants entering the U.S. from Mexico. These data were obtained from the U.S. Census Bureau's American Community Survey for the year 2016. Based on previous literature, I expect this coefficient to be negative, suggesting that as the percentage of Hispanic individuals rises in a given city, the crime rate decreases. I base this theory upon Gonzalez's (2006) finding that natural-born citizens commit more crimes than foreign-born immigrants. Economic theory suggests that illegal immigrants attempt to avoid crime as much as possible, since they are in the country illegally and prefer to remain undetected. Illegal immigrants have more to lose if caught committing a crime, and therefore are less inclined to commit crimes than those who are native-born.

Table 1.
Description of dependent and independent variables

Data	**Description**	**Source**
Violent crime rate	(number of violent crimes)/(city population)*1000	Federal Bureau of Investigation (2016)
Property crime rate	(number of property crimes)/(city population)*1000	Federal Bureau of Investigation (2016)
Percentage Hispanic/Latino (%HISP)	(number of Hispanic individuals)/(city population)	U.S. Census Bureau American Community Survey (2016)
Percentage foreign-born (%FB)	(number of foreign-born individuals)/(city population)	U.S. Census Bureau American Community Survey (2016)
Median age (MEDIANAGE)	Median age of both females and males in city	U.S. Census Bureau American Community Survey (2016)
Percentage of population over age 25 with less than high school diploma (%NOHSDIP)	(number of individuals in city over age 25 with less than high school diploma)/(total population of city over age 25)	U.S. Census Bureau American Community Survey (2016)
Median household income (MHI)	Median income for entire household in city	U.S. Census Bureau American Community Survey (2016)
Percentage of non-citizens (%NONCIT)	(number of non-citizens)/(city population)	U.S. Census Bureau American Community Survey (2016)
Sanctuary city (dummy variable) (SANCCITY)	City denoted by "1" if sanctuary city, denoted by "0" if not a sanctuary city	Center for Immigration Studies (2017)

I include the percentage of foreign-born individuals in each city variable because of previous studies conducted on illegal immigration and crime; these studies suggest that foreign-born individuals are less likely to commit crimes, on average, when compared to natural-born citizens. Thus, I assume this variable exhibits a negative correlation with respect to crime rates. As the percentage of foreign-born individuals rises in a city, I expect the crime rate to decrease since foreign-born individuals have more to lose when committing a violent crime compared to natural-born citizens.

I predict that the average age of the population in each city will have a negative correlation with respect to crime rates; as the average age increases in a given city, I believe the crime rate will decrease. Based upon economic theory, the incentives will be much higher for a younger person to commit a crime than an older person. I expect that younger people are more likely to be involved with criminal activity since they are less developed and mature than someone who is older and has a more-established life. Similarly, they may have more of an incentive to commit a property crime. If they are younger, and therefore presumably less established and financially sound, these younger individuals have more of an incentive to commit burglary or theft. Although research on the topic of crime and age has been minimal, much of the research does suggest that, on average, people commit less crimes as they age. Although there is common consensus overall, some research suggests that, although crime *as a whole* tends to decrease with age, different classifications or offenses of crime "peak" at various ages. "Traditional research and theory on the age-crime relationship suggest some of the patterns that may be expected: (1) most crimes peak in adolescence or early adulthood, then decline fairly steadily; (2) crime types vary in peak ages of criminality and in rates of decline from the peak; (3) because of the effect of industrialization, peak ages have become younger over the past four decades, and the descent of the age curve from the peak has become steeper" (Steffensmeier et al., 1989). In their research, Steffensmeier et al. suggest that dissimilar crimes have different "peak ages." This presents the idea that, although most studies still may accurately claim that "crime" in the general sense is committed more frequently by young people on average, other research shows that separate crimes may have different peak ages.

The percentage of the population over the age of twenty-five in each respective city that has completed less than a high school education includes individuals who both did not attend high school and those who attended high school for a period of time but did not graduate high school nor earn their diploma and/or General Educational Diploma (GED). These were obtained from the U.S. Census Bureau's American Community Survey for the year 2016. I predict this variable to have a positive correlation with respect to crime rates; as the percentage of those with less than a high school education increases in a

respective city, one could conclude that the crime rate in that city will increase as well. Theory states that those who are more educated are less likely to participate in criminal activities and tend to avoid a lifestyle involving crime. As one becomes more educated and earns a higher income, one has much more to lose than a less-educated individual when it comes to deciding whether to commit a crime or not.

The median household income in each city variable is expected to display a negative correlation; as the median household income increases in a given city, I predict a decrease in the crime rate. The more money a person makes, the more financially stable they are. Therefore, I expect that the incentives to participate in criminal activities as a wealthier person decline.

I do not have a prediction as to whether the percentage of non-citizens in each city variable exhibits a positive or negative correlation with the crime rate. I assume that this variable will be statistically insignificant and result in no significant impact on crime rates in the given cities. Based upon the recent rhetoric in our nation suggesting that immigrants/non-citizens tend to be more likely to engage in criminal activities than citizens, this is a timely variable to include in my study.

I include a dummy variable for sanctuary cities in this study. If a given city is defined as a sanctuary city, that city is denoted by a "1." If a given city in this study is not defined as a sanctuary city, that city is denoted by a "0." Relative to my objective, this is the variable of greatest interest in my study. I do not have a prediction as to whether the coefficient for this dummy variable will display a negative or positive correlation. I do expect, however, that the dummy variable for sanctuary cities will be statistically insignificant, which would suggest that sanctuary cities do not experience higher crime rates (neither property nor violent) than non-sanctuary cities.

Data

I obtained 2016 crime rate data from the FBI's official website for 147 U.S. cities. As part of the FBI's National Incident-Based Reporting system, these statistics include all crimes occurring in each city reported to law enforcement. The definition of "violent" crimes for the first dependent variable includes the cumulative number of the following crimes committed in each city during 2016: murder/nonnegligent manslaughter, rape (includes the revised and legacy definitions), robbery and aggravated assault. To obtain the violent crime rate in each of the selected cities, the number of violent crimes in each city is divided by that city's total population. This relatively small decimal represents the average number of violent crimes per person in a city. The decision to multiply each result by one-thousand results in a number that represents the average number of violent crimes per one-thousand people in each city, respectively.

For the second dependent variable used, property crime rates, I also use 2016 crime rate data from the FBI's official website for the same 147 U.S. cities. As part of the FBI's National Incident-Based Reporting system, these statistics include all property crimes occurring in each city reported to law enforcement. The definition of "property" crimes for the second dependent variable includes the cumulative number of the following crimes committed in each city throughout 2016: burglary, larceny-theft, motor vehicle theft and arson. To obtain the property crime rate in each of the selected cities, the number of property crimes in each city is divided by that city's total population (the same population used for the violent crime dependent variable). This small decimal represents the average number of property crimes per person in a city. The decision to multiply each result by one thousand provides a number that represents the average number of property crimes per one-thousand people in each city, respectively.

Some of the descriptive statistics for the dependent variables (violent crime rate per one-thousand people and property crime rates per one-thousand people) are highlighted for greater understanding of the cities (Appendix A) used within this study. For violent crimes, the mean is 6.18 violent crimes per one-thousand people, and some of the cities that are close to this mean value include: Bristol, Tennessee; Seattle, Washington; Annapolis, Maryland; and Joplin, Missouri. The median is Glendale, Arizona with 4.96 violent crimes per one-thousand people. The city with the lowest violent crime rate per one-thousand people is Lafayette, Louisiana with 0.005 violent crimes, and the city with the highest violent crime rate per one-thousand people is Detroit, Michigan with 20.47 violent crimes. For property crimes, the mean is 36.43 property crimes per one-thousand people, and cities with property crime rates close to this mean value are: Albany, New York; Denver, Colorado; Savannah, Georgia; and Tuscaloosa, Alabama. The city with the lowest property crime rate per one-thousand people is Independence, Kentucky with 5.66 property crimes, and the city with the highest property crime rate per one-thousand people is Myrtle Beach, South Carolina with 137.93 property crimes.

Aside from summarizing the descriptive statistics of the two dependent variables used in this study, I also highlight some independent variables with interesting descriptive statistics in this section. The median age in each city and the median household income in each city are the two independent variables that are statistically significant, irrespective of whether crime is measured as damage to person or property. That being said, I want to compare the descriptive statistics for both of these independent variables between non-sanctuary cities and sanctuary cities to identify if the descriptive statistics are similar across cities that identify as "sanctuary" and those that do not.

When reviewing the descriptive statistics for median household income in each city, the mean household income for non-sanctuary cities used in this study

is approximately $51,700. On the other hand, the mean household income for sanctuary cities used in this study is approximately $57,974. The minimum and maximum household incomes for non-sanctuary cities are $27,551 and $120,246, respectively. The minimum and maximum household incomes for sanctuary cities are $27,577 and $123,326, respectively. As shown by the mean, minimum and maximum household incomes compared between sanctuary cities and non-sanctuary cities, there is not a significant difference between the incomes.

When reviewing the descriptive statistics for average age in each city, the mean age for non-sanctuary cities used in this study is approximately 35 years old. On the other hand, the mean age for sanctuary cities used in this study is 33 years old. The minimum and maximum median ages for non-sanctuary cities are 23.3 and 46.7, respectively. The minimum and maximum median ages for sanctuary cities are 20.5 and 45, respectively. It is important to note that these descriptive statistics are relatively similar, even when separating the cities based upon their sanctuary city or non-sanctuary city distinction. Results in response to the question, "Do sanctuary cities experience higher violent and property crime rates than non-sanctuary cities?" comprise the next section.

Results

The results section includes two subsections. In the first subsection, the violent crime rate serves as the dependent variable while the property crime rate is the dependent variable in the second subsection.

Regression I: Violent Crime Rate

Using the violent crime rate as the dependent variable, the initial regression is described by Equation 1.

Equation 1.

$$Violent\ Crime\ Rate = 3.254 + 18.489(\%FB) - (9.405)(\%HISP) + (0.132)(MEDAGE) + (30.400)(\%NOHSDIP) - (0.0001)(MHI) - (30.142)(\%NONCIT) + (0.493)(SANCCITY)$$

Associated regression results are summarized in Table 2.

Table 2.
Regression I (Violent Crime Rate)

Variable	Coef.	Std. Err.	t	P>t
ForeignBorn	18.489	12.098	1.53	0.129
Hispanic2016	-9.405	3.241	-2.90	**0.004**
MedianAge2016	0.132	0.081	1.64	0.103
NoHSDiploma2016	30.400	8.561	3.55	**0.001**
MedianHouseholdIncome2016	-0.0001	0.0001	-3.08	**0.002**
SanctuaryCity1or0	0.493	1.008	0.49	0.626
NonCitizen2016	-30.142	20.894	-1.44	0.151
_cons	3.254	2.849	1.14	0.255

Three variables are significant at the one-percent level of significance: the percentage of Hispanic individuals in each city (p-value of 0.004), the percentage of the population over the age of twenty-five in each city with less than a high school diploma education level (p-value of 0.001) and the median household income of each city (p-value of 0.002). However, not all variables are statistically significant. The following four variables in this regression are not statistically significant even at the ten-percent level of significance: the percentage of foreign-born individuals in each city, the median age in each city, the dummy variable of either a sanctuary or non-sanctuary city and the percentage of non-citizens in each city.

With an F-statistic value of 8.06, the initial regression equation is statistically significant. The R-squared value for this regression is 0.2888, meaning that the initial linear model explains 28.82 percent of the variation in the violent crime rate. A summary of additional tests conducted follows.

After completing a Breusch-Pagan/Cook-Weisberg test to determine if this regression exhibits any heteroskedasticity, I find a chi-squared test statistic equal to 11.71. The chi-squared value at a ten-percent level of significance (with seven degrees of freedom) equals 12.02, and the chi-squared value at a five-percent level of significance (with seven degrees of freedom) equals 14.07. Because the test statistic (11.71) is less than both 12.02 at the ten-percent level of significance and 14.07 at the five-percent level or significance, I conclude that heteroskedasticity is unlikely.

In addition to testing for heteroskedasticity, I examine variance inflation factors to determine if Regression I exhibits multicollinearity, as shown in Table 3.

Table 3.
Variance inflation factors – Regression I (Violent Crime Rate)

Variable	VIF	1/VIF
NonCitizen2016	16.32	0.061
ForeignBorn2016	16.11	0.062
NoHSDiploma2016	3.62	0.276
MedianHouseholdIncome2016	1.94	0.514
Hispanic2016	3.30	0.303
SanctuaryCity (0 or 1)	1.44	0.692
MedianAge2016	1.27	0.787

Two variables exhibit a VIF greater than five, highlighting the presence of multicollinearity within the regression. The percentage of non-citizens in each city and foreign-born individuals in each city variables generate VIF values of 16.32 and 16.11, respectively. Based on the possible relationship of these two variables to one another, the detection of multicollinearity is not surprising. To correct this issue, I remove the percentage of non-citizens variable.

Regression II: Violent Crime Rate (omitting non-citizens variable)

Regression II uses the initial regression but omits the non-citizens variable as described by Equation 2.

Equation 2.
Violent Crime Rate = 2.143 + 2.653(%FB) – 9.856(%HISP) + 0.168(MEDAGE) + 28.431(%NOHSDIP) - 0.0001(MHI) + 0.734(SANCCITY)

Associated regression results are summarized in Table 4.

Table 4.
Regression II (Violent Crime Rate)

Variable	Coef.	Std. Err.	t	P>t
ForeignBorn	2.653	5.105	0.52	0.604
Hispanic2016	-9.856	3.239	-3.04	**0.003**
MedianAge2016	0.168	0.077	2.19	**0.030**
NoHSDiploma2016	28.431	8.484	3.35	**0.001**
MedianHouseholdIncome2016	-0.0001	0.0001	-3.14	**0.002**
SanctuaryCity1or0	0.734	0.998	0.74	0.463
_cons	2.143	2.754	0.78	0.438

In regression II, the only coefficient that changes in a noticeable manner is the one associated with the variable percentage of foreign-born individuals; this coefficient changes from 18.489 in the Regression I to 2.653 in the Regression II. Although the coefficient changes, the variable remains statistically insignificant in terms of explaining changes in each city's violent crime rate.

All variables that were significant/insignificant in Regression I remain significant/insignificant in Regression II, with the exception of the median age in each city which becomes significant in Regression II. The following independent variables are statistically significant at the one-percent level in the second regression with violent crimes as the dependent variable: the percentage of Hispanic individuals in each city, the percentage of the population over the age of twenty-five with less than a high school diploma and the median household income in each city. The median age in each city variable is significant at the five-percent level. Two variables (the percentage of foreign-born individuals in each city and whether the city is defined as a sanctuary city or not) are not significant at the five percent level.

The F-statistic for Regression II (8.99) exceeds the associated F-statistic, conveying that this regression equation is also significant. The R-squared value for Regression II is 0.2781, meaning that this particular linear model explains 27.81 percent of the variation in the violent crime rate.

Table 5 displays variance inflation factors (VIF) associated with Regression II.

Table 5.
Variance inflation factors – Regression II (Violent Crime Rate)

Variable	VIF	1/VIF
NoHSDiploma2016	3.53	0.283
Hispanic2016	3.27	0.306
ForeignBorn2016	2.85	0.351
MedianHouseholdIncome2016	1.94	0.516
SanctuaryCity (0 or 1)	1.40	0.712
MedianAge2016	1.15	0.870

After removing the percentage of non-citizens variable from Regression I, all remaining variables' VIFs are now below five, suggesting that the multicollinearity issue was addressed.

Regressions I and II convey that sanctuary cities are statistically insignificant when describing changes in violent crime rates throughout U.S. cities. In the next subsection, I use property crime rates as the dependent variable

in Regression III and report similarities and differences between Regressions II and III which include the same independent variables.

Regression III: Property Crime Rate

Equation 3 provides the formula for regressing the independent variables from Regression II on the property crime rate.

Equation 3.

Property Crime Rate = 23 – 11.166(%FB) – 0.762(%HISP) + 1.001(MEDAGE) – 6.41(%NOHSDIP) -0.0003(MHI) + 1.346(SANCCITY)

Table 6 displays the results from Regression III.

Table 6.

Regression III (Property Crime Rate)

PropertyCrimeRate	Coef.	Std. Err.	t	P>t
ForeignBorn	-11.166	21.049	-0.530	0.597
Hispanic2016	-0.762	13.353	-0.057	0.955
MedianAge2016	1.001	0.317	3.155	**0.002**
NoHSDiploma2016	-6.41	34.980	-0.183	0.855
MedianHouseholdIncome2016	-0.0003	0.0001	-3.56	**0.0005**
SanctuaryCity1or0	1.346	4.115	0.327	0.744
_cons	23.000	11.353	2.026	0.045

The F-statistic for Regression III is 5.17, conveying that this regression equation is statistically significant. The R-squared value of Regression III is 0.1814, meaning that this particular linear model explains 18.14 percent of the variation in the property crime rate. In this third regression using property crimes as the dependent variable, only two of the independent variables are statistically significant. Both the median age of a given city and the median income of a given city are statistically significant at the one-percent level, with p-values of 0.002 and 0.0005, respectively. The coefficient for the median age in each respective city is 1.001. This means that for every one-year increase in the median age within a given city, I expect the property crime rate in that city to increase by 1.001 crimes. The coefficient for the median household income of each given city is -0.0003, suggesting that for every one-dollar increase in the median income in a respective city, I expect the property crime rate per one-thousand people to decrease by 0.0003.

The other four variables in this regression [(the percentage of foreign-born individuals in each city (p-value of 0.597), the percentage of Hispanic individuals in each city (p-value of 0.955), the percentage of individuals in each city over the age of twenty-five without a high school diploma (p-value of 0.855) and the dummy variable of either a sanctuary city or not a sanctuary city (p-value of 0.744))] are not statistically significant, even at the ten-percent level. Regression III reveals that sanctuary cities do not experience higher property crime rates than non-sanctuary cities. I ran both the Breusch-Pagan/Cook-Weisberg and the VIF Tests to rule out heteroskedasticity and multicollinearity, respectively, in Regression III and neither issue was detected.

Although the results show that sanctuary city designation is not correlated with increases in either violent crime rates or property crime rates, there are some differences between Regressions II and III. In the second regression with violent crime rates as the dependent variable and after correcting for multicollinearity by removing the non-citizen variable, four of the independent variables (the percentage of Hispanic individuals born in each city, the median age of the population in each city, the median income of each city and the percentage of the population over the age of 25 with less than a high school diploma) are statistically significant. In comparison, Regression III includes only two statistically significant variables: the median age in each city and the median income in each city.

Although the primary focus of this study is to analyze the correlation between sanctuary cities and crime rates, it is not quite clear to me why different variables are statistically significant and statistically insignificant when comparing violent crime and property crime. While the percentage of the population in each city with no high school diploma was statistically significant when analyzing violent crime, it was statistically insignificant when analyzing property crime. Could this suggest that less-educated individuals are more prone to committing violent crimes (murder, manslaughter, rape and aggravated assault) as opposed to property crimes (burglary, larceny-theft, motor vehicle theft and arson)? Although it is difficult to compare the severity of different types of crimes, these results may suggest that violent crimes (especially murder and rape) are more personal than certain property crimes, such as burglary or larceny. These results convey that there is a correlation between less-educated people committing violent crimes, but no significant correlation between less-educated people committing more or less property crimes. Another variable that differs between the two regressions using different dependent variables is the percentage of Hispanic people in each city. For violent crimes, the percent Hispanic variable is statistically significant at the one-percent level. As the percentage of Hispanic people in a given city increases, the violent crime rate decreases. Although the percentage Hispanic in each city still has a negative correlation when using

property crimes as the dependent variable, it is statistically insignificant and cannot explain changes in the property crime rate within cities. The only two variables that remain significant between the two different analyses are the median income in each city and the median age in each city. Irrespective of whether the dependent variable is property crimes or violent crime, the median income of a city and the median age of a city are statistically significant for both types of crime. Additionally, the signs of both of these independent variables remain the same when comparing property crimes and violent crimes; the median age of a city has a positive correlation with respect to crime, while the median income of a city has a negative correlation with respect to crime. Although these variables are not the primary focus of this research, the robustness of these findings with respect to alternative specifications of the dependent variable is interesting.

Conclusion

Consistent with the limited amount of prior research focused on sanctuary cities and crime, this research finds that no statistically significant relationship between violent and/or property crime rates in cities that have enacted sanctuary city policies and those cities that have not. Sanctuary cities do not, on average, experience higher violent and/or property crime rates simply because of the fact that they are labeled as "sanctuary cities" and have enacted associated policies.

Although the independent variable of "sanctuary cities" is the focus of this study, several other variables are noteworthy. The percentage of Hispanic individuals in each given city is statistically significant when analyzing violent crimes, and it displays a negative correlation in relation to each city's violent crime rate. This suggests that as the percentage of Hispanic people in a given city increases, I would expect the violent crime rate to decrease. In addition, the variable for the percentage of foreign-born individuals is statistically insignificant when analyzing both property and violent crimes. This is consistent with results from other studies as well; prior research suggests that cities with a higher foreign-born and/or non-citizen population tend to experience lower crime rates, on average, than cities with a higher native-born and/or U.S. citizen population.

The differences identified between the regression that uses violent crime rates as the dependent variable and the regression that uses property crime rates as the dependent variable are also fascinating. Why does education appear to be a valid indicator of increases or decreases in violent crimes (rape, murder, etc.), but it is not statistically significant when analyzing increases or decreases in property crimes? Additional research on this topic would be of added value. It is also important to note that both median household income and median age in each city were statistically significant in the property crime and violent crime regressions, respectively, and they displayed the same type of correlation (positive or negative) in both regressions. This provides strong evidence that, for both violent

crimes and property crimes, crimes committed decrease in cities as the median household income rises. Families with higher incomes are less inclined to commit crime, whether classified as violent or property offenses. This study also provides strong evidence that as the median age in a given city rises, the more violent and property crimes are committed. Further research on these topics would be of additional public interest.

Aside from ongoing research, some adjustments could refine this study further. The dependent variable exhibits vulnerability in relation to endogeneity. Sanctuary cities are defined as "sanctuary cities" when someone (typically a local government) declares the city as such and decides not to comply completely with ICE's demands. To correct for potential endogeneity, it could be beneficial to include a variable that could help explain why a city would choose to be a sanctuary city. A variable such as the political makeup/leanings of a sanctuary city could be a suitable independent variable to include to try and resolve this issue. Finally, another improvement that could be made is to revisit this study as a panel regression instead of a one-year (2016), cross-sectional study. This adaptation reveals whether the same conclusions apply for other years and ensures that 2016 was not an anomaly among crime rates in these particular cities.

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Appendix A (List of Cities Studied)

Alabaster AL	South Bend IN	Omaha NE	Brookings SD
Tuscaloosa AL	Iowa City IA	Henderson NV	Rapid City SD
Mobile AL	Cedar Rapids IA	Las Vegas NV	Sioux Falls SD
Anchorage AK	Sioux City IA	Reno NV	Bristol TN
Fairbanks AK	Dodge City KA	Concord NH	Franklin TN
Juneau AK	Hays KA	Dover NH	Memphis TN
Avondale AZ	Wichita KA	Portsmouth NH	Arlington TX
Casa Grande AZ	Florence KY	Bayonne NJ	Dallas TX
Glendale AZ	Independence KY	Newark NJ	San Antonio TX
Little Rock AK	Radcliff KY	Princeton NJ	Orem UT
Pine Bluff AK	New Orleans LA	Albuquerque NM	Provo UT
Sherwood AK	Lafayette LA	Farmington NM	Salt Lake City UT
Oakland CA	Shreveport LA	Las Cruces NM	Alexandria VA
Berkeley CA	Auburn ME	Ithaca NY	Norfolk VA
Fremont CA	Lewiston ME	Albany NY	Roanoke VA
Watsonville CA	Sanford ME	New York City NY	Mount Vernon WA
Santa Ana CA	Baltimore MD	Burlington NC	Olympia WA
Tulare CA	Annapolis MD	Goldsboro NC	Seattle WA
San Francisco CA	Greenbelt MD	Wake Forest NC	Charleston WV
Aurora CO	Amherst MA	Bismarck ND	Huntington WV
Denver CO	Lawrence MA	Fargo ND	Parkersburg WV
Hartford CT	Boston MA	Minot ND	Eau Claire WI
East Haven CT	Worcester MA	Solon OH	Green Bay WI
Dover DE	Detroit MI	Toledo OH	Madison WI
Newark DE	Grand Rapids MI	Cleveland OH	Cheyenne WY
Wilmington DE	Lansing MI	Broken Arrow OK	Gillette WY
Cape Coral FL	Duluth MN	Oklahoma City OK	Rock Springs WY
Daytona Beach FL	Minneapolis MN	Tulsa OK	
Miami FL	St. Cloud MN	Eugene OR	
Albany GA	Biloxi MS	Salem OR	
Newnan GA	Jackson MS	Springfield OR	
Savannah GA	Olive Branch MS	Allentown PA	
Boise ID	Ferguson MO	Philadelphia PA	
Nampa ID	Joplin MO	Pittsburg PA	
Post Falls ID	St. Louis MO	Central Falls RI	
Chicago IL	Billings MT	Newport RI	
Joliet IL	Bozeman MT	Providence RI	
Rockford IL	Helena MT	Charleston SC	
Bloomington IN	Fremont NE	Columbia SC	
Evansville IN	Lincoln NE	Myrtle Beach SC	

Does Immigration Enforcement Reduce Crime? Evidence from Secure Communities

Thomas J. Miles *University of Chicago*
Adam B. Cox *New York University*

Abstract

Prior research investigates whether immigrants commit more crimes than native-born people. Yet the central policy used to regulate immigration—detention and deportation—has received little empirical evaluation. This article studies a recent policy innovation called Secure Communities. This program permits the federal government to check the immigration status of every person arrested by local police and to take the arrestee into federal custody promptly for deportation proceedings. Since its launch, the program has led to a quarter of a million detentions. We utilize the staggered rollout of the program across the country to obtain differences-in-differences estimates of its impact on crime rates. We also use unique counts of the detainees from each county and month to estimate the elasticity of crime with respect to confined immigrants. The results show that the Secure Communities program has had no observable effect on the overall crime rate.

1. Introduction

The belief that immigrants—or at least certain immigrants—commit more crime than native-born people is persistent and widely held. Popular concern with immigrant criminality has risen with each succeeding wave of immigrant groups: first Irish and Chinese immigrants, then Italians and others from southern and eastern Europe, and today Mexicans and others from Latin America. This belief has translated directly into public policy and has made crime control a leading objective of immigration regulation.

Deportation, the physical removal of an immigrant from the country, has long

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been a key tool in pursuing this objective. Well before the United States became a nation, other states regularly used "transportation"—the deportation of large numbers of criminals—as a means of crime control. Early efforts at immigration restriction in America built on these ideas, excluding those who were serving criminal sentences elsewhere and anyone whose sentence was commuted in exchange for passage to the United States (see section 3 of the Page Act, 18 Stat. 477 [March 3, 1875]). Moreover, in recent decades immigration policy has been reshaped as an instrument of criminal justice policy on a breathtaking scale. New grounds for criminal deportation have proliferated, immigration status violations have increasingly been used to remove suspected criminals who would be difficult or impossible to prosecute under criminal law, and the immigration and criminal enforcement bureaucracies have become deeply entwined (Cox and Miles 2013; Sklansky 2012).

The belief in immigrant criminality that produced these policies has persisted even though academic research generally finds that immigrants are no more prone (and may be less prone) to engage in crime than native-born people. Yet, given the longstanding focus on whether immigration increases crime, it is remarkable that the policy at the heart of the immigration-crime debate—the detention and deportation of immigrants who commit crimes—has received almost no empirical evaluation. A central goal of the immigration system is to screen out undesirable immigrants and shape the behavior of immigrants living in the host country (Cox and Posner 2007). Thus, even if immigrants have lower average offending rates than citizens, the selectivity of the immigrant-screening system might produce meaningful crime reductions. Moreover, this screening system today represents a massive social intervention. Policies requiring the detention and deportation of noncitizens who commit crimes have grown explosively in recent years. Yet little is known about whether detention and deportation of these immigrants makes communities safer.

This article fills this gap by examining a policy innovation specifically intended to increase the detention and deportation of immigrants who commit crimes, whom we refer to as "immigrant offenders."[1] The program, called Secure Communities, has a simple goal: to ensure that every person arrested for a crime by local law enforcement anywhere in the country is screened by the federal government for immigration violations. Before this program, a criminal arrestee's immigration status was rarely investigated because it was a labor-intensive task, typically requiring the physical presence of a federal officer in a local lockup. Secure Communities revolutionized the screening process by relying instead on fingerprint information about arrestees that was already being collected by local police and forwarded electronically to the Federal Bureau of Investigation (FBI). Under Secure Communities, these fingerprints are now also forwarded to the Depart-

[1] Given the relatively common confusion about the civil nature of most immigration law, we should emphasize that we use the term "immigrant offenders" throughout this article to refer to noncitizens who commit crimes, not noncitizens who are simply in violation of immigration law. In popular discourse, immigrant offenders are often referred to as criminal aliens.

ment of Homeland Security (DHS). This relay is electronic, instantaneous, and routine. Local police can avoid submitting fingerprints to DHS only by refusing to conduct a criminal background check on an arrestee, which would be contrary to standard arrest practices.

At DHS, the fingerprints are checked against its database of foreign-born persons. If it identifies a putatively deportable noncitizen whom it wishes to remove, the enforcement arm of DHS, known as Immigration and Customs Enforcement (ICE), notifies the local police and takes custody of the noncitizen when she is released from criminal confinement. Typically, ICE retains custody of the person until the completion of removal proceedings, which leads, in the vast majority of cases, to the person's deportation. Secure Communities thus increases the probability that immigrant offenders who would otherwise be released are subject to immediate federal detention and ultimate deportation. In its first 4 years, ICE has detained over 250,000 immigrants through the program and has deported over 200,000 people. As a percentage of noncitizens in the United States, this detention figure exceeds the percentage of the U.S. population currently incarcerated in the criminal justice system.

Technological constraints prevented the simultaneous nationwide implementation of Secure Communities. Instead, DHS rolled out the program on a county-by-county basis over 4 years. This staggered implementation created variation across counties in the rates at which immigrant offenders were detained. We argue that the timing of Secure Communities activations was exogenous to county crime rates and provides an ideal experiment to identify the impact on crime of detaining and deporting noncitizen offenders. It also allows us to evaluate whether Secure Communities met its objective of reducing violent crime—a critical question for a program that represents the largest effort in U.S. history to integrate local police into federal immigration enforcement.

We estimate the impact of Secure Communities on crime rates using a monthly panel of nearly 3,000 counties over 9 years. Exploiting the quasi experiment created by the staggered timing in the program's implementation, we develop differences-in-differences estimates of the impact of immigrant detention under the program on crime. In addition, we use unique data obtained from DHS through a series of Freedom of Information Act (FOIA) requests—data on the number of persons detained by month and county under the program—to estimate directly the elasticity of crime with respect to immigrant detention.

Our results indicate that Secure Communities has had no impact on the overall rate of crime. This finding is striking given the large number of people detained under the program and the fact that the program is designed to prioritize the removal of immigrants with offending rates higher than those of the average immigrant. The rollout of the program coincided with a period of declining crime rates in counties with high immigrant concentrations, and, accordingly, the estimates are sensitive to controlling for county-specific trends. When accounting for county-level trends, the estimates indicate that Secure Communities had no effect on the FBI index crime rate, and this 0 estimate is fairly precise. Some spec-

ifications suggest modest declines in the rates of two property crimes—burglary and motor vehicle theft—but even these results imply an elasticity of crime with respect to immigrant detention that is far smaller than existing estimates of the elasticity of crime with respect to prison populations.

The article proceeds in five parts. Section 2 reviews the existing empirical literature on immigration and crime, and Section 3 develops a simple theoretical framework for understanding how immigration enforcement may influence crime. Section 4 describes the details of the policy intervention, explaining both the operation and the rollout of Secure Communities. Section 5 explains the empirical strategy and data, and Section 6 presents the results.

2. Literature on Immigration and Crime

Crime and immigration have long been linked by two corollary questions. First, does immigration increase crime rates? Second, can detention and deportation reduce crime rates? There is substantial prior work on the first question, yet very little on the second.

Scholars have examined the first question using both microdata and geographic aggregates. Individual-level studies relate self-reports of criminal activity to immigrant status. Butcher and Piehl (1998b) examine the 1980 wave of the National Longitudinal Survey of Youth and find that immigrant youth were less likely to report offending. This pattern was evident in unconditioned comparisons and robust to controlling for a large set of youth and family characteristics. Sampson, Morenoff, and Raudenbush (2005) survey over 3,000 individuals in Chicago neighborhoods and find that rates of violence rose with a generation's distance from immigration. The odds of violence for a first-generation immigrant was three-fourths that of a second-generation immigrant and half that of a third-generation immigrant. Desmond and Kubrin (2009) examine the National Longitudinal Survey of Adolescent Health, which contains a rich set of individual and community characteristics. They estimate that youths living in neighborhoods with more foreign-born residents have a lower risk for violence, and this risk is even lower for foreign-born youths in these communities.

Other studies with individual-level data infer offending rates from institutionalization rates. Butcher and Piehl (1998a, 2007) examine multiple decennial censuses and find that, even without conditioning on age or education, immigrant institutionalization rates were less than half those of native-born people. Moreover, this gap widened over time, and deportation does not appear to account for it (Butcher and Piehl 2000). To the contrary, immigration enforcement policies may cause an overrepresentation of immigrants in jail populations. Hagan and Palloni (1999) find that in El Paso, Texas, and San Diego, California, immigrants were more likely to be detained before trial, a pattern that they speculate may be due to detainer requests (also known as immigration holds) placed on immigrants by federal authorities. They observe that defendants detained before trial faced higher rates of conviction and incarceration, and after adjusting for the

effects of pretrial detention on adjudication and sentencing, they estimate that immigrants offended less often than native-born people. Similarly, Butcher and Piehl (2007) conclude that the self-selection of immigrants who have either lower criminal propensities or greater responsiveness to punishments than native-born people explain their lower institutionalization rates.

The second approach to studying the effect of immigration on crime focuses on geographic aggregates rather than individuals. This approach asks whether crime rates differ in times when, or places where, there are higher concentrations of immigrants. Some studies make cross-sectional comparisons either across cities or across neighborhoods in a city. Reid et al. (2005) examine a cross section of metropolitan areas in the year 2000. They find that, after controlling for the characteristics of each area, the share of immigrants in the population had either a negative correlation or no correlation with the rates of four categories of crimes. Graif and Sampson (2009) apply a variety of regression techniques to a cross section of Chicago census tracts and similarly find that, after controlling for other neighborhood characteristics, immigrant concentration had either no relationship or a negative relationship to homicide rates.

Cross-sectional estimates may be biased upward because immigrants tend to locate in cities where rates of poverty and crime are higher. To combat this selection bias, some authors have turned to panel data so that fixed effects can be used to control for time-invariant differences across locations. Butcher and Piehl (1998a) analyze a panel of metropolitan areas over the 1980s. They identify a positive association between the inflow of immigrants and a city's crime rate, consistent with larger cities having both more crime and more immigrants. But once they condition on a city's demographics and labor-market conditions, this association disappears. Similarly, Ousey and Kubrin (2009) examine a sample of large cities at three points in time between 1980 and 2000 and find that an index of immigrant concentration correlated negatively with a city's crime rate. MacDonald, Hipp, and Gill (2013) instrument for immigrant concentration in Los Angeles neighborhoods in 2000 with the level in 1990. They find that the presence of higher concentrations of immigrants led to substantial reductions in crime. Spenkuch (2014) takes a similar approach but reaches somewhat different conclusions. Analyzing counties at three decennial intervals, his ordinary least squares estimates imply that a 10 percent increase in a county's share of immigrants has no effect on violent crime but raises property-crime rates by 1.2 percent. When the share of immigrants is instrumented with its own lag, the point estimates are stable but lose statistical significance. Spenkuch concludes that the social benefits of immigration outweigh the costs of these additional crimes.

Other panel studies consider immigration shocks, focusing on periods of sudden immigration waves that permit before-and-after comparisons within a location. Martínez, Stowell, and Lee (2010) analyze homicides in census tracks in San Diego between 1980 and 2000, a period of rapid immigration. They find that declines in homicide rates accompanied increases in the share of the foreign-born population. Bell, Fasani, and Machin (2013) look at two waves of immigration

since the 1990s in the United Kingdom. They report that the rate of property crimes increased in local areas experiencing the largest influx of immigrants during the first episode, but they observe no change in crime rates during the second episode. Chalfin (2014) observes that deviations in rainfall from its long-term mean in Mexican states raised the likelihood of migration to the United States. Using these rainfall shocks as instruments, Chalfin estimates that Mexican immigration had no effect on rates of violent or property crime in major U.S. metropolitan areas.

Existing work largely ignores the corollary question of whether immigration policy can be an instrument of crime control. This is puzzling, because a central goal of immigration law has long been to screen out undesirable immigrants and to shape the behavior of immigrants once they arrive in the host country. The explosive growth of these screening policies during the 20th century—particularly of policies directly targeting immigrants who commit crimes—raises important questions about the effect of immigration policy on crime.

The first is whether granting immigrants legal status affects their offending behavior. Mastrobuoni and Pinotti (2012) examine two exogenous variations in the legal status of Italian immigrants: a 2006 clemency and the 2007 enlargement in the European Union. They estimate that having legal immigration status halved the odds of reincarceration for a criminal offense, and this accounts for one- to two-thirds of the gap in offending between immigrants and the native-born population. Two papers study the 1986 immigration reform that granted legal resident status to some immigrants and imposed penalties on employers that hire immigrants without legal status. Baker (2014) examines a nationwide panel of counties and estimates that for every 1 percent of a county's population that received legal status, crime rates, especially property-crime rates, fell by 1–4 percent, a drop he attributes to increased labor-market access by immigrants who gained legal status. By contrast, Freedman, Owens, and Bohn (2014) find that in San Antonio, Texas, felony charges, especially for economically motivated crimes, filed against Hispanics rose by nearly 60 percent after the 1986 immigration reform. Because the neighborhood characteristics of these defendants suggest that they were likely immigrants who did not gain legal status as part of the reform, the authors attribute the increase in crime to the presence of new barriers to work imposed by the employer-sanctions regime that accompanied the legalization program.[2]

A second question—in some sense the opposite of the first—is whether detaining and deporting immigrant offenders can reduce crime. Over the past 2 decades, efforts to deport noncitizen offenders have increased and have in many ways effectively integrated the immigration and criminal justice systems. In particular, the practice of deporting noncitizens who commit crimes, unheard of in U.S. law prior to 1907, has become perhaps the most important tool for screening

[2] Freedman, Owens, and Bohn (2014) also show that after the legal change, felony charges against Hispanics were less likely to end in conviction, which implies that policing behavior also responded to the legal change. But they conclude that even after adjusting for altered policing practices, barriers to legal work may increase immigrant offending.

out immigrants (Cox and Posner 2007). These developments have led legal scholars such as Sklansky (2012) to see the immigration and criminal justice systems today as effectively a single governance system focused on deporting noncitizen offenders and worthy of its own name, "crimmigration."

We are aware of only one prior article attempting to study the effect of deportation on crime.[3] Stowell et al. (2013) examine deportations in several metropolitan areas between 1994 and 2000 and conclude that they had no relationship to the incidence of violent crime. Yet Stowell et al. do not specifically measure the deportation of immigrant offenders, and the vast majority of deportees during that period had no criminal convictions. They also did not consider the detention of immigrants prior to deportation, even though periods of detention may be lengthy. Moreover, their measure of deportations is at the level of an ICE administrative district, a unit that typically encompasses multiple states, and this mismatch between the policy and the outcome introduces the risk of attenuation bias.[4]

3. Theoretical Framework

An obstacle in using geographic aggregates to examine the relationship between immigration and crime is that the offending rates of immigrants and native-born people are not directly observed. Instead, the observed crime rate is a weighted average of the offending rates of immigrants and native-born people. Specifically, letting β_k be population group k's offending rate, permitting α_k to be the group's share of the population, and allowing index k to refer either to I (immigrants) or N (native-born people), the overall crime rate can be written as

$$\beta = \alpha_I\beta_I + \alpha_N\beta_N. \quad (1)$$

The primary intervention of Secure Communities is to detain and deport immigrant arrestees whom local law enforcement would otherwise release back into

[3] In work subsequent to ours, Treyger, Chalfin, and Loeffler (2013) also investigate the impact of Secure Communities. Their analysis has several differences from that conducted here. They measure only a single cross section of deportations on the last date of their sample. By contrast, we observe both detentions and deportations in each month since the program's launch. Also, the Department of Homeland Security (DHS) rolled out Secure Communities on a county-by-county basis, but Treyger, Chalfin, and Loeffler's observations of crime data are at the city level, which creates a mismatch between their outcome and treatment variables and causes the exclusion of a substantial number of jurisdictions affected by Secure Communities. As we describe below, our data are structured as county-month observations.

[4] A strand of the literature on cooperation with police has examined how immigrants perceive law enforcement. This work has focused almost entirely on New York and has tended to examine general policing rather than immigration enforcement specifically. Kirk et al. (2012) survey New York City residents and find that persons residing in neighborhoods with high concentrations of foreign-born people were less cynical about the law and more willing to cooperate with the police. Davies and Fagan (2012) analyze crime and policing data for New York and find that neighborhoods with higher concentrations of immigrants were subjected to more intense police activity, as measured by arrests and street stops, even though these communities had lower rates of total and violent crime. Tyler, Schulhofer, and Huq (2010) show that the risk of deportation reduced feelings of trust and police legitimacy among Muslims in New York.

the community. First, the program raises the probability of an arrestee's immediate and continued confinement. It increases the likelihood that an immigrant who is arrested for a local offense is placed in federal custody rather than released while her criminal case is adjudicated.[5] Once an immigrant is detained as a result of Secure Communities, she often remains in federal custody until the completion of deportation proceedings. Second, Secure Communities increases the probability that the noncitizen will be deported following the completion of her criminal adjudication and any criminal sentence imposed. Removal from the country constitutes continuing incapacitation because reentry is difficult, both legally and practically. Immigration law currently prohibits deportees from seeking readmission for at least 10 years, and offenders convicted of certain offenses are barred for life (8 U.S.C. sec. 1182[a][9]). While border enforcement is imperfect, avoiding detection at the border is costly, and reentering without permission is a felony punishable by a term of incarceration of up to 20 years for those deported after being convicted of certain crimes (8 U.S.C. sec. 1326).

To capture the incapacitative effects of Secure Communities, let S represent the number of immigrants detained under the program. The immigrants' share of the population is a function of S, or $\alpha_I = \alpha_I(S)$. The removal of persons through detention and deportation implies a mechanical reduction in the size of the immigrant population, or $\partial\alpha_I/\partial S < 0$.[6]

Secure Communities also alters the expected sanction for crimes committed by immigrants. The increased risk of immediate and continuing postarrest confinement is surely seen by arrestees as a sanction, as are the eventual removal from the country and the accompanying long-term bar on reentry. Importantly, Secure Communities supplies these additional sanctions without undercutting the underlying criminal sanction. Immigration law prohibits federal authorities from deporting an incarcerated noncitizen prior to the completion of his criminal sentence.[7] Deportation is thus imposed in addition to, rather than in lieu of, any criminal sentence imposed on the arrestee.

In the classic deterrence model of Becker (1968), an increase in the expected sanction should dissuade offending. Accordingly, it is assumed that the offending behavior of immigrants depends on detention, or $\beta_I = \beta_I(S)$, and that the im-

[5] It also increases the likelihood that the immigrant will be denied bond in her criminal proceeding, as some local courts routinely deny bond to criminal defendants against whom immigration detainers have been lodged.

[6] We do not directly consider the possibility of replacement because the focus of our empirical analysis is on relatively short-term effects.

[7] Subject to limited exceptions, "the Attorney General may not remove an alien who is sentenced to imprisonment until the alien is released from imprisonment" (8 U.S.C. sec. 1231[a][4][A]). This does not mean, of course, that the availability of deportation never affects the criminal sanction received by a noncitizen. First, some federal criminal enforcement initiatives use immigration violations to remove gang members. Often this occurs in situations in which there are obstacles to criminal prosecution (Chacón 2007; Sklansky 2012), but there might be instances in which prosecutors would bring criminal charges but for the availability of deportation as an option. Second, in practice there are almost certainly situations in which a person charged with a crime receives a lighter sentence, or has her changes dropped altogether, when local officials know that she will be handed over to Immigration and Customs Enforcement (ICE) following any criminal sentence.

migrant offending rate declines with the number of noncitizens detained under Secure Communities, or $\partial\beta_I/\partial S < 0$. Last, it is assumed that Secure Communities has no effect on β_N, the offending rate of citizens, because citizens cannot legally be deported, and thus the policy should have no incentive or incapacitating effects on citizens.[8]

The effect on the overall crime rate of increasing the number of incapacitated immigrants can be seen by differentiating equation (1) with respect to S. Using the definition $\alpha_I + \alpha_N = 1$, we can write this as

$$\partial\beta/\partial S = \alpha_I\partial\beta_I/\partial S + (\beta_I - \beta_N)\partial\alpha_I/\partial S. \quad (2)$$

The impact of the policy is the sum of two terms, the first of which represents the effect on the offending behavior of immigrants and the second of which is its effect on the size of the immigrant population. The first term on the right-hand side is negative because of the assumption of a deterrent response to the heightened sanction of immediate detention and eventual deportation.

The second term on the right-hand side consists of the change in the size of the immigrant population weighted by the difference in offending rates of immigrants and native-born people. Before considering the sign of this term, it is worth noting that it may be 0 in two circumstances. Trivially, it is 0 when the policy intervention is too small to affect the size of the immigrant population, or $\partial\alpha_I/\partial S = 0$. We rule out this possibility. Section 4 describes how Secure Communities has induced substantial movement in the share of the immigrant population, especially in counties with high concentrations of immigrants. Next, the term may be 0 when immigrants and native-born people offend at similar rates, or $\beta_I = \beta_N$.

Setting aside these two possibilities of 0 values, the sign of the second term on the right-hand side of equation (2) depends on the relative values of β_I and β_N. The persistent popular belief is that immigrants offend at higher rates than native-born people, or $\beta_I > \beta_N$. If this were true, the effect of Secure Communities on the overall crime rate would be unambiguously negative. The policy would reduce observed crime rates through both a behavioral effect (deterrence) and an incapacitative effect (removing individuals who offend at higher rates from the population).

If instead the majority of the academic evidence is correct that immigrants offend at lower rates than native-born people, or $\beta_I < \beta_N$, then the second term on the right-hand side of equation (2) would be positive. This term would represent a counterintuitive consequence of Secure Communities: that it would contribute to a higher observed crime rate. It would do so by removing persons who offend less frequently (immigrants) and would leave a population composed of proportionately more persons who offend more frequently (native-born people). Whether the overall consequence of Secure Communities would be to increase

[8] While enforcement errors do on occasion lead to the unlawful detention (or even deportation) of citizens, there is no evidence that such errors are common enough to produce a meaningful incentive or incapacitation effect among citizens.

the observed crime rate would depend on whether this compositional effect exceeds the deterrent effect.

The stylized framework above can be extended in two ways. First, the model assumes that offending rates are uniform within groups. In reality, of course, individuals differ considerably in their propensity to commit crimes. If ICE prioritizes the most serious immigrant offenders, the marginal immigrant detained under Secure Communities would have a higher offending frequency than the average immigrant. This is likely because the program targets only those who have been arrested by local criminal authorities. It implies that estimates of immigrant offending from Secure Communities may be biased upward from the average rate.

Second, the framework assumes that there is just one type of crime, but there are many crimes of varying levels of seriousness. Serious offenders are more likely to be confined prior to adjudication and typically receive longer sentences than more minor offenders. This practice influences the types of immigrant offenders who are likely detained under Secure Communities and who are likely deterred by it. A person charged with a serious felony, such as murder, is likely to be detained by state authorities prior to her trial and, if convicted, to receive an extremely long prison sentence. She will face deportation only at the completion of the prison term. In such cases, Secure Communities has no marginal incapacitative impact in the short term, and given the discounting of future utility, it is also unlikely to have much deterrent impact. By contrast, a person convicted of a less serious offense, such as simple drug possession, might typically be sentenced to time served during the pendency of the criminal proceeding. Before Secure Communities, time served would be the only sanction, but after Secure Communities, ICE may elect to take this defendant into federal custody and begin deportation proceedings. Thus, Secure Communities' effect on deterrence and incapacitation may be greatest when the offense of conviction is less serious.

Given the relatively short period during which Secure Communities has been in operation, violent offenders are unlikely to constitute a large fraction of immigrants detained under the program—even if DHS wishes to target such offenders. As shown below, the data demonstrate that the majority of the immigrants taken into federal custody as a result of Secure Communities are not violent offenders. Yet the program may still reduce violent-crime rates if offenders are not highly specialized. For example, in a study of state-prison populations, Kuziemko and Levitt (2004) find that incarcerating a drug offender reduced violent and property crime by about as much as confining other types of offenders.

4. Secure Communities

4.1. Program Operation

Federal immigration law makes many crimes grounds for deportation (8 U.S.C. secs. 1182[a][2], 1227[a][2]). Until recently, however, the task of identifying noncitizens among the pool of persons arrested by state and local law en-

forcement authorities was costly. Before Secure Communities, immigrant arrestees were identified principally through individual inmate interviews in local jails and prisons. These interviews were conducted by federal officials pursuant to the Criminal Alien Program and by deputized local law enforcement officials under so-called 287(g) agreements.[9] These labor-intensive efforts were piecemeal. Federal personnel conducted these screenings in less than 15 percent of local jails and prisons, and local officials were authorized to do the screenings themselves in only about 2 percent of the nation's counties (Cox and Miles 2013).

Secure Communities shifted to a system of universal and automated screening such that every person arrested by a local law enforcement official anywhere in the country would be screened by the federal government for immigration status and deportation eligibility. The program accomplished this through a technological innovation that piggybacks on standard arrest procedures. Traditionally, when a person is arrested and booked by a state or local law enforcement agency, her fingerprints are taken and forwarded electronically to the FBI, which conducts a criminal background check and sends the results to the local agency. Secure Communities' innovation was to take the fingerprints received by the FBI and automatically and electronically forward them to DHS, which then compares the fingerprints against its Automated Biometric Identification System, a database that stores biometric and biographical information on persons encountered by the agency in the course of its immigration-related or other activities. The database includes fingerprints of three categories of foreign-born persons: noncitizens present in the United States in violation of immigration law, such as persons who were previously deported or overstayed their visas; noncitizens who are lawfully in the United States but who might become deportable were they to be convicted of the crime for which they have been arrested; and citizens who naturalized at some date after their fingerprints were included in the database.

If the fingerprints match a set in the DHS database, DHS personnel evaluate the person's immigration status and decide whether to place a detainer (sometimes referred to as an immigration hold) on the person. The detainer requests that the local law enforcement agency hold the person for 48 hours beyond the scheduled release, in order to permit ICE to transfer her to federal custody for the initiation of deportation proceedings. The detainer thus allows the federal government to readily apprehend and place in deportation proceedings a noncitizen whom the local criminal justice system would otherwise release. This includes a person who otherwise would have been released because her arrest did not result in conviction, because she was granted bail pending the outcome of her criminal proceeding, or because she had completed her term of incarceration following conviction.

Secure Communities thus increases the likelihood that noncitizens arrested for crimes by local authorities will be identified by the federal government, ap-

[9] The name "287(g)" refers to section 287(g) of the Immigration and Nationality Act (8 U.S.C. sec. 1357[g]), the federal statute that authorizes the attorney general to enter into these agreements.

prehended by the immigration authorities (rather than released), and ultimately deported. The program's ambitious scope makes it the largest expansion of local involvement in immigration enforcement in the nation's history.

4.2. Rollout of Secure Communities

Secure Communities, unlike most federal policies and programs, could not be activated everywhere in the country at once. Resource bottlenecks, technological constraints, and the sheer scope of the task of communicating with the roughly 31,000 booking locations around the country necessitated a staggered rollout. Over a period of 4 years, beginning on October 27, 2008, the federal government rolled out the program on a county-by-county basis. By spring 2012, Secure Communities had been formally activated in all but a handful of counties, and by January 2013 it was completely activated nationwide. Figure 1 shows the pattern of rollout.

This staggered sequence of rollout creates a natural experiment in the detention and deportation of immigrant offenders, and we use this policy variation to identify the effect on crime of detaining noncitizen offenders. The ideal experiment for measuring the causal impact of immigrant detention on crime would be to assign a program of enhanced enforcement randomly to some jurisdictions and not to others. We argue that the timing of Secure Communities activation approximates this ideal.

The federal government determined the sequence of rollout. It prohibited local governments from formally opting out of Secure Communities even though elected officials in some localities wished not to participate.[10] Moreover, the program's structure made informal noncompliance with the screening system practically impossible. Once Secure Communities is activated in a county, local authorities have no way to share the fingerprints of arrestees with the FBI but not with DHS. The only way a local law enforcement agency could prevent DHS's immigration check from taking place would be to stop fingerprinting arrestees altogether, and we are aware of no local agency that has done so.[11]

Cox and Miles (2013) explore in detail the determinants of Secure Communities' activation using proportional hazard analysis and find that, while the timing of activation was not wholly random, it appeared to mirror federal enforcement priorities for immigration generally rather than for crime control. The strongest correlates of an early activation were a county's location on the southern border

[10] Initially, there was some confusion about whether Secure Communities was mandatory, in part because DHS failed to provide clear public guidance and in part because the agency initially entered into memoranda of understanding with state governments (though not with local governments or law enforcement agencies). As soon as some states began to resist signing these agreements, however, the government made clear that the agreements were not required because the program required no actions by state or local officials; all that was required was a rerouting of the fingerprint data stream among the federal agencies (Department of Homeland Security 2012).

[11] While law enforcement agencies are powerless to stop the immigration check, they can resist the program by refusing to honor detainer requests issued by ICE. We discuss this possibility in Section 6.2.

and the fraction of the county's population that was Hispanic. Although Hispanic and foreign-born populations correlate closely with each other, Cox and Miles (2013) find that, after controlling for other factors, only the Hispanic population fraction had a statistically significant relationship to activation timing. The federal government's decision to commence the program on the southern border, a flashpoint of popular debate over immigration policy, suggests that despite the allusion to public safety in the program's title, the federal government saw immigration regulation rather than crime control as Secure Communities' main purpose.

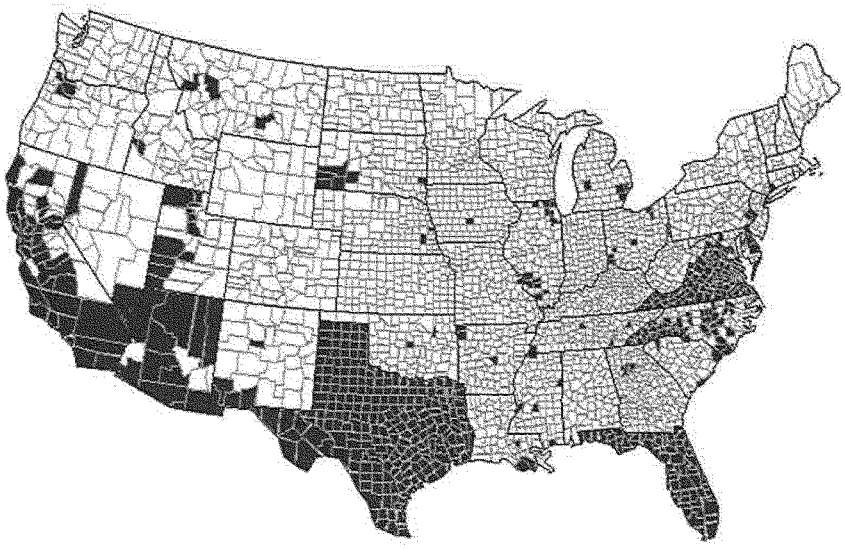
Figure 2 provides a summary of these findings. The solid line shows that Secure Communities expanded its coverage of the total population in a steady, at times linear, fashion. By contrast, the rapid activation of counties on the southern border resulted in over 90 percent of that population being covered by Secure Communities in its first year. The dashed line shows that the Hispanic population was covered more quickly than the overall population but at a substantially slower pace than counties directly on the border. The foreign-born population received coverage less rapidly than the Hispanic population but more quickly than the general population. The correlation of Hispanic and foreign-born populations with the speed of activation is far less dramatic than that of border counties. Although these population groups are associated with earlier activation, large portions of the Hispanic and foreign-born populations were not covered by Secure Communities until late in its rollout.

Cox and Miles (2013) also find that, after controlling for urban density, the timing of activation does not correlate with crime rates or the level of local policing. When measures of urban density were excluded from the analysis, higher crime rates are correlated with only a modest increase in the speed of activation. Even in counties with very large Hispanic populations, higher crime rates implied only small movements in the timing of activation. Thus, the pattern of activation does not have a close relationship to the primary outcome of interest here, crime rates. Moreover, even if the federal government had activated sooner the counties where it expected the largest crime reductions, such targeting should bias our differences-in-differences estimates toward finding a crime-reducing effect, which makes our findings—of almost no meaningful reductions in crime—more compelling.

4.3. Program Intensity

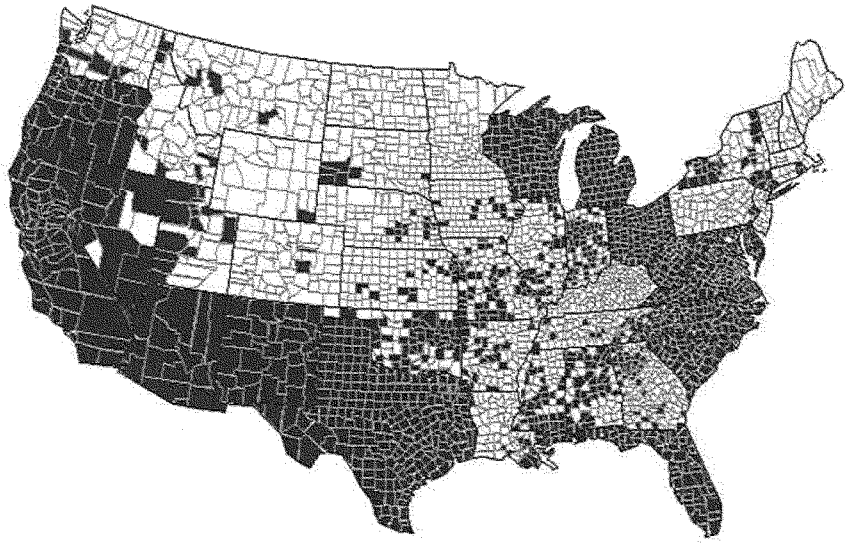
The rates at which ICE detains immigrants under Secure Communities varies across counties and within a county over time. A simple classification of counties as activated or not activated under the program therefore measures with error the intensity of the program's intervention, and differences-in-differences estimates of the program's effect will suffer from attenuation bias. We overcome this problem by measuring the magnitude of the program's primary intervention directly. Through a series of FOIA requests to DHS, we obtained tallies of the persons

Counties Activated Prior to October 1, 2009

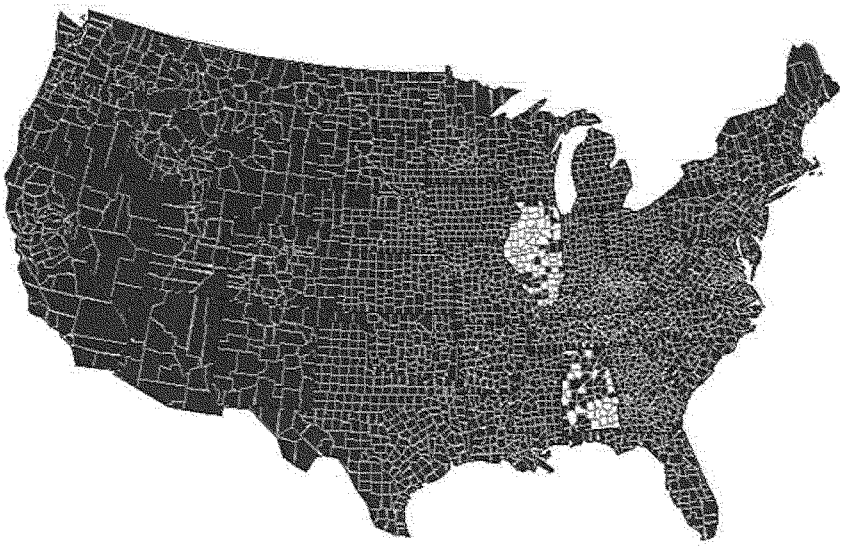


Counties Activated Prior to October 1, 2010

Figure 1. Pattern of Secure Communities activation



Counties Activated Prior to October 1, 2011



Counties Activated Prior to October 1, 2012

Figure 1. *Continued*

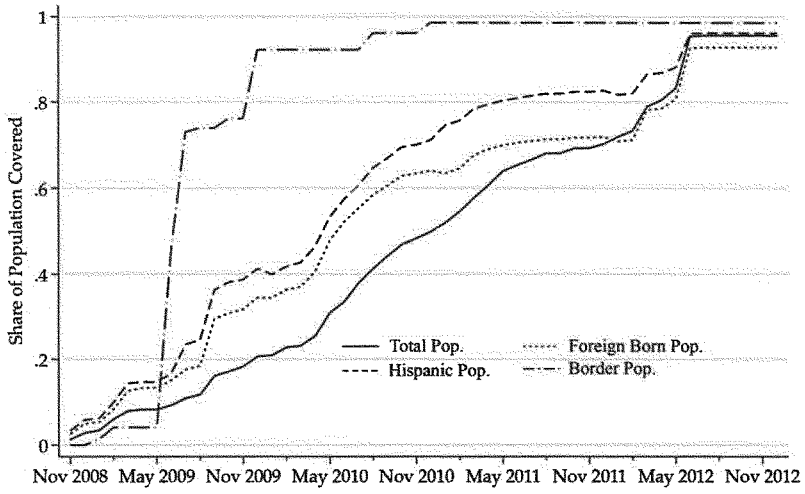


Figure 2. Shares of population groups covered by Secure Communities

detained by ICE under Secure Communities in each county and month.[12] The cumulative number of immigrants taken into federal custody under Secure Communities in each county by each month is our measure of the program's intensity.

Figure 3 highlights the importance of using this direct measure of program intensity. The cumulative number of activated counties is shown in bars corresponding to the left scale and the cumulative number of persons taken into federal custody under Secure Communities is shown in lines corresponding to the right scale. The lighter (taller) bars indicate all activated counties, while the darker (shorter) bars indicate the number of activated counties whose share of foreign-born residents is in the top quartile of all counties. The figure illustrates two important differences between this direct measure of program intensity and a binary measure of program activation. First, detention rates rise with the length of time a county has been activated. The final 8 months of 2012 saw virtually no change in the number of activated counties, as all but a few locations had been activated, but the number of immigrants detained continued to grow steadily. Sec-

[12] We also obtained data from DHS on other potential intensity measures, including the monthly number of persons deported under Secure Communities in each county. Most deportees identified through Secure Communities are detained by the federal government during the pendency of their deportation proceedings, and a very high percentage of those who are detained are ultimately deported. For these reasons, the date of federal detention, rather than the date of deportation, is a more accurate measure of the program's intervention. Moreover, the deportation counts are not necessarily a good (lagged) proxy for the detention counts, because the period of federal detention before removal varies widely from case to case: some noncitizens are detained only a few days because they agree to stipulated orders of removal and forgo deportation proceedings entirely, while others are detained for months or years during the pendency of their deportation proceedings in immigration court. That being said, all of the results we find using federal detention as an intensity measure hold, largely unchanged, if the number of deportations is instead used.

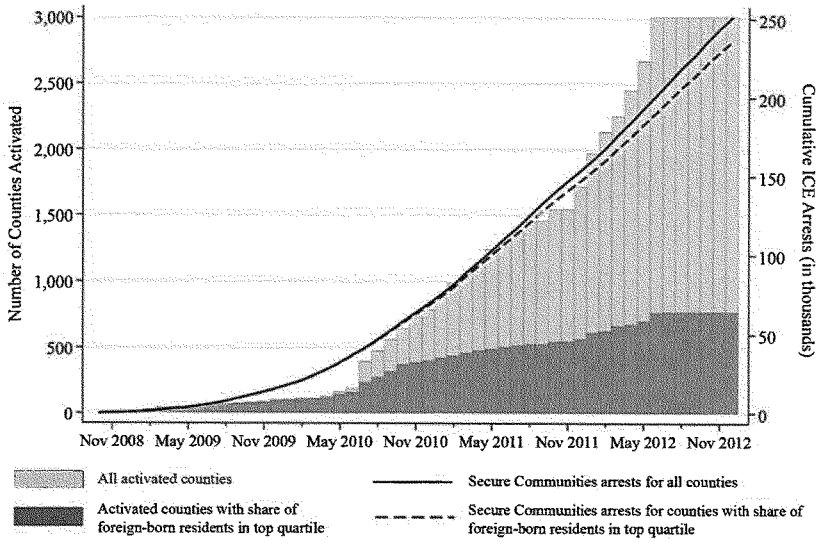


Figure 3. Activated counties and persons taken into Immigration and Customs Enforcement custody under Secure Communities.

ond, there is significant heterogeneity in the use of detention across counties. The dashed line indicates the total number of detentions occurring in counties with foreign-born population shares in the 75th percentile. The line for these counties lies nearly on top of that for the nationwide total; that is, nearly all detentions under Secure Communities occurred in counties with the highest proportions of foreign-born population.

Figure 3 also shows the overall magnitude of Secure Communities. Between its launch and the end of 2012, Secure Communities led to the detention of just over 250,000 immigrants nationwide. This is more people than are currently in federal prison, which today incarcerates about 216,000 inmates.[13] The comparison to the entire U.S. criminal justice system is also remarkable: Secure Communities over this period detained 1.13 percent of all noncitizens in the United States,[14] while approximately .71 percent of the U.S. population is currently incarcerated in prison and jails (Glaze and Herberman 2013)—and the U.S. incarceration rate is one of the highest in the world (Walmsley 2013). Secure Communities is an enormous social intervention.

[13] See Federal Bureau of Prisons, Statistics (http://www.bop.gov/about/statistics/population_statistics.jsp).

[14] Secure Communities detentions are roughly .63 percent of the foreign-born population in the United States. Acosta, Larsen, and Grieco (2014) are the source for the noncitizen population, and Grieco et al. (2012) are the source for the foreign-born population. Because the foreign-born population includes naturalized citizens who are no longer at risk of deportation, the noncitizen population is a more appropriate denominator—though this is a measure that the Census Bureau calculates only at the state and national levels.

5. Data and Empirical Strategy

The data set is a panel of monthly, county-level observations from 2004 to 2012. The observation period terminates with the final year of available crime data, and the start date was chosen to balance roughly the number of years before and after the launch of Secure Communities in late 2008. Each county-month observation includes three types of information: crime data, demographic and other county-level characteristics, and measures of the intensity of the program's treatment.

Crime data are the index offenses from the FBI's Uniform Crime Reports (UCR) (FBI 2004–12b). The index consists of violent crimes (murder, rape, robbery, and aggravated assault) and property crimes (burglary, larceny, and motor vehicle theft). The FBI was also the source for annual counts of the number of police (sworn officers) by county (FBI 2004–12a). Maltz and Targonski (2002) and Maltz (2006) document the presence of missing values in the county-level monthly UCR and the difficulties of imputing these missing values. A relatively simple imputation or smoothing procedure was used here. When data were reported only quarterly, biannually, or annually, the reported figure was averaged over the relevant time period, and that average value was assigned to each month. Missing values necessitated this reallocation for only about 7 percent of the observations in the sample, and, as shown below, this imputation does not affect the conclusions.

Demographic characteristics of each county and information on each county's median income were obtained from the Census Bureau.[15] Table 1 reports summary statistics for these variables. It is worth noting that the Census Bureau does not determine the number of noncitizens in each county. It does determine for each county the number of foreign-born and Hispanic persons, and these groups are closely correlated with immigrant status. Measures of income, foreign birth, and household composition were gathered from decennial censuses and linearly interpolated. Table 1 also reports the summary statistics for these characteristics.

Data on the Secure Communities program were obtained from DHS. The date of each county's activation under Secure Communities was publicized by the agency. Through several FOIA requests, we obtained from DHS tallies of the immigrants detained under Secure Communities during each month in each county since the program's launch. Immigration and Customs Enforcement classifies detainees into four groups according to their criminal histories. Level 1 (L1) includes noncitizens convicted of a single aggravated felony, a set of offenses defined in the Immigration and Nationality Act, or of two or more (nonaggravated) felonies. The category of aggravated felonies is complex and not easily summarized, but it includes violent felonies like murder and rape, as well as some property crimes and a wide swath of drug offenses (see 8 U.S.C. sec. 1101[a][43]).[16]

[15] U.S. Census Bureau, USA Counties Data File Downloads (http://www.census.gov/support/USACdataDownloads.html).

[16] For example, theft and burglary are treated as aggravated felonies if the offense is one "for which the term of imprisonment is at least one year," but not otherwise (8 U.S.C. sec. 1101[a][43][G]).

Table 1
Summary Statistics for Counties

Variable	Mean	SD
Secure Communities enforcement:		
Activated	.2359	.4226
Detentions per 100,000 (all counties)	14.3209	60.5598
Detentions per 100,000 (only activated counties)	59.6907	112.1534
Fraction foreign born	.1225	.1051
Fraction Hispanic	.1582	.1671
Fraction black	.1205	.1232
Population density	1,067.42	1,636.87
Median household income ($)	51,035.04	1,3551.73
Fraction of female-headed households	.1890	.0512
Sworn officers per 100,000	7.263	5.125
287(g) Agreement	.2631	.4403

Note. Observations are monthly county-level data from 2004–12. N = 302,388; number of counties = 2,985. Values are weighted by population.

All of the violent index crimes—murder, rape, robbery, and aggravated assault—qualify as aggravated felonies. Level 2 (L2) includes noncitizens convicted of any felony that is not an aggravated felony or convicted of three or more misdemeanors. Level 3 (L3) includes noncitizens convicted of one or two misdemeanors. The final category, noncriminal immigration violations, includes noncitizens who have no recorded criminal conviction. These noncitizens have only civil violations of immigration law, such as overstaying a visa or entering the country without inspection (ICE 2013).

Figure 4 shows the proportion of all detainees in these categories by month since the program's launch. Immigrants with no convictions compose nearly a third of all Secure Communities detainees. Those in the lower criminal categories, L2 and L3, make up another 40 percent. The most severe offenders account for only 28 percent of detainees. Figure 4 also demonstrates that these percentages have remained relatively stable over time. The comparatively low share of detainees with aggravated felony or multiple felony convictions does not necessarily contradict ICE's repeated public statements that it prioritizes the most serious offenders under Secure Communities. Rather, it likely reflects two factors: the underlying pool of arrestees and the requirement that immigrant offenders serve their full state criminal sentences before the federal government takes custody of them to pursue to deportation proceedings. This requirement delays ICE's apprehension of the most serious offenders since, in expectation, those arrestees will serve the longest sentences.

To control for earlier forms of federal-local cooperation, we also assembled by hand the dates and locations of 287(g) agreements by reading the agreements on ICE's Web site.[17] Immigration and Customs Enforcement made these agreements with local police and sheriff's departments and with states' prison systems. We

[17] U.S. Immigration and Customs Enforcement, Delegation of Immigration Authority Section 287(g) Immigration and Nationality Act (http://www.ice.gov/factsheets/287g).

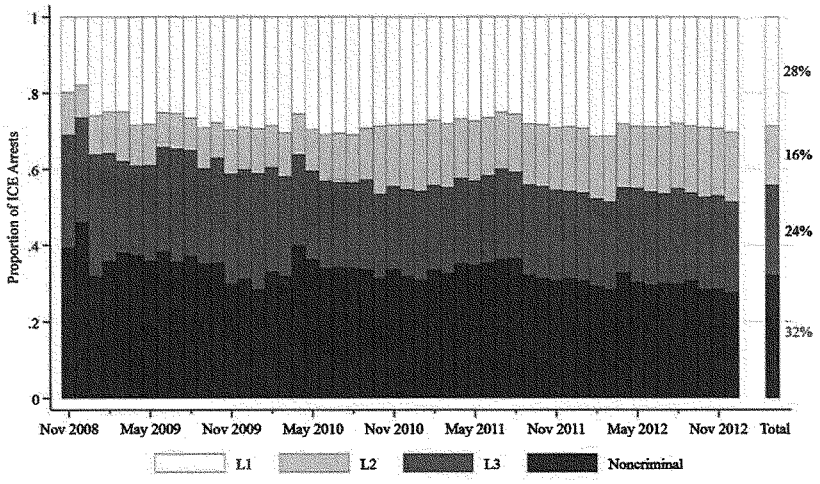


Figure 4. Immigration and Customs Enforcement classifications of persons taken into federal custody under Secure Communities.

created a binary variable for the presence of any active 287(g) agreement in a county that takes a value of one when there was an agreement with a law enforcement agency in the county or at the state level and is zero otherwise.[18]

The core sample consists of 84 monthly observations of 2,985 counties—a sample size of 336,204. But the tallies of police employment were not available for every county in each year, and controlling for police employment seemed particularly important because the program piggybacks on arrests by local police. Therefore, the baseline sample consists of 302,388 observations in an unbalanced panel.

To control for the numerous influences on a county's crime rate, we estimate ordinary least square regressions. The estimating equation takes the form

$$\ln C_{it} = g(\text{Activate}_{it})\delta + X_{it}\beta + \alpha_i + \alpha_t + \varepsilon_{it}, \tag{3}$$

where $\ln C_{it}$ is the natural log of the crime rate in county i in calendar month t.[19] When a county experienced no offenses in a particular month, the dependent variable is assigned a value of one, and these observation are permitted to have a separate intercept.

The term Activate_{it} is a variable representing whether Secure Communities is active in county i on date t. Several different functional forms of $g(\cdot)$ are used to

[18] In the regression analysis, the coefficient on this variable is a differences-in-differences estimate of the impact of a 287(g) agreement. These estimates are not reported in order to conserve space, but they suggest that 287(g) agreements had at best a modest crime-reducing effect. For example, in the regression for activated counties in column 1 of Table 2, the estimate for a 287(g) agreement is −.0322 (SE = .0144), and with county-level trends it was −.0136 (SE = .0096).

[19] As is standard, a crime's rate is defined as the number of offenses per 100,000 people.

capture the activation of Secure Communities. The simplest is an indicator variable that takes the value one when a county has been activated and zero otherwise, and in this case the coefficient δ is the differences-in-differences estimate of the program's impact. In other specifications, activation is expressed with two indicators or two differences-in-differences estimates in order to isolate locations where immigrant populations are largest and where any impacts of Secure Communities should be strongest. In other regressions, the indicator variables are replaced with direct measures of the intensity of Secure Communities intervention. As described above, this is the cumulative number of persons detained under the program in the county since Secure Communities was activated. To parallel the standard measure of crime rates, we express this as the cumulative number of detainees per 100,000 persons, and in the regressions, we express this as a natural logarithm.

The vector $\boldsymbol{X}_{it}$ contains a set of county- and date-varying control variables that are commonly included in studies of crime. These are shown in Table 1, with the exception that police employment, income, and population density are expressed in logs in the regressions. The terms α_i and α_t are fixed effects for county i and calendar date t, respectively. As discussed below, an extension of equation (3) includes county-specific trends. The term ε_{it} captures the error. The regressions are weighted by a county's population, and the standard errors are clustered by county.

6. Results

6.1. Estimates for Aggregate Crime Rates

Table 2 reports the differences-in-differences estimates for the total index crime rate. The data imply that a county's activation under Secure Communities is associated with a statistically significant decline of 4 percent in the crime rate. When the basic estimate is decomposed into two components, one for counties that are likely to have high immigrant concentrations and one for counties likely to have low concentrations, almost all of the 4 percent decline observed in the baseline estimate is due to reductions in the counties with high concentrations of foreign-born people. The other counties experienced no decreases in crime following Secure Communities activation; in fact, the estimate for these counties is positively signed. A similar comparison for counties on and not on the southern border shows a very large decline in border counties, about 19 percent, following activation, compared to a statistically insignificant effect of 3 percent in nonborder counties. In sum, the basic differences-in-differences estimates suggest that the activation of Secure Communities in counties with high concentrations of foreign-born persons is associated with sizable declines in crime.

The period of Secure Communities activation, 2008–12, was a time of falling crime rates nationally. This raises the possibility that places chosen for earlier activation were locations where crime was falling fastest.[20] We test this possibility

[20] For example, if the federal government targeted the border for reasons related to immigration enforcement rather than crime (as suggested by Cox and Miles 2013), but crime rates happened

Table 2
Impact of Secure Communities on Rate of Total Index Crime: Differences-in-Differences Estimates for Alternative Measures of Activation

	Log Level		Log Change
	(1)	(2)	(3)
Baseline regression: Activated	−.0400**	.0025	.0025**
	(.0173)	(.0118)	(.0010)
Foreign-born population regression:			
Activated × 75th Percentile Foreign Born	−.0545**	−.0024	.0039**
	(.0191)	(.0144)	(.0010)
Activated × Not 75th Percentile Foreign Born	.0067	.0120	−.0016
	(.0154)	(.0095)	(.0014)
Southern border regression:			
Activated × Border County	−.1887**	−.0067	.0080**
	(.0282)	(.0569)	(.0019)
Activated × Not Border County	−.0312	.0028	.0023
	(.0170)	(.0114)	(.0011)
County-level trends	No	Yes	No

Note. The dependent variable is the log of the monthly index crime rate. Values are ordinary least squares regression coefficients, with standard errors in parentheses. All specifications include county fixed effects. N = 302,388; number of counties = 2,985.
** $p < .05$.

by including county-specific trends. These specifications eliminate variation in crime rates caused by factors that vary linearly over time and that are specific to individual counties. Identification of the impact of Secure Communities in these equations comes from within-county variation after netting out county-specific trends. The county trends have a dramatic effect on the estimates. Their inclusion drives all the differences-in-differences estimates to 0, and they lose statistical significance. The baseline estimate, for example, falls from −.0400 to .0025. Column 3 of Table 2 verifies this pattern by specifying the continuous variables on both the left- and right-hand sides in first differences. This is akin to county-level trends because when the dependent variable is first differenced, the county fixed effects capture trends. Here, the estimate again reverses sign from the baseline result, becoming positive but remaining small, less than .003.

To obtain a clearer portrait of the time series of crime rates around the dates of activation in counties with high concentrations of immigrants, we estimated a specification analogous to an event study. We restrict the data for each county to observations between 75 months before activation and 40 months after it and created a series of indicator variables for each month relative to the date of activation in counties with high concentrations of foreign-born people.[21] The indicator for the nth month relative to the activation date takes a value of one in counties at or above the 75th percentile of the share of the foreign-born population during month n and a value of zero otherwise. These indicators are equivalent to a set of lead and lag variables for the month of activation for counties with large

to be falling faster along the border than in other parts of the country, the basic differences-in-differences estimates could wrongly lead one to attribute falling crime rates to program activation.

[21] This restricts the sample to 259,097 observations.

foreign-born populations. We include them in a regression that has the same set of controls as in equation (3). This specification captures the time path of crime rates in counties where Secure Communities produced the most detentions relative to other counties.

Figure 5 presents the coefficients for the indicators. It shows a steady downward decline in crime rates that begins well before the activation date and continues through the end of the observation period. The figure clarifies why including county trends has such a dramatic effect on the differences-in-differences estimates. Total index crime rates in counties with the highest concentrations of foreign-born people were declining before the rollout of Secure Communities began, and the decline continued steadily throughout the rollout period.

Table 3 presents data for the direct measures of immigrant detentions under Secure Communities. As described above, we measure intensity as the cumulative number of persons detained under the program in the county since Secure Communities was activated. To parallel the standard measure of crime rates, we express this as the cumulative number of detainees per 100,000 persons. Accordingly, we use a variable that in the months after a county's activation takes the value of the natural log of this ratio and in the months before a county's activation is assigned a value of zero.[22] In each month following activation in a county, this variable captures how many noncitizens have been incapacitated by the program through that month. Column 1 shows a negative relationship between Secure Communities detentions and crime. The coefficient in this log-log regression equation can be read as an elasticity, and its magnitude, −.0163, is smaller than analogous estimates for prison populations (Johnson and Raphael 2012).

The negative relationship in the baseline specification is due almost entirely to counties with the highest concentrations of foreign-born persons. This is consistent with the patterns of Figure 3: nearly all Secure Communities detentions occurred in counties with the highest shares of foreign-born population. Table 3 also shows that the association between crime and detentions in column 1 is most negative in counties directly on the southern border. A persistent criminological finding is that criminal history predicts recidivism (Gendreau, Little, and Goggin 1996), and therefore one might expect detentions of persons with more serious criminal histories to be associated with more substantial declines in crime. The estimates decomposed by ICE classifications provide some support for this view. The coefficient on L2 and L3 detainees is slightly less negative than that for L1 detainees. The specification for detentions of immigrants without any criminal convictions has a slightly positive estimate, but none of the estimates for detentions in this specification achieve statistical significance at the conventional .05 level.

In column 2, the same specifications are repeated but including county-specific trends. As with the differences-in-differences estimates in Table 2, the introduc-

[22] The indicator for activation under Secure Communities is excluded from these regressions, which in a few instances creates a nonmonotonicity in the measure of the program when the number of detentions is small. Although not shown here in order to conserve space, the estimates are not sensitive to the exclusion of the program indicator, and the more parsimonious specification was used.

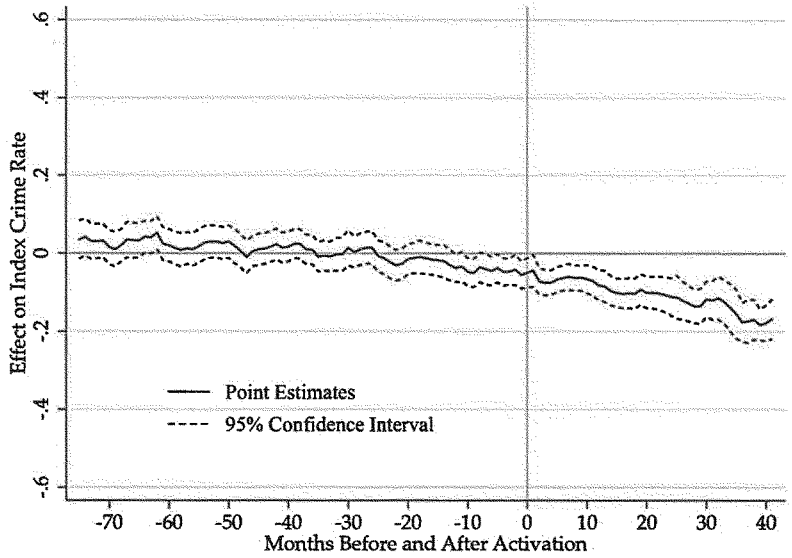


Figure 5. Log total index crime rates in counties with foreign-born population shares in the 75th percentile.

tion of county trends drives the estimates to 0; none are statistically significant. Many of them fall by more than an order of magnitude. The estimate for persons in ICE custody, for example, drops from −.0163 to −.0006. The precision of this estimate is worth noting. Its standard error implies that that even a coefficient as small as .009 would be statistically significant at the 5 percent level. Suggestive patterns, including the correlation between detainees' criminal history classification and the level of crime reduction, disappear.

The regressions in column 3 confirm the importance of crime trends within each location. In these regressions, the data are again expressed in first differences, and in effect, they relate changes in crime to the flow of new detainees under the auspices of Secure Communities. As with column 2, none of the coefficients imply a decline in crime rates. The only estimate above standard levels of significance is positively signed. The analysis of detentions thus confirms what the differences-in-differences estimates indicate: after accounting for each county's trend, the evidence shows that Secure Communities has not reduced the total index crime rate.

6.2. Robustness of Estimates

Table 4 probes the sensitivity of the estimates to alternative specifications. It takes as a baseline the equation in column 2 of Table 3, which employs county-level trends. Each column in Table 4 shows the coefficient on the (log of) the detention rates under Secure Communities or, where noted, a variation of it.

Table 3
Impact of Secure Communities on Rate of Total Index Crime: Measures of Activation Interacted with Immigration and Customs Enforcement (ICE) Custody

	Log Level		Log Change
	(1)	(2)	(3)
Baseline regression: Persons in ICE Custody × Activated	−.0163**	−.0006	−.0013
	(.0051)	(.0045)	(.0029)
Foreign-born population regression:			
Persons in ICE Custody × Activated × 75th Percentile Foreign Born	−.0165**	−.0006	−.0017
	(.0052)	(.0048)	(.0036)
Persons in ICE Custody × Activated × Not 75th Percentile Foreign Born	−.0067	−.0007	−.0004
	(.0063)	(.0045)	(.0037)
Southern border regression:			
Persons in ICE Custody × Activated × Border County	−.0410**	−.0013	.0121**
	(.0042)	(.0112)	(.0042)
Persons in ICE Custody × Activated × Not Border County	−.0134**	−.0006	−.0021
	(.0054)	(.0047)	(.0030)
Regression for categories of ICE custody:			
Persons in L1 Category in ICE Custody × Activated	−.0189*	.0036	−.0013
	(.0116)	(.0053)	(.0034)
Persons in L2 and L3 Categories in ICE Custody × Activated	−.0144	−.0059	.0044
	(.0091)	(.0054)	(.0027)
Persons in Noncriminal Category in ICE Custody × Activated	.0132	.0022	−.0037
	(.0100)	(.0057)	(.0028)
County-level trends	No	Yes	No

Note. The dependent variable is the log of the monthly index crime rate. Values are regression coefficients, with standard errors in parentheses. All specifications include county fixed effects. $N = 302,388$; number of counties = 2,985.
* $p < .1$.
** $p < .05$.

Table 4 first assesses the sensitivity of the estimates to two measurement issues. Missing values for the size of the local police force cause the panel to be unbalanced. Column 1 reports a regression in which the policing variable is excluded, and the sample is the balanced panel of counties by months. In column 2, the total index crime rate is calculated without adjusting for missing values in the UCR. Neither of these changes alters the inference drawn about the program's impact on crime. The coefficients are slightly more negative than the baseline estimate of −.0006, but both remain small (less than .01 in absolute value) and statistically insignificant.

Columns 3–5 report results from alternative measures of the program's intensity. The regression in column 3 replaces the cumulative number of immigrants taken into federal custody under Secure Communities with the number taken into custody during that month. In effect, it measures the monthly flow of immigrants into custody under the program rather than the number of immigrants in custody. If the program shapes offending rates principally by changing

Table 4

Impact of Secure Communities on Rate of Total Index Crime: Sensitivity of Estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Persons in ICE Custody	−.0013	−.0020	−.0038		.0066			
	(.0050)	(.0033)	(.0059)		(.0044)			
Persons Deported				−.0010	−.0029			
				(.0043)	(.0041)			
Persons in ICE Custody × Local Immigrant Policy						−.0064	−.0010	−.0055
						(.0042)	(.0048)	(.0042)
Persons in ICE Custody × No Local Immigrant Policy						.0082	.0003	.0057
						(.0064)	(.0055)	(.0064)
Change to baseline specification	Excludes police per capita	Crime measured without imputation	Persons in ICE custody measured as flow					
Local immigrant policy						Antidetainer	Immigrant sanctuary	Antidetainer or sanctuary

Note. The dependent variable is the log of the monthly index crime rate. Values are regression coefficients, with standard errors in parentheses. The baseline regression specification is the regression in Table 3, column 2, which includes county-specific trends. In column 1, N = 306,244; number of counties = 3,114. In columns 2–8, N = 302,388; number of counties = 2,985.

the probability of ICE apprehension, then this flow measure would be a more appropriate measure of the policy intervention. The regression in column 4 considers nearly the opposite case. It employs the cumulative number of deported immigrants rather than detained immigrants. This measure would be appropriate if Secure Communities increased only deportations rather than detentions, but as described above, it increases both. Column 5 includes both the cumulative detention and deportation measures. A possible theoretical justification for including both measures is that it permits us to disentangle the effects of shorter- and longer-term incapacitation. Yet such fine theoretical distinctions may not be possible in practice since ICE detains immigrants before deporting them, which makes these measures highly correlated. None of these specifications suggest a different conclusion about Secure Communities' impact. All the estimates imply small effects on crime, and none are statistically significant.

Since 2012, the popular media's coverage of Secure Communities has often focused on local resistance to the program. As described above, local law enforcement agencies have no way to resist Secure Communities' core mechanical feature of screening all local arrestees for federal immigration violations, short of refusing to check arrestees' fingerprints. But local political authorities can resist the program in other ways.

First, they could adopt policies that interfere with ICE's ability to take custody of immigrants identified through Secure Communities. Under such a policy, the local authorities would refuse to honor ICE detainer requests—that is, requests by ICE that local authorities hold immigrants for up to 48 hours after they would ordinarily be released, in order to give ICE time to take physical custody of them. Refusing to honor a detainer makes it more likely that a person will be released from local custody before ICE is able to take custody of her. Antidetainer policies were rare before 2013, but they expanded rapidly in 2014 following a decision by a federal court in Oregon concluding that some detainers violate arrestees' Fourth Amendment rights (Medina 2014). We measure these policies using a comprehensive list assembled by the Catholic Legal Immigration Network, Inc. (2014).[23]

A second way in which a local government might resist Secure Communities is to adopt a sanctuary policy. A sanctuary policy restricts local participation in immigration enforcement in various ways, and it is potentially broader than an antidetainer policy insofar as it may limit other forms of local enforcement or cooperation.[24] The adoption of sanctuary policies occurred primarily before the launch of Secure Communities, and Trevor Gardner provided us with a hand-

[23] There is considerable variation in the stringency of these antidetainer policies. As the Catholic Legal Immigration Network, Inc. (2014) describes them, they "range from broad limitations prohibiting local law enforcement from honoring any ICE detainer requests to more narrow measures restricting compliance to cases where ICE has obtained a warrant from a judge backed by probable cause, or when ICE has agreed to reimburse the locality for the costs of the detention, or when the individual has been convicted of a certain felony or other serious crime."

[24] As with antidetainer policies, there is significant variation in the degree to which any given policy restricts local enforcement collaboration.

assembled list of sanctuary policies adopted between 2001 and 2008.[25] It is worth noting that only about 10 percent of counties have adopted either of these policies, and they are correlated: 55 percent of counties with sanctuary policies also adopted antidetainer policies.

While the timing of these policies prevents us from obtaining differences-in-differences estimates of their effects, we can interact the Secure Communities variables with dummies for the presence of these policies. This allows us to test whether Secure Communities had a different impact in counties that adopted such policies (or will adopt them shortly after our sample ends) than in other counties. If Secure Communities was effective in reducing crime and if local resistance impeded its efficacy, any drops in crime rates should be concentrated in counties without these polices. The regressions provide no support for this prediction. The coefficients for counties with these policies are negatively signed, while those without these policies are positively signed. None of them are statistically significant, and all of them imply changes in crime that are smaller than 1 percentage point.

6.3. Estimates for Individual Crime Rates

The existing literature on immigration and crime has devoted most attention to homicides and aggregate categories of violent and total index crimes. Under Secure Communities, large numbers of immigrants with histories of less serious offending or no offending at all have been detained. For these persons, less severe crimes such as property crimes may be more relevant. Tables 5 and 6 present a series of specifications for each of the seven categories of offenses constituting the FBI's index. Table 5 contains results for four categories of violent crimes, and Table 6 has the results for a fifth category of violent crime and for property crimes.

The patterns for the individual offense categories largely mirror those for the total crime index. The violent-crime categories show a high degree of sensitivity to the presence of county-level trends, often in opposite directions. For example, for persons in ICE custody, the estimate for aggravated assault moves closer to 0, from −.0208 to −.0074, when the regression includes county trends, while that for robbery becomes more negative, moving from −.0118 to −.0233. To obtain a clearer view of the time path of crime rates in counties with a high proportion of foreign-born people, event-study estimates analogous to those shown in Figure 5 were estimated for the rates of individual offenses. Figure 6 shows the patterns for offenses in the violent-crime category. The estimates for aggravated assault show a steady, almost linear, decline, which is consistent with estimates moving from negative to 0 in the presence of county-specific trends in Table 5. Other categories have less clearly linear patterns. The robbery rate, for example, is flat at the beginning of the observation window, appears to rise toward the middle, and falls at its end.

[25] Trevor Gardner, faculty fellow at New York University Law School, e-mail to the authors, June 19, 2014.

Table 5

Impact of Secure Communities on Rates of Violent Offenses

	Homicide		Rape		Robbery		Aggravated Assault	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline regression: Activated	−.0162	.0448**	−.0133	−.0187	−.0206	.0048	−.0432**	.0033
	(.0296)	(.0214)	(.0294)	(.0194)	(.0144)	(.0109)	(.0204)	(.0109)
Main regression for ICE custody: Persons in ICE Custody × Activated	−.0260**	−.0130*	−.0099	−.0208**	−.0118**	−.0233**	−.0208**	−.0074*
	(.0081)	(.0078)	(.0073)	(.0074)	(.0042)	(.0048)	(.0072)	(.0043)
Foreign-born population regression:								
Persons in ICE Custody × Activated × 75th Percentile Foreign Born	−.0262**	−.0170**	−.0098	−.0215**	−.0118**	−.0260**	−.0212**	−.0072*
	(.0082)	(.0083)	(.0073)	(.0075)	(.0043)	(.0050)	(.0072)	(.0045)
Persons in ICE Custody × Activated × Not 75th Percentile Foreign Born	−.0184*	.0147	−.0113	−.0158	−.0110	−.0045	−.0064	−.0088
	(.0101)	(.0123)	(.0127)	(.0122)	(.0087)	(.0087)	(.0095)	(.0089)
Southern border regression:								
Persons in ICE Custody × Activated × Border County	−.0250*	−.0249	.0019	−.0165	−.0162**	−.0167**	−.0140**	.0158**
	(.0128)	(.0177)	(.0089)	(.0153)	(.0043)	(.0075)	(.0053)	(.0053)
Persons in ICE Custody × Activated × Not Border County	−.0261**	−.0122	−.0112	−.0211**	−.0113**	−.0238**	−.0216**	−.0091**
	(.0087)	(.0081)	(.0076)	(.0075)	(.0045)	(.0050)	(.0077)	(.0045)
Regression for categories of ICE custody:								
Persons in ICE Custody × L1 Persons in ICE Custody × Activated	−.0308**	−.0168	−.0056	−.0074	−.0261	−.0074	−.0314**	−.0034
	(.0155)	(.0145)	(.0136)	(.0111)	(.0145)	(.0096)	(.0168)	(.0090)
Persons in ICE Custody × L2/L3 Persons in ICE Custody × Activated	.0066	−.0016	.0129	−.0181	.0078	−.0168	−.0026	−.0100
	(.0157)	(.0183)	(.0153)	(.0125)	(.0129)	(.0112)	(.0135)	(.0100)
Persons in ICE Custody × Noncriminal Persons in ICE Custody × Activated	−.0146	−.0120	−.0221	−.0030	.0021	−.0070	.0068	.0058
	(.0149)	(.0183)	(.0143)	(.0113)	(.0125)	(.0092)	(.0123)	(.0095)
County-level trends	No	Yes	No	Yes	No	Yes	No	Yes

Note. The dependent variable is the log of the monthly index crime rate. Values are coefficients from ordinary least squares regressions, with standard errors in parentheses. All regressions include county fixed effects. $N = 302{,}388$; number of counties = 2,985.

* $p < .1$.

** $p < .05$.

Table 6

Impact of Secure Communities on Rates of Simple Assault and Property Offenses

	Simple Assault		Property Offenses					
			Burglary		Larceny		Motor Vehicle Theft	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline regression: Activated	−.0269**	−.0034	−.0411**	−.0032	−.0342**	.0083	−.0617**	−.0161
	(.0106)	(.0078)	(.0181)	(.0085)	(.0154)	(.0074)	(.0277)	(.0111)
Main regression for ICE custody: Persons in ICE Custody × Activated	−.0156**	−.0044	−.0185**	−.0146**	−.0127**	.0006	−.0173**	−.0230**
	(.0036)	(.0029)	(.0047)	(.0038)	(.0043)	(.0031)	(.0085)	(.0044)
Foreign-born population regression:								
Persons in ICE Custody × Activated × 75th Percentile Foreign Born	−.0156**	−.0039	−.0189**	−.0162**	−.0130**	−.0004	−.0175**	−.0234**
	(.0037)	(.0034)	(.0047)	(.0041)	(.0044)	(.0034)	(.0085)	(.0047)
Persons in ICE Custody × Activated × Not 75th Percentile Foreign Born	−.0140*	−.0079	−.0063	−.0030	−.0012	.0081*	−.0105	−.0204**
	(.0077)	(.0058)	(.0074)	(.0059)	(.0067)	(.0043)	(.0113)	(.0075)
Southern border regression:								
Persons in ICE Custody × Activated × Border County	−.0245**	.0033	−.0357**	−.0209**	−.0347**	.0226	−.0633**	−.0864**
	(.0064)	(.0050)	(.0063)	(.0066)	(.0055)	(.0259)	(.0094)	(.0170)
Persons in ICE Custody × Activated × Not Border County	−.0145**	−.0050*	−.0166**	−.0141**	−.0101**	−.0010	−.0120	−.0183**
	(.0038)	(.0030)	(.0048)	(.0040)	(.0043)	(.0032)	(.0088)	(.0043)
Regression for categories of ICE custody:								
Persons in ICE Custody × L1 Persons in ICE Custody × Activated	−.0134	−.0010	−.0304**	−.0158**	−.0242**	−.0005	−.0100	.0061
	(.0120)	(.0066)	(.0116)	(.0072)	(.0112)	(.0047)	(.0166)	(.0092)
Persons in ICE Custody × L2/L3 Persons in ICE Custody × Activated	.0027	−.0020	−.0137	−.0020	−.0034	.0047	−.0133	−.0235**
	(.0127)	(.0069)	(.0104)	(.0073)	(.0091)	(.0063)	(.0159)	(.0098)
Persons in ICE Custody × Noncriminal Persons in ICE Custody × Activated	−.0104	−.0037	.0201*	−.0053	.0111	−.0042	.0037	−.0120
	(.0100)	(.0060)	(.0107)	(.0065)	(.0099)	(.0052)	(.0151)	(.0095)
County-level trends	No	Yes	No	Yes	No	Yes	No	Yes

Note. The dependent variable is the log of the monthly index crime rate. Values are coefficients from ordinary least squares regressions, with standard errors in parentheses. All regressions include county fixed effects. $N = 302{,}388$; number of counties = 2,985.

* $p < .1$.

** $p < .05$.

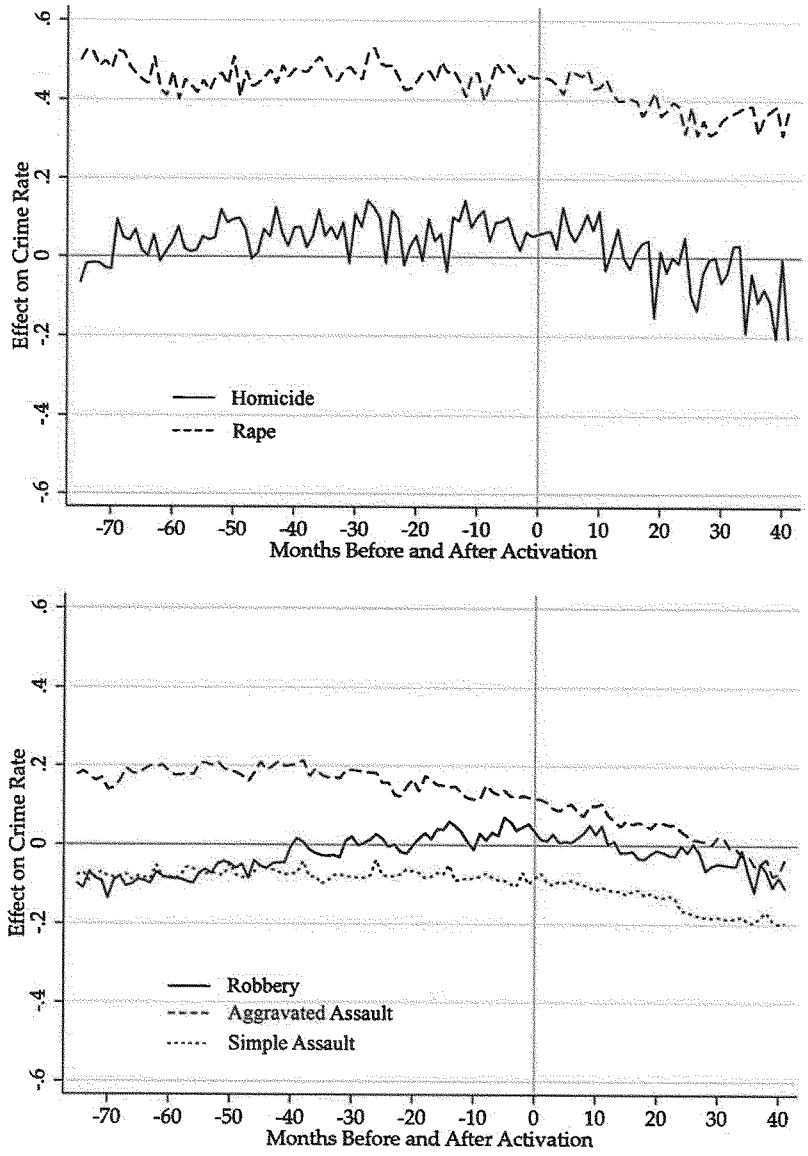


Figure 6. Log violent-crime rates in counties with foreign-born population shares at or above the 75th percentile.

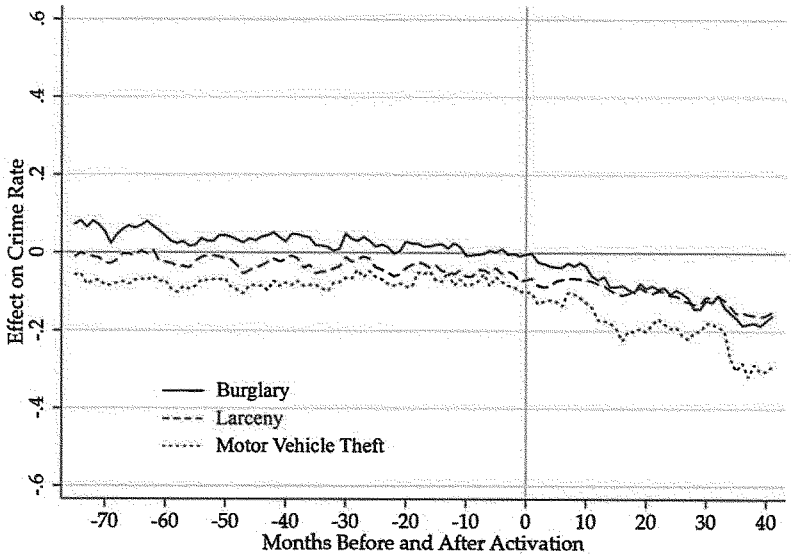


Figure 7. Log property-crime rates in counties with foreign-born population shares at or above the 75th percentile.

Simple assault is not an element of the FBI's crime index, but it is an offense that occurs frequently. It often reflects public disorder, and immigration figures prominently in social disorganization theories of crime (Shaw and McKay 1942; Sampson 2008). Figure 6 shows that rates of simple assault exhibit a decline that is similar to that of aggravated assaults but less pronounced. The rate of simple assaults appears to decline about 30 months before the activation of the program, and accordingly, the estimates in Table 6 for simple assaults move from negative to 0 in the presence of county-level trends.

Table 6 also displays the patterns for property crimes, which have received less attention in the literature on immigration and crime. In most specifications without county trends, property offense rates have negative and statistically significant correlations with Secure Communities activation and detentions. Two of them—burglary and motor vehicle theft—continue to have negative and statistically significant estimates in the presence of county trends. In addition, the estimates for these two crime categories are more negative in counties with the highest concentrations of foreign-born people and counties on the southern border.

To determine why these two negative estimates are robust to the presence of county trends, Figure 7 shows estimates for the time paths of the three property offenses. All three of these crime rates are relatively stable for at least 4 years before the activation of Secure Communities, and in the months after activation they all experience steep declines. If the declines commenced on the date of Se-

cure Communities activation or shortly thereafter, it would suggest that the program was the causal mechanism for the declines. The trend for burglary most closely matches this prediction, with declines appearing to commence right around the time of activation. In contrast, the larceny rate does not conform to the prediction: the downward slope is evident about 20 months before the activation date. The rate of motor vehicle theft is an intermediate case: it appears to decline 10 months before activation, but after activation the drop accelerates. These estimates provide some support for the conclusion that the detention of immigrants under Secure Communities reduced two categories of property crime: burglary and motor vehicle theft.

6.4. Interpreting the Results

In short, the evidence shows that Secure Communities did not cause a meaningful reduction in the FBI's overall index crime rate. Nor did it reduce the rate of any individual violent offense. This is important because Secure Communities specifically, and criminal deportation policies more generally, have long been publicly justified primarily on grounds that they keep communities safer from violent crime. At the same time, we find suggestive evidence that Secure Communities is associated with modest reductions in two property offenses. Homicide and violent crimes, rather than property crimes, have typically been the focus of the empirical literature on the connection between immigration and crime (for example, Lee, Martínez, and Rosenfield 2001; Sampson 2008; Martínez, Stowell, and Lee 2010).

If the estimated declines in rates of burglary and motor vehicle theft are interpreted as the causal effects of the program rather than artifacts of downward trends in crime, the question arises how one might evaluate the significance of the estimates. One approach is to compare the crime-reducing potential of criminal incarceration with detention through Secure Communities. From this perspective the estimates denote quite modest effects on these crimes. Evaluated at the sample mean, they imply that the annual detention of one additional immigrant prevents .18 of an additional burglary and .12 of an additional motor vehicle theft. By comparison, Johnson and Raphael (2012) estimate that during the period 1991–2004, the annual imprisonment of one additional criminal prevents about .514 of a burglary and about .505 of a motor vehicle theft. (In addition, they find that increasing imprisonment by one criminal prevents approximately 1.5 larcenies and .3 of a violent crime.) This implies that the marginal Secure Communities detainee is a far less frequent and serious offender than the marginal prison inmate.

Put differently, these estimates imply that Secure Communities prevented a very small share of criminal offenses nationwide. The growth of the detainee population and the estimated elasticities in Table 6 suggest that Secure Communities has prevented 65,586 burglaries and 46,093 motor vehicle thefts since its launch. These figures represent .75 percent and 1.55 percent of the burglaries and motor vehicle thefts committed, respectively, nationwide between 2009 and 2012.

Another way of assessing the estimates—one that circles back to the literature on the relative rates of offending by immigrants and citizens—is to consider their implications for the difference in offending patterns of immigrants and native-born people. To see the relevant parameters, it is helpful to rearrange equation (2):

$$\beta_I - \beta_N = (\partial\alpha_I/\partial S)^{-1}(\partial\beta/\partial S - \alpha_I\partial\beta_I/\partial S). \quad (4)$$

The left-hand side of the equation now represents the gap in the two groups' offending rates. All of the parameters on the right-hand side, except one, are known or estimated in the empirical analysis. The main finding of Tables 2 and 3 was the absence of any impact on aggregate crime rates, or $\partial\beta/\partial S \approx 0$. The term α_I is known because, as discussed above, immigrants constitute roughly 11 percent of the population. The FOIA disclosures from ICE show that Secure Communities has resulted in the detention of roughly 1.13 percent of the noncitizen population, which permits us to estimate $\partial\alpha_I/\partial S$.

The one unknown parameter on the right-hand side of equation (4) is $\partial\beta_I/\partial S$, which is the deterrent response of immigrants to the program. If this effect were 0, it would imply, given the other known values, that immigrants offend at rates similar to native-born people. Yet determining the magnitude of any deterrent effect is difficult, and there are few convincing estimates of deterrence, as distinguished from incapacitation (Miles and Ludwig 2007; Durlauf and Nagin 2011). A prominent estimate was produced by Kessler and Levitt (1999), who exploit variation in sentence enhancements for recidivists. Their estimates range from a 4 percent decline in crime in the first year following enactment of the enhancement to an 8 percent decline in 3 years. An implication of our estimates and equation (4) is simply that even modest deterrent responses to the program's threat of detention and deportation would imply that immigrants offend less frequently than native-born people.

7. Conclusion

The finding that Secure Communities does not reduce the overall rate of FBI index crime calls into question the long-standing assumption that deporting noncitizens who commit crimes is an effective crime-control strategy. Our estimates suggest that the marginal immigrant detainee is a much less serious offender than the marginal prisoner in the criminal justice system—even when that immigrant detainee has been selected for detention using a program designed to target the most serious immigrant criminal offenders. This lower offending rate undercuts the effect that incapacitation through deportation has on overall crime rates. Moreover, as explained earlier, the elasticity of immigrant offending rates to deportation policies is likely to be inversely correlated to the seriousness of the crime, which makes it less likely that such policies will effectively deter more serious, rather than less serious, crimes. Our estimates are consistent with this hypothesis, as the only index crimes for which there was even suggestive evidence

of a small reduction associated with Secure Communities were the less serious property crimes of burglary and perhaps motor vehicle theft.

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Sanctuary Cities and Crime

Yuki Otsu*†

The University of Tokyo

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Abstract

Sanctuary policies may attract criminals and lower the opportunity cost of crime, while these policies produce a spiral of trust that supports police and raises informal social control over crime. I estimate the dynamic effects of sanctuary policies on crime rates at city level. This paper finds (1) no evidence that sanctuary policies cause an increase in any crime, (2) some evidence that they may lead to a decrease in property crime, and the effect is strengthened over time after the adoption, (3) the increased trust between residents and police is likely the reason for the negative effect.

JEL Classification Code: J15, K37, R59
Keywords: sanctuary city, crime policy, undocumented immigrants

*Center for Spatial Information Science, The University of Tokyo. 5-1-5 Kashiwanoha, Kashiwa, Chiba 277-8568 Japan. Email: y.otsu@csis.u-tokyo.ac.jp

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The author declares that he has no relevant material or financial interests that relate to the research described in this paper.

1 Introduction

Sanctuary policies have gained more attention in recent policy debates. Although there is no single definition of a sanctuary city, it is generally considered to be a city that does not cooperate with the federal government in terms of immigration policies.[1] In debates about immigration policies, the sanctuary cities are often mentioned in the context of the safety of communities. One perspective says that sanctuary policies could raise crime. The U.S. Department of Homeland Security (2018) described sanctuary cities as follows:

> Non-cooperative jurisdictions that do not honor U.S. Immigration and Customs (ICE) detainer requests to hold criminal aliens who are already in their custody, endanger the public and threaten officer safety by releasing criminal aliens back into the community to re-offend. In addition to causing preventable crimes, this creates another "pull factor" that increases illegal immigration.

This statement points out that sanctuary policies release criminal aliens to the communities and make the city more attractive to illegal immigrants.

However, another perspective says that sanctuary policies do not raise the crime rate, and it could even decrease crimes. The predominant reason to implement a sanctuary policy is to encourage undocumented immigrants to report incidents when they become witnesses or victims. As the International Association of Chiefs of Police (2004) said,

> Many law enforcement executives believe that state and local law enforcement should not be involved in the enforcement of civil immigration laws since such involvement would likely have a chilling effect on both legal and illegal aliens reporting criminal activity or assisting police in criminal investigations. They believe that this lack of cooperation could diminish

[1] For a formal definition, see Section 3.1.

the ability of law enforcement agencies to effectively police their communities and protect the public they serve.

Although there are several papers investigating sanctuary cities and crime (Lyons et al., 2013; Wong, 2017; Martínez-Schuldt and Martínez, 2017; O'Brien et al., 2019), this paper investigates whether the adoption of sanctuary policies causes a change in crime rates for a city, filling gaps in the literature. So far, the literature lacks five things. First, we know little about the dynamic effects of sanctuary policies on crime rates. The pre-trend and dynamics of the effect are unclear. Residents might anticipate the policy adoption and changes their behavior, for example, through movement. Even after the adoption, people need some time to learn the policy or may forget it as time goes by. Second, the selection issue is not well investigated. Since sanctuary policies are a policy adopted through political decisions, there is likely a selection issue. For example, a higher population share of immigrants may let the city to adopt the policy. Or an efficient local government might adopt a sanctuary policy to reduce crime rates. Third, the analysis about crime type heterogeneity is limited. The empirical papers about sanctuary policies focus only on a few types of crime, such as homicide and robbery, and it remains unknown if the findings hold for other crimes. Fourth, the literature does not control for heterogenous trends in crime rates across geographic areas. For instance, California has more sanctuary cities, but the crime rate in that state is also declining. We need analyses to avoid simply capturing a trend specific to the states that have more sanctuary cities. Lastly, the reason of the effect is not well investigated. The existing papers mention potential reasons but these claims are not supported by data analysis.

For causal inference, I construct a panel data of U.S. cities from 1999 to 2010. Following the literature, sanctuary status is determined based on a list made by the National Immigration Law Center (NILC). Using data from the Uniform Crime Reports (UCR) from 1999 to 2010, this paper tracks how much crime rates change before and after the implementation of the policy. Then, I exploit the differential timing of the policy implementation across sanctuary cities to identify the causal effect of sanctuary

policies on crime.

This paper checks the pre-trend and the heterogeneity of the policy effect over time by the DIDM estimator invented by de Chaisemartin and D'Haultfœuille (2020). The DIDM result shows that sanctuary policies do not cause an increase in violent crimes, but they do decrease property crimes somewhat. Moreover, I confirm that (a) there is no clear pre-trend before the adoption of sanctuary policies, and (b) property crime rates show evident decreases starting from the implementation year, whereas violent crime rates decrease afterward. These results are robust to controls for the efficiency of the local government and other immigration policies, although results using a matching DID methodology do not show any effect of sanctuary policies on crime. It is worth noting that none of these results show a positive effect of sanctuary policies on local crime rates.

Then, this paper tests whether sanctuary policies affect other outcomes such as composition of the city population. Sanctuary policies do not appear to increase the fraction of foreigners, migrants (including domestic migrants), and likely undocumented Mexican immigrants but are associated with decreased police officers. Hence, the sanctuary policy does not seem to attract immigrants and the increased trust between residents and police seems a key factor of the effect.

1.1 Literature

The relationship between immigration and crime in the U.S. context has been investigated for a long time.[2][3] Ousey and Kubrin (2018) published a recent survey on the literature in sociology. They found that most of the papers find no relationship between

[2]For example, immigrants overall are less likely to be committed to prison (Moehling and Piehl, 2009), and more immigrants in Metropolitan Statistical Area (MSA) lead to a lower crime rate (Reid et al., 2005). Looking specifically at Mexican immigrants, Chalfin (2015) used a network instrumental variable and found negative impacts of Mexican immigrants on rape and larceny rates at MSA level and a positive impact on assault. Light and Miller (2018) focused on undocumented immigrants and, with a fixed-effect model, found a negative association between undocumented immigrants and violent crime at the state level.

[3]The effect of immigration on crime rates is also a common topic in European countries. Piopiunik and Ruhose (2017) used an exogenous allocation of immigrants in Germany following the collapse of the Soviet Union and found it had a positive effect on crime. Bell et al. (2013) used two immigrant shocks in the UK and found one positive and one insignificant effect on property crime.

immigration and crime, and even among papers that found a significant relationship, most of them found a negative relationship. Hence they concluded that the overall immigration-crime association is negative but weak.

Another literature focuses on the effects of immigration policies on crime (Baker, 2015; Freedman et al., 2018). Granting legal status of immigrants decreased property crime (Baker, 2015), but felony charges rose for those who had not gained legal status (Freedman et al., 2018).[4]

In terms of policy impacts on immigrants, Miles and Cox (2014) looked at whether a harsher policy against undocumented immigrants changes crime rates. In particular, they focused on the Secure Communities (SC) program, where fingerprint information collected by local police is automatically sent to the Department of Homeland Security. Miles and Cox (2014) performed a difference-in-differences regression using a county-level variation of implementation timing and concluded that the SC program does not change crime rates. Although Miles and Cox (2014) found no impact on crime rates, Alsan and Yang (2018) found the SC program induces fear of deportation among Hispanics and decreases demand for safety net programs. Churchill et al. (2019) focused on E-Verify laws, which require employers to check if new employees are legally eligible to work. They found that E-Verify laws decreased property crimes involving Hispanic arrestees, likely because of more employment among low-skilled natives of Hispanic origin and outmigration of young immigrants.

There are several papers investigating sanctuary cities and crime (Lyons et al., 2013; Wong, 2017; Martínez-Schuldt and Martínez, 2017; O'Brien et al., 2019; Hausman, 2020). Martínez et al. (2018) summarized the recent contributions of the literature. So far, the literature has found evidence of null or negative relationships. Lyons et al. (2013) used data at the census tract level and found a strong negative relationship in sanctuary cities between violent crime and the concentration of immigrants in a neighborhood. Wong (2017) compared sanctuary and non-sanctuary cities using a matching method and found that crime rates are lower in a sanctuary city. O'Brien

[4]Granting legal status of immigrants decreased reincarceration in Italy (Mastrobuoni and Pinotti, 2015).

et al. (2019) conducted two types of analysis. They statistically tested if the passage of sanctuary policies changed crime rates within a city, and also checked whether there was a difference in crime rates between a group of sanctuary cities and a group of matched non-sanctuary cities. Their paper found no discernible difference in violent crime, rape, and property crime rates after the passage of sanctuary policies. Also, the matching results suggest that the sanctuary cities have no differences in crime rates compared to the non-sanctuary cities. Martínez-Schuldt and Martínez (2017) used an unconditional negative binomial model with city fixed effects to estimate the impact of sanctuary policies. They found that policy implementation is associated with the reduction of robbery, and they found no significant relationship with homicide. Taken all together, these papers found either no effect or negative effect on rates of various crimes. None of them found a positive effect on crime rates.

These existing papers have some limitations and the aim of this paper is to fill the remaining gaps. First, existing papers do not focus on the dynamic effects of sanctuary policies on crime rates, although the effects might be strengthened or diminished over time. Most of the papers found null effects, but it might be due to the increasing or decreasing effects. Hence, it is necessary to investigate more about the dynamic effects. Second, the selection to sanctuary cities could be an issue for identification, since sanctuary policies are adopted through political decisions. As O'Brien et al. (2019) pointed out, sanctuary cities are different from non-sanctuary cities, even in the observable characteristics. Hence, the possible selection issue should be addressed. Third, each analysis focus on a few crime types and so the discussion about heterogenous impacts across types is limited. Fourth, the literature does not control for heterogenous trends in crime rates across geographic areas such as counties or cities. In particular, California has more sanctuary cities and the crime rate is declining. We need analyses to avoid simply capturing a trend specific to the states that have more sanctuary cities. Lastly, the reason of the effect is not well investigated. The existing papers mention potential reasons but these claims are not supported by data analysis.

2 Theoretical prediction

When a policy changes at a city level, two things should be considered to interpret the effect on local crime rates: sorting and incentives. Both could affect local crime rates.

Sorting means that when a policy is implemented, the composition of the population could change through migration between cities. Sanctuary cities might attract undocumented immigrants from other cities, since they feel more safe in the sanctuary city. If undocumented immigrants have a different propensity for crime, then the local crime rate may change (Ousey and Kubrin, 2009). For instance, Chalfin and Deza (2020) found that labor market immigration enforcement in Arizona reduced property crimes since young male immigrant population declined. However, crime rates could fall if immigrants are less crime-prone than natives (Butcher and Piehl, 2007; Tonry, 1997). Hence, the direction of the sorting effect is ambiguous and also depends on incentives.

Incentives arise from the cost and benefit of crime. In particular, sanctuary policies change the costs of crime. Becker (1968) explained that crime could be rational. From this perspective, an increase in the costs of crime would reduce crime.

Sanctuary policies might affect the opportunity cost of committing a crime. Under strict immigration policies, it is difficult for undocumented immigrants to find a job (East et al., 2018) and to apply for welfare programs (Alsan and Yang, 2018). Hence, mitigating the strict policies may spread job opportunities for illegal immigrants.[5] Sanctuary policies may also enable them not to worry about their immigration status to find a job or to get welfare benefits, and so the policies could increase the opportunity cost of crime.

In addition, the policies affect the expected cost of committing a crime. The expected cost of crime (c_{crime}) consists of the probability of being caught (p_{caught}) and the amount of sanctions ($\delta_{sanction}$): $c_{crime} = p_{caught} \times \delta_{sanction}$. Sanctuary policies could affect each part, so the overall crime cost could go up or down. Sanctuary policies

[5] Amuedo-Dorantes et al. (2020) found that issuing Driver's License encourages the labor supply of illegal immigrants.

lower the sanction on undocumented immigrants when they commit a crime. Specifically, the punishment decreases because the local police do not begin the removal process. Undocumented immigrants no longer need to be concerned about deportation even when they are caught. For other people, the sanction of crimes does not seem to change because of a sanctuary city policy.

However, the expected cost of crime also depends on the probability of getting caught. The literature claims the probability could increase because of two reasons: a spiral of trust and informal social control. Lyons et al. (2013) referred to "a 'spiral of trust' that improves communication between officials and immigrants, promotes legislation protecting immigrant interests, and generates greater system-level trust in government." Without a sanctuary policy, undocumented immigrants may not report a crime when they become victims or witnesses, because local police might discover their legal status, and deportation could happen. However, with such a policy, undocumented immigrants report more crime and trust the police. If sanctuary policies induce residents to trust police, then the police can get cooperation from residents and work more efficiently and effectively, and eventually, apprehension probability would increase. Also, the policy strengthens informal social control, which arises from ties among residents. Buonanno et al. (2012) showed that dense social interaction is associated with a lower rate of property crime. Moreover, informal social controls are closely related to the relationship between residents and police. Using survey data in Chicago, Silver and Miller (2004) found that neighborhoods where people have more satisfaction with local police have a higher level of informal social control over delinquent behavior of youth. The probability of being caught will increase if residents strengthen informal social controls over crime. In summary, the change in the costs of crime is ambiguous, and it is hard to predict the sign of the policy effect from theory alone.

All the mechanisms so far are about committed crimes, but, in practice, we can only observe the reported crimes. Underreporting is common in crime issues. Sanctuary policies may induce residents to report more crimes, because the policies increase the

trust in police. Even if the number of committed crimes remains the same or decreases because of the sanctuary policy, the number of reported crimes could increase. Hence, we need to be cautious about the interpretation of the results. If the reported crime rate increases after the enactment of sanctuary policies, it could be either because of more committed crimes, less underreporting, or both. However, if we observe a decrease in the reported crime rates after the policies, it must be because the change in crimes committed dominates the effect of reduced underreporting.[6]

3 Data & Approach

3.1 Definition of sanctuary city

There is no universally accepted definition of a sanctuary city. For example, Executive Order 13768, from 2017, defines sanctuary cities as "locales that refuse to comply with federal statute 8 U.S.C. 1373 enhancing information related to individuals' immigration statuses with ICE or CBP." Alternatively, the Department of Justice defines a sanctuary city as a "jurisdiction that may have state laws, local ordinances, or departmental policies limiting the role of local law enforcement agencies and officers in the enforcement of immigration laws." There are also many lists of sanctuary cities, including the ones made by the Center for Immigration Studies (CIS),[7] and Ohio Jobs & Justice PACS (OJJPACS).[8] Throughout this paper, the definition of a sanctuary policy is as follows: a sanctuary policy is a policy legislated by a local government that (1) inhibits the local enforcement agencies from cooperating with federal immigration authorities and (2) is stated explicitly in administrative documents such as resolutions, ordinances, executive orders, or police orders. A sanctuary city is a city that has any sanctuary policy. Note that this paper focuses only on the formal sanctuary cities. There are also informal sanctuary cities, which do not cooperate with federal author-

[6]This argument relies on two assumptions. First, I assume sanctuary policies do not exacerbate underreporting. The second assumption is that sorting is weak, which is confirmed in Section 4.3.

[7]https://cis.org/Map-Sanctuary-Cities-Counties-and-States

[8]http://www.ojjpac.org/sanctuary.asp

ities without having an explicit statement or policy. These cities are not counted as sanctuary cities in this paper since the definition of an informal sanctuary city is arbitrary to some extent.[9]

Following the literature (O'Brien et al., 2019; Martínez-Schuldt and Martínez, 2017), this paper uses a list of sanctuary cities offered by the National Immigration Law Center (NILC). The list by the NILC has some advantages over other lists. One is that there is a brief description of the policy for each city, which helps to identify the date of implementation and categorize its type of policy. However, one disadvantage is that the list was last updated in 2008 and updated until 2010 by Martínez-Schuldt and Martínez (2017). Following O'Brien et al. (2019) and Martínez-Schuldt and Martínez (2017), I checked each document and made a list of sanctuary cities (see Table A.1).[10] There were 42 sanctuary cities in 2010.

3.2 Data source

The data ranges from 1999 to 2010. The beginning year is set to 1999, because most of the sanctuary cities in the NILC list adopted their policies after 2000. I chose 2010 as the end period because Martínez-Schuldt and Martínez (2017) updated the list up to 2010 (the original list in the NILC was last updated in 2008). This extension gives a larger sample size for analysis. The list of sanctuary cities and the type of policies are summarized in Table A.1 and Appendix A. There are several types of sanctuary policies, and the most popular type is the policy to limit the enforcement of the immigration laws.[11]

Crime data is collected from the FBI Uniform Crime Reports (UCR). The data used for the analysis is annual, city-level, and reported. Categories of crime are violent

[9] A list made by the CIS contains cities that have policies regarding detainers. OJJPACS includes informal sanctuary cities in its list.

[10] Sanctuary policies could be determined by state and county levels as well. In the list from the NILC, 4 states (Alaska, Montana, New Mexico, Oregon) and 7 counties (Sonoma County, CA; Cook County, IL; Prince George's County, MD; Butte-Silver Bow County, MT; Rio Arriba County, NM; Marion County, OR; Dane County, WI) are considered to be sanctuary jurisdictions. However, local police enforcement is operated under city governments. Appendix B.5.1 investigates how the results change when including state and county level policies.

[11] See Appendix A for details.

crime (homicide, rape, robbery, aggravated assault) and property crime (burglary, larceny, auto theft).[12] The FBI also provides the number of police officers of each agency in the UCR,[13] and I use the number of sworn officers per capita to investigate the channels of the effect.

For robustness checks and the channels analysis, I use demographic information for each city. To get the demographic information, this paper also uses data from the Census and the American Community Survey (ACS). The data is obtained from IPUMS for public use (Ruggles et al., 2019). Annual data is not available for all years, and so I use the 5% sample of the Census in 2000 and the annual sample of the ACS from 2005 to 2010. The data between 2000 and 2005 is filled by linear interpolation. The demographic analysis in Section 4.3 begins in the year 2000.

Sample cities satisfy the following two criteria: the population is larger than 100,000, and there are at least two observations during the sample period. To have as large a sample size as possible for the analysis and to do a more in-depth investigation, the paper uses two sets of samples: (1) the baseline sample, used for the main analysis, and (2) the sample with time-variant characteristics, used for robustness checks and detailed analysis. The baseline sample is made only from the UCR. Among all cities in the UCR data, 288 satisfy the two criteria, and there are 2981 observations in total.[14] Although the main analysis is based on the baseline sample, for some analysis, this paper also uses the sample with time-variant characteristics, which is constructed from the UCR and the ACS. Not all cities in the UCR are identified in the ACS, be-

[12] In the UCR, some city-year pairs have a missing value for some types of crimes. The reasons are incomplete data, not following the UCR guidelines, and overreporting. These city-year pairs are included for the analysis because most of the cities do not have any missing value (268 out of 288 cities have no missing value), and usually the problem is specific to one type of crime. For example, cities in Illinois use a different definition of rape, so they are not reported in the UCR.

[13] Not all cities have their own police department. For example, the city of Charlotte in North Carolina does not have its own police department. Instead, the Charlotte-Mecklenburg Police Department covers Mecklenburg county, including Charlotte.

[14] Although the sample size is almost the same, there is a slight difference by crime categories. The type of crime and the number of missing observations are Rape (78), Robbery (0), Assault (1), Burglary (2), Larceny (10), Auto theft (6), Violent (79), and Property (17). 78 observations of Rape are missing since Illinois and Minnesota did not follow the definition of rape. Homicide and Robbery do not have missing values. However, 133 observations of zero homicide count are treated as missing when log of the counts is used.

cause the ACS Public Use data does not define all cities.[15] The difference is mainly from dropping relatively small cities in terms of population and geographic size. As a result of merging the UCR and the ACS, the total sample size is 170 cities (including 35 sanctuary cities) and 1749 city-year observations.[16]

3.3 Regression model

The regression is based on an event study approach. Specifically, the analysis is based on the following specification:

$$\log y_{it} = \sum_{\tau} \alpha_{\tau}^{sanc} \mathbb{1}(\tau = t - SanctuaryYear_i) + \eta_i + \eta_t + \gamma_{st} + \epsilon_{it}. \quad (1)$$

The outcome variable y_{it} is the crime rate of city i at year t, which is the number of reported crimes per 100,000 people in the city. I use a natural logarithm of the crime rate as a dependent variable. Both violent and property crime rates and each subcategory are analyzed. $SanctuaryYear_i$ is the year that the city i adopts sanctuary policies. η_i and η_t are city and time fixed effects, respectively. γ_{st} captures state-specific time trends.[17] The regression coefficient of interest is α_t^{sanc} and the coefficient is the event study estimates of the effect of sanctuary policies. Since the dependent variable is log of crime rates, α_t^{sanc} is the percentage change in crime rates due to the sanctuary policy. The regressions are weighted by city population, and standard errors are clustered at the city level.

An event study design is a standard tool to identify the policy effect under the staggered adoption. The estimation require the parallel trend assumption, and so this paper assumes that the time trends between treatment and control groups are parallel. If the parallel trend assumption fails, then the event study estimators are likely to con-

[15] A city code in IPUMS is based on a Public Use Micro Data Area (PUMA), which contains at least 100,000 residents but does not necessarily correspond to city boundaries. The city code for public use is given only if (1) for an individual, the majority of the PUMA population lives in the city, and (2) based on the classification of individuals, the sum of two errors (inclusion of non-residents and exclusion of residents in the city) is less than 10% of the city population.

[16] The sample period is from 2000 to 2010, and 1999 is not included for the sample with the time-variant characteristics.

[17] Alternatively, I use city-specific linear trend terms ($\gamma_i t$) in some specifications.

tain the effects of other factors. For example, the anticipation of policy implementation could change the behavior of people even before the date of the policy implementation. Cities may start an informal sanctuary policy gradually and then make it as a formal policy at a later point. Hence, the existence of the pre-trend should be checked.

A problem of the event study design with staggered adoption is that the estimators are not robust to heterogeneous treatment effects over time or group (de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2020). The heterogeneity arises, for example, when residents in a city need time to learn about sanctuary policies and forget the existence of the policies if the city became sanctuary a long time ago. However, the standard event study approach cannot properly capture the heterogeneous effects over time, due to cohort effects. Moreover, the heterogeneity of the policy effect may bias the event study estimators (de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2020). Since the heterogeneous effects contaminate the estimated coefficients before treatment, the standard event study design is insufficient to confirm the pre-trend. Alternatively, to confirm the pre-trend and heterogenous effects of sanctuary policies, this paper uses the DIDM estimator that is introduced by de Chaisemartin and D'Haultfœuille (2020) and is robust to the heterogeneous effects.

3.4 Summary statistics

In the data, violent crime consists of homicide, rape, robbery, and assault, and more than half of the violent crime rate is from assault. As for property crime, among three subcategories (burglary, larceny, and auto theft), larceny occupies more than half of the total.[18] In the 170 sample cities, there are 35 sanctuary cities. Among the three types of sanctuary policies defined by Kittrie (2006), the most popular type is the *don't enforce* policy, and its share is about half of the total observations. The *don't ask* policy follows and the *don't tell* policy is the least popular. Three types of policies do not sum up to one since some policies fall into multiple categories.

The violent crime rate is higher in sanctuary cities, and that is true for each subcate-

[18]The summary tables are available in Appendix A.

gory except forcible rape. However, sanctuary cities have a lower property crime rate except for the auto theft rate, which does not show a significant difference between sanctuary and non-sanctuary cities. Overall, both sanctuary and non-sanctuary cities show a similar declining trend of crime rates, but not all the trends for each subcategory of crime are monotone.

The sanctuary and non-sanctuary cities are quite different in terms of the observable demographic characteristics. Sanctuary cities have a larger population, are racially more diverse, have more educated people, and have more foreigners.[19]

3.5 Identification

The DIDM estimator gives the average treatment effect on the treated (ATT). Identification relies on a change of sanctuary status and the different timing of the policy implementation. The number of sanctuary cities has increased over time. At the beginning of 1999, 9 cities (Los Angeles, San Francisco, Washington D.C., Chicago, Jersey City, Cleveland, Salem, Austin, and Houston) were formal sanctuary cities. The number increased to 29 by 2005, and all 42 cities in the list employed at least one type of sanctuary policy by the end of 2008. Note that no city abolished its sanctuary policy in the sample period.

The DIDM approach also requires a parallel trend assumption for identification of ATT: both treatment and control groups have the same time trend before and after the treatment. This paper assumes the parallel trend and argues the validity of the assumption is discussed in the result section.

[19]In addition, most of the sanctuary cities are lead by democratic leaders. Democratic mayors governed 33 out of 42 sanctuary cities when the sanctuary policies were adopted. The rest is 6 Republicans (Chandler, Garden Grove, Sam Diego, Jersey City, New York, Cleveland) and 3 Independents (Fresno, Richmond, St. Paul).

4 Results

4.1 Main results

Figure 1 graphically shows the DIDM estimators of treatment effects until five years after and the placebo estimators up to five years before the policy implementation. Table 1 and 2 summarize the estimators and the standard errors, as well as the average effects after the policy adoption.

[Insert Figure 1, Table 1, and Table 2 here]

The first column in Table 1 is the effects on violent crime as a broad category. Although the point estimates are negative, most of the coefficients are insignificant. As the average effect, after adopting the policy, sanctuary cities have a 15% lower violent crime rate than non-sanctuary cities. However, the estimator is statistically insignificant at the 5% level. The second to fifth columns are the results for subcategories of violent crime. All subcategories have negative coefficients on average, but none of them except robbery show a statistically significant decrease at the 5% level. For robbery, the coefficient becomes significant at 4 and 5 years after the adoption, and the average effect is about 14%. Overall, sanctuary policies do not have a significant impact on violent crime rates. As its subcategory, the negative effect on robbery may exist, but it takes at least a few years for the effect to become evident.

Table 2 shows clear pattern for property crimes. The property crime rate falls from the implemented year and the magnitude is increasing over time. On average, sanctuary policies lead to a decrease in the property crime rate by 11.2%. For subcategories, the coefficients show negative signs and the negative impacts on burglary and auto theft are strengthened. The average effects on burglary, larceny, and auto theft are 18%, 9%, and 16%, respectively.

To check the validity of the parallel trend assumption, Table 1 and 2 also have placebo coefficients, which capture changes of outcome variables before the policy implementation. If the coefficients are different from zero, the parallel trend assumption

is doubtful: the DIDM estimators may have a bias due to, for example, Ashenfelter dip (Ashenfelter and Card, 1985). However, most of the coefficients for violent crimes and none for property crimes are not statistically different from zero. Hence, the DIDM placebo estimators shows no evidence of violation of the parallel trend assumption, at least before the adoption.[20]

In summary, none of crime types increases due to sanctuary policies. Instead, sanctuary policies seem to have a negative effect on the general category of property crime and its subcategories as well as robbery. Moreover, the negative effects are strengthened over time.

4.2 Matching DID

To evaluate the effect of sanctuary policies, the treatment and control groups should be similar. However, sanctuary cities and non-sanctuary cities have different demographic characteristics as O'Brien et al. (2019) pointed out.[21] Since sanctuary status is an outcome of choice by a local government, the local government might self-select the status; the self-selection causes a bias for the estimates. To check the bias, I perform DID regression with matched samples.[22] The method in this section is based on Heckman et al. (1997). I match each sanctuary city based on its propensity score to be a sanctuary city by 2010.[23] The city characteristics in 2000 are used for the propensity-score matching (PSM) and so I drop nine cities that started a sanctuary policy before 2000. To compute the propensity scores, the following variables are used: (1) log of population, (2) % female, (3) % age groups (15–29, 30–44, 45–59, 60 or above), (4) % racial groups (Black, Asian, Pacific Islander, Native American, Other race), (5) % Hispanic, (6) % less than high school degree, (7) % college or above degree, (8) poverty

[20] The placebo estimator for violent crime is lower at 3 years before the implementation. To check the selection issue, I also perform the matching DID analysis in Section 4.2.

[21] O'Brien et al. (2019) noted "... sanctuary cities–compared with non-sanctuary cities–are larger, less White, more racially and ethnically diverse, have lower median incomes, have higher levels of poverty, have larger foreign-born populations, and more Democratic".

[22] See Appendix B for the methodology in details (B.1), the base line results (B.2), and other robustness checks (B.3-B.5).

[23] Although the PSM method mitigates observable differences between treatment and control groups, a bias due to latent characteristics may still exist.

rate, (9) % unemployed, and (10) the sum of the violent and property crime rates. The matching method is the Epanechnikov kernel matching on the common support.[24] After the propensity-score matching, the total sample used for the analysis is 136 including 14 sanctuary cities. Figure 2 confirms that the matching method reduces the difference between the treated and control groups. Table 3 indicates the treated and control groups have no statistical difference after matching in terms of observables.

[Insert Figure 2 and Table 3 here]

The regression results in Table 4 show an insignificant effect on property crime and violent crime. As a subcategory, larceny shows a significant negative effect at the 10% level. However, none of the coefficients shows a discernible effect of sanctuary policies once the city-specific time trend is controlled for. The coefficient for larceny thefts has a positive coefficient but it is statistically insignificant. Although the point estimates are insignificant, all the estimates show a relatively small effect of the sanctuary policy. The point estimate for robbery indicates an increase of 3.6% but it is only slightly above one standard deviation. The estimator for auto theft indicates a 3.3% decrease with high standard errors.

[Insert Table 4 here]

I also try the same matching estimation with fewer variables used for matching. Even when only three variables (population, % high school dropouts, % unemployed) are used to calculate the propensity score, the pseudo R^2 is still 0.57 (0.70 for the original specification). After the same matching procedure, 142 cities (including 17 sanctuary cities) remain for the estimation. The estimation results are similar to Table B.3 and B.4. For example, the effects on the property crime rate are -0.085 (without linear trends) and -0.0431 (with linear trends), and only the former estimator is statistically significant at 1 percent level.

[24] To check the robustness, I also use the Nearest Neighbor (NN) matching with replacement. For each treated observation on the common support, the NN matching identifies the closest observation in the control group. However, the estimated coefficients are similar to the main results.

Lastly, I also run DIDM after the matching procedure adopted in Table 4. The results are summarized in Figure 3. In Figure 3, the negative effect on property crime is still confirmed at one year after the adoption. Although the results show stable point estimates after two years, they are statistically insignificant due to higher standard errors.

[Insert Figure 3 here]

In summary, sanctuary policies may have negative effects on some categories of crime, but the effects are not clear under the matched sample; the level of significance changes, in particular, for property crimes. The insignificance may be because of too many variables used for matching. However, both the baseline and matching results provide no evidence that sanctuary policies increase crimes.

4.3 Why do sanctuary policies lead to lower crime rates?

The main results show that sanctuary policies lead to a decrease in crime rates for some categories. Section 2 explained the potential reasons why sanctuary policies affect crime rates: sorting and incentives. To explain the lower crime rates associated with sanctuary policies, I now investigate two possible channels: composition change of population and change in size of police force. If sorting is the reason for lower crime rates, that would mean sanctuary policies change the composition of city populations. As for incentives, Section 2 explains that sanctuary policies are expected to improve police operations through a spiral of trust. However, the sanctuary policy could reduce crimes without the spiral of trust if it also increases the size of the police force. In this section, I check how much the population composition and the police force size change depending on the policy. To investigate the effect on population composition, the UCR data is merged with the ACS data. Hence, this section uses the subsample of the main

analysis.[25] The regression is based on the DID specification.[26]

[Insert Table 5 here]

First, to check demographic changes caused by the sanctuary policy, I run a regression of different demographic groups' population percentages in Table 5. The dependent variable is the percent of the demographic group in the city and so ranges from 0 to 100. The interpretation of the coefficient is the percentage point change for that demographic group. The results in Table 5 show a modest decrease in the fraction of foreigners, but the coefficients are not significant at the 5% level. The second row in Table 5 shows the result for those who migrate from other states to the city in the past year. Although the estimated coefficients show a negative impact, the fraction does not change significantly before and after the implementation of sanctuary policies. The third row uses likely undocumented Mexican immigrants (LUMEX) as a dependent variable. The definition of LUMEX is based on Hall and Stringfield (2014). They define LUMEX as a group of immigrants from Mexico who (1) are non-citizens, (2) are not current students, (3) do not have some college or a higher degree, (4) do not work in the government sector, and (5) arrived to the U.S. after 1990. The results show no evidence that LUMEX responds to the sanctuary policy. A sanctuary policy may also change the share of young men, which are the main crime-prone group. However, the estimated result in the fourth row shows no discernible effect on the share of young men. So, overall, demographic changes are unlikely to be the reason for the crime rate changes.

Next, the last rows in Table 5 check if sanctuary policies cause any change in police

[25]Since the sample is different from the main analysis, I check the sensitivity using the DID results in Appendix B.2. Because of this sample selection, the significance of coefficients changes for property crime. This difference is probably from dropping San Diego. Since San Diego shows a sharp decrease in the property crime rate after the policy adoption, its impact on the coefficient may be large. Property crime still shows a negatively significant effect in the model without linear trends but becomes insignificant with linear trends. The coefficients for auto theft become insignificant. The estimated coefficient is smaller than that of the baseline result. The coefficient on the rape rate is 2%, which is larger (in the absolute value) than the baseline result (1%). The point estimate for the robbery rate is less than 1%. The effect on property crime is smaller than the result in the previous section. Overall, although some coefficients change the statistical significance, the subsample analysis gives similar results.

[26]Figure 4 shows the DIDM results. Although point estimates are negative, the impacts on the number of police officers are statistically insignificant.

decline in police force. Sanctuary policies are intended to induce resident cooperation with law enforcement agencies, for example, in case of police investigation. The cooperation enables to achieve the same crime level with less police officers. To check this channel, I regress the log of the number of sworn officers per capita as a dependent variable.

The results show a significant negative effect of the policy. The sanctuary policy reduces per-capita sworn officers by 3%, although the effect becomes less clear with linear trends. Since the size of the police force is not increased by sanctuary policies, the negative effect of sanctuary policies is not caused by more police. Instead, this result supports that sanctuary policies help efficient operation of police force.[27]

Lastly, I also check the effect on the labor market performance of foreigners. The labor market performance is measured by the percentage change of the immigrants who are unemployed. However, the results show no significant change in the share of unemployed foreigners. Although it may have some impact after a few years (Figure 4), the labor market channel does not seem to be as important as the police trust channel.[28]

[Insert Figure 4 here]

In summary, at least, there is no evident change in population composition. Hence, a decrease in crime rates is not due to sorting. Instead, the analysis supports the police force's more efficient operation since sanctuary policies decrease the size of the police force, which is compatible with the spiral of trust or informal social control.

[27] Sanctuary policies may affect potential police applicants. However, police applicants pool seems stable, even when the perceived risk of the job increases. For example, Rhodes and Tyler (2019) found that a decrease in trust or higher risk followed by the Ferguson protests do not change the number of applications, regardless of gender, race, or ethnicity. Hence, it is unlikely to change the application strategies of individuals.

[28] See Appendix B.6 for the effect on the labor market performance of LUMEX.

5 Discussion

The results of this paper indicate that a sanctuary policy has a negative effect on property crime, and some subcategories also show a negative effect. The results for homicide and robbery imply no effect of the policy, while Martínez-Schuldt and Martínez (2017) found no effect on homicide but a negative effect on robbery. The negative binomial regression in Appendix B.4 confirms the negative effect of the policy. In addition, the DIDM results suggest there may be a long-run effect on the robbery rate. Hence, the effect on the robbery rate is sensitive to the specification and time length. Although the effect on robberies is clearer in violent crimes, the effect is not as apparent as property crimes.

Although most of the results support the crime-reducing effect of sanctuary policies on property crime. The effect is not confirmed under the matching DID estimation. The reason might be the sample difference. However, the impact on property crime may not be strong enough.

This paper considers two reasons why a sanctuary policy could change crime rates: sorting and incentives. The results do not show that sanctuary policies cause any change in the proportions of foreigners or migrants, so this paper found no evidence of sorting. Hence, the reduction of crime is likely from the lower propensity to commit crimes rather than compositional change. This paper does not directly test the reason why the propensity changes, but the results are compatible with the spiral of trust and informal social control stories. Increased trust from residents makes law enforcement operation efficient and so sanctuary policies achieve the lower crime rates with less police force. This paper does not directly test the change in confidence in police. However, given that recent papers (Nguyen and Gill (2016); Amuedo-Dorantes et al. (2020); Amuedo-Dorantes and Arenas-Arroyo (2021)) also support the increased trust in police, the findings in this paper would support the police trust story. For future direction, it would be helpful to understand which groups (natives, legal immigrants, and undocumented immigrants) are affected by the policy. The policy discussion on this topic has mainly been concerned with undocumented immigrants. However, the

sanctuary policy could change the costs of crime for natives and legal immigrants as well, because police operations seem to become more efficient because of the spiral of trust and informal social control.

A sanctuary policy may affect the reporting of crimes, so decomposing the effect into criminal behavior and reporting behavior would be helpful. So far, most of the results show no discernible effect. However, it is not clear if the sanctuary policy has no impact on any behavior, or if the sanctuary policy affects both criminal and reporting behaviors but they cancel each other out.

Lastly, although sanctuary policies vary by cities, this paper does not consider the variation of the sanctuary policies. Kittrie (2006) offered a way of classification of sanctuary policies, but variation exists even within the same type. The variation of the policies may cause different effects across cities. However, it is hard to measure heterogeneity using city-level data. Further investigation is necessary for better policy design, and it may be useful to focus on particular cities that the same policy is adopted.

6 Conclusion

This paper investigates whether sanctuary policies increase crime, using a city-level variation of implementation timing from 1999 to 2010. The results show that sanctuary policies do not cause an increase in crime. Instead, sanctuary policies lead to a decrease in the property crime rate. The main regression results do not confirm the pre-trend. The results show no positive effect of the policy and find the effect for the property crime strengthened over time. Moreover, the policy does not lead to an increase in the proportion of foreigners or migrants, so sorting is unlikely to be the source of a decrease in crime rates. Instead, the negative effects stem from the improved relationship between police and residents.

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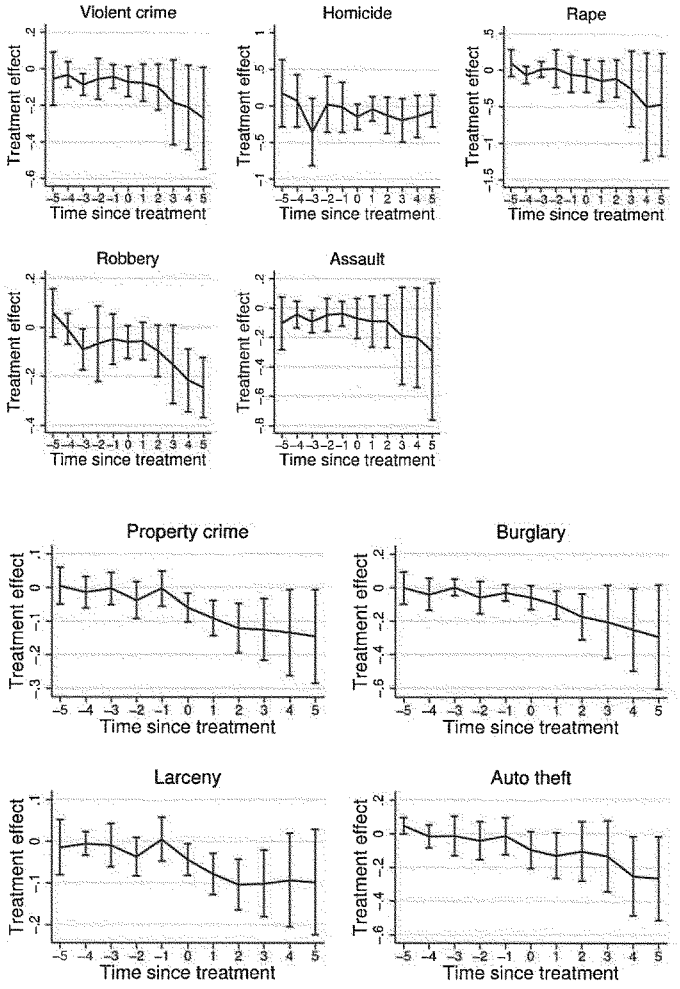


Note: The graphs are based on the DIDM estimator introduced by de Chaisemartin and D'Haultfœuille (2020) with 95% confidence interval. The standard errors are based on bootstrapping (100 iterations). The estimations consider state-specific time trends nonparametrically.

Figure 1: The DIDM estimators over time

Table 1: DIDM coefficients (baseline sample: violent crime)

VARIABLES	(1) Violent	(2) Homicide	(3) Rape	(4) Robbery	(5) Assault
Event date (-5)	-0.055	0.172	0.097	0.059	-0.102
	(0.074)	(0.235)	(0.094)	(0.051)	(0.092)
Event date (-4)	-0.031	0.070	-0.068	-0.006	-0.044
	(0.036)	(0.183)	(0.060)	(0.032)	(0.046)
Event date (-3)	-0.087***	-0.358	0.011	-0.090**	-0.091**
	(0.030)	(0.235)	(0.053)	(0.043)	(0.038)
Event date (-2)	-0.054	0.024	0.023	-0.067	-0.046
	(0.057)	(0.196)	(0.132)	(0.079)	(0.057)
Event date (-1)	-0.042	-0.016	-0.056	-0.048	-0.037
	(0.033)	(0.174)	(0.124)	(0.053)	(0.044)
Event date (0)	-0.070*	-0.147*	-0.077	-0.060*	-0.070
	(0.042)	(0.088)	(0.113)	(0.034)	(0.070)
Event date (1)	-0.076	-0.040	-0.145	-0.056	-0.091
	(0.052)	(0.084)	(0.141)	(0.040)	(0.088)
Event date (2)	-0.100	-0.123	-0.113	-0.097*	-0.089
	(0.064)	(0.126)	(0.129)	(0.054)	(0.091)
Event date (3)	-0.184	-0.193	-0.252	-0.151*	-0.186
	(0.118)	(0.152)	(0.265)	(0.082)	(0.169)
Event date (4)	-0.209*	-0.141	-0.492	-0.216***	-0.200
	(0.117)	(0.146)	(0.374)	(0.065)	(0.173)
Event date (5)	-0.268*	-0.064	-0.463	-0.246***	-0.292
	(0.142)	(0.113)	(0.357)	(0.062)	(0.239)
Average effect	-0.151*	-0.118	-0.257	-0.138***	-0.155
	(0.085)	(0.087)	(0.214)	(0.046)	(0.132)
City FE	Y	Y	Y	Y	Y
State-Year FE	Y	Y	Y	Y	Y
Number of Cities	285	288	285	288	288
Observations	2902	2848	2903	2981	2980

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

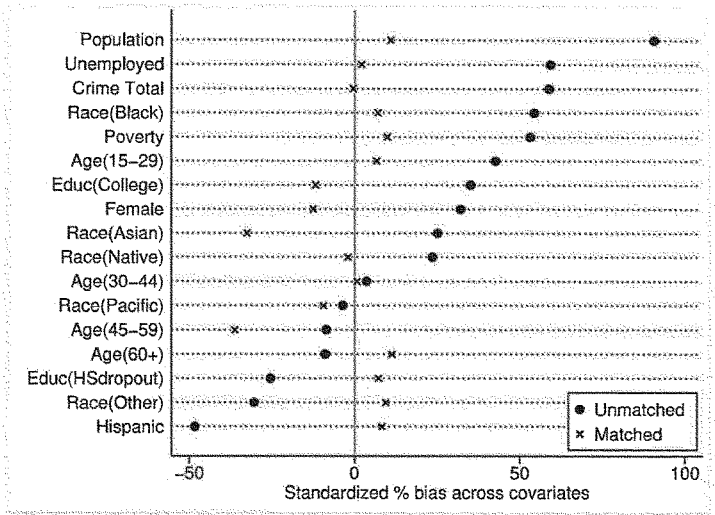
Notes: The average effect is the average of the effects from 0 to 5 years. The standard errors are based on bootstrapping (100 iterations).

Table 2: DIDM coefficients (baseline sample: property crime)

VARIABLES	(1) Property	(2) Burglary	(3) Larceny	(4) Auto theft
Event date (-5)	0.006	-0.001	-0.015	0.047*
	(0.028)	(0.049)	(0.034)	(0.025)
Event date (-4)	-0.013	-0.039	-0.005	-0.015
	(0.024)	(0.049)	(0.015)	(0.035)
Event date (-3)	-0.003	0.004	-0.009	-0.013
	(0.024)	(0.025)	(0.026)	(0.059)
Event date (-2)	-0.037	-0.056	-0.037	-0.040
	(0.028)	(0.050)	(0.024)	(0.057)
Event date (-1)	-0.003	-0.029	0.005	-0.014
	(0.027)	(0.025)	(0.027)	(0.055)
Event date (0)	-0.059***	-0.056	-0.043**	-0.096*
	(0.022)	(0.036)	(0.019)	(0.056)
Event date (1)	-0.091***	-0.101**	-0.078***	-0.128*
	(0.027)	(0.043)	(0.025)	(0.069)
Event date (2)	-0.120***	-0.172**	-0.104***	-0.102
	(0.038)	(0.071)	(0.031)	(0.090)
Event date (3)	-0.124***	-0.203	-0.100**	-0.131
	(0.047)	(0.112)*	(0.041)	(0.107)
Event date (4)	-0.133**	-0.249**	-0.092	-0.250**
	(0.065)	(0.125)	(0.057)	(0.119)
Event date (5)	-0.144**	-0.293*	-0.097	-0.264**
	(0.071)	(0.159)	(0.064)	(0.126)
Average effect	-0.112***	-0.179**	-0.086***	-0.162**
	(0.040)	(0.086)	(0.032)	(0.081)
City FE	Y	Y	Y	Y
State-Year FE	Y	Y	Y	Y
Number of Cities	288	288	288	288
Observations	2964	2979	2971	2975

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Notes: The average effect is the average of the effects from 0 to 5 years. The standard errors are based on bootstrapping (100 iterations).

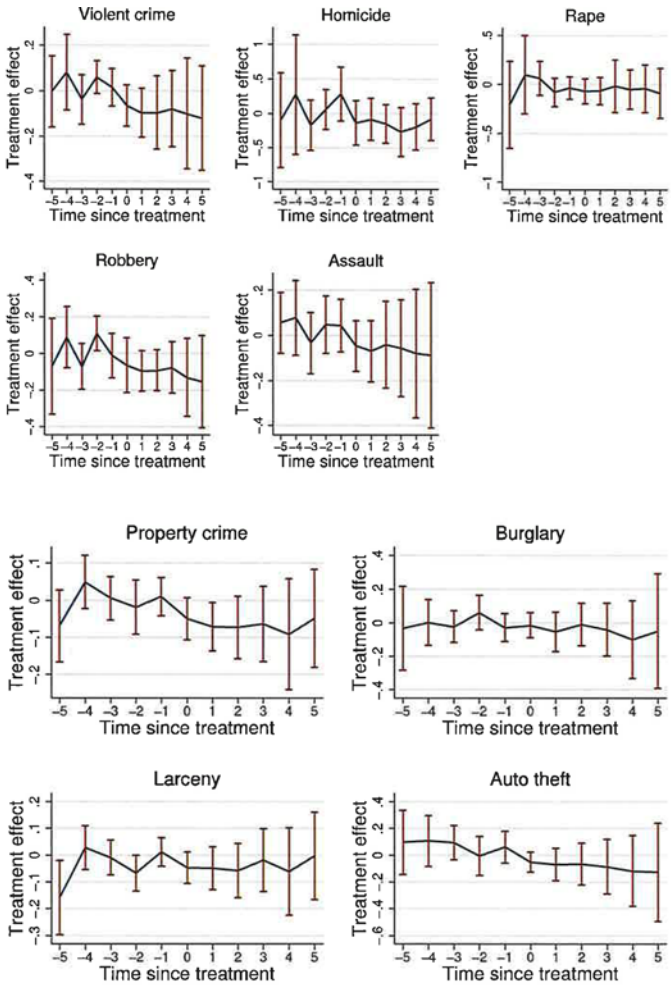


Note: Matching is based on the following characteristics: the sum of violent and property crime, log of population, female, age groups (15–29, 30–44, 45–59, 60 or above), racial groups (Black, Asian, Pacific Islander, Native American, Other race), Hispanic, less than high school degree, college or above degree, poverty rate, and unemployed.

Figure 2: Pre- and post-matching

Table 3: Post-matching comparison

	Mean			t-test		Variance ratio
Variable	Treated	Control	%bias	t-value	p>t	V(T)/V(C)
Crime Total	16442	16575	-0.3	-0.02	0.98	0.49
Log of Population	12.379	12.295	11.0	0.40	0.689	0.83
Female	0.51557	0.51720	-12.4	-0.36	0.723	0.72
Age(15-29)	0.25827	0.25532	6.8	0.15	0.882	0.92
Age(30-44)	0.23365	0.23344	0.9	0.02	0.981	0.86
Age(45-59)	0.15555	0.16207	-36.1	-0.9	0.379	0.19
Age(60+)	0.13316	0.12972	11.4	0.36	0.725	0.73
Race(Native)	0.01614	0.01643	-1.9	-0.08	0.936	0.37
Race(Asian)	0.08665	0.10829	-32.4	-0.55	0.589	0.46
Race(Black)	0.22360	0.20980	7.2	0.21	0.838	0.70
Race(Pacific)	0.00286	0.00317	-9.3	-0.30	0.765	0.41
Race(Other)	0.10721	0.09787	9.5	0.26	0.795	1.22
Hispanic	0.16639	0.15177	8.3	0.27	0.786	1.01
Educ(HSdropout)	0.38090	0.37465	7.3	0.18	0.858	1.33
Educ(College)	0.18311	0.19285	-11.6	-0.25	0.805	1.05
Poverty	0.18729	0.18080	10.0	0.24	0.814	1.03
Unemployed	0.04993	0.04959	2.2	0.05	0.959	1.61



Note: The graphs are based on the DIDM estimator introduced by de Chaisemartin and D'Haultfœuille (2020) with 95% confidence interval. The standard errors are based on bootstrapping (100 iterations). The estimations consider state-specific time trends nonparametrically.

Figure 3: The DIDM estimators after matching

Table 4: Matching DID (kernel)

	(1)	(2)
Violent crime		
b	0.0320	0.0166
S.E.	(0.0636)	(0.0457)
# Cities	136	136
Obs.	1463	1463
Homicide		
b	0.0423	0.0093
S.E.	(0.0603)	(0.0755)
# Cities	136	136
Obs.	1403	1403
Rape		
b	-0.0215	0.0195
S.E.	(0.0757)	(0.1017)
# Cities	136	136
Obs.	1463	1463
Robbery		
b	0.0273	0.0356
S.E.	(0.0563)	(0.0339)
# Cities	136	136
Obs.	1464	1464
Assault		
b	0.0568	0.0135
S.E.	(0.0971)	(0.0744)
# Cities	136	136
Obs.	1464	1464
Year FE	Y	Y
City FE	Y	Y
Linear Trend	N	Y

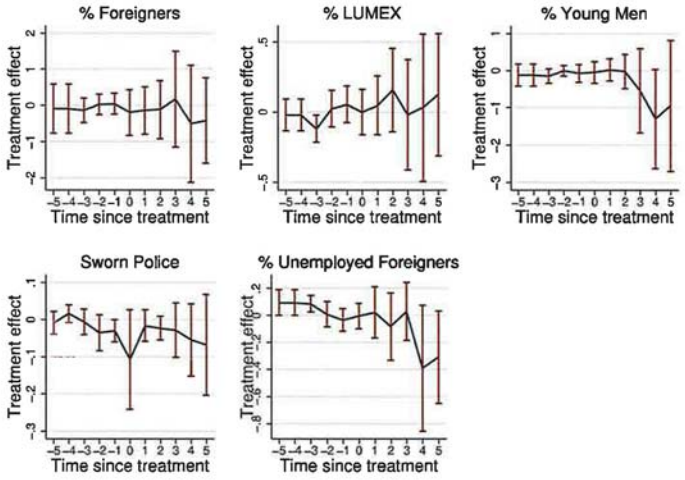
	(1)	(2)
Property crime		
b	-0.0375	-0.0074
S.E.	(0.0396)	(0.0324)
# Cities	136	136
Obs.	1460	1460
Burglary		
b	0.0308	0.0140
S.E.	(0.0970)	(0.0328)
# Cities	136	136
Obs.	1463	1463
Larceny		
b	-0.0576*	0.0100
S.E.	(0.0337)	(0.0356)
# Cities	136	136
Obs.	1461	1461
Auto theft		
b	0.0245	-0.0334
S.E.	(0.0747)	(0.0794)
# Cities	136	136
Obs.	1464	1464
Year FE	Y	Y
City FE	Y	Y
Linear Trend	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Linear trend is the city-specific trend.

Table 5: Effects on other outcomes

	(1)	(2)	(3)
% Foreigners (Mean: 22.5%)			
b	-0.0873	-0.103	-0.348
S.E.	(0.263)	(0.227)	(0.301)
# Cities	170	170	170
Obs.	1749	1749	1749
% Migrants (Mean: 3.3%)			
b	-0.0988	0.580	-0.358
S.E.	(0.455)	(0.485)	(0.256)
# Cities	167	167	167
Obs.	979	979	979
% LUMEX (Mean: 4.0%)			
b	0.0253	-0.213	0.175
S.E.	(0.104)	(0.138)	(0.122)
# Cities	170	170	170
Obs.	1749	1749	1749
% Young Men (Mean: 15.6%)			
b	0.183	-0.209	-0.0243
S.E.	(0.139)	(0.203)	(0.235)
# Cities	170	170	170
Obs.	1749	1749	1749
Sworn Police (Mean: 200.0)			
b	-0.0296***	0.0407	-0.0325*
S.E.	(0.0103)	(0.0276)	(0.0195)
# Cities	277	277	277
Obs.	2646	2646	2646
% Unemployed foreigners (Mean: 1.1%)			
b	-0.0934	0.0748	-0.127
S.E.	(0.0673)	(0.0943)	(0.125)
# Cities	170	170	170
Obs.	1749	1749	1749
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	N	Y	N
State-Year FE	N	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Except "sworn police," the dependent variables are the percentage of each group and so they range from 0 to 100. The coefficients indicate the percentage point change. % Migrants is the fraction of the population who people moved to the city within a year. The sample size is small since the question is available from 2005 to 2010 only. % LUMEX is a fraction of likely undocumented Mexican immigrants. % Young male is a fraction of men whose age is between 15 and 34.
"Sworn Police" is the log of the number of sworn officers in the city per capita and is obtained from the UCR. The mean value is 200 officers per 100,000 population before log transformation. The coefficient is the percentage change associated with the sanctuary policy. The number of observations is smaller than the main analysis since not all cities have their own police.



Note: The graphs are based on the DIDM estimator introduced by de Chaisemartin and D'Haultfœuille (2020) with 95% confidence interval. The standard errors are based on bootstrapping (100 iterations). The estimations consider state-specific time trends nonparametrically.

Figure 4: The DIDM estimators over time (other outcomes)

Appendix

This online appendix covers the details of the data and the DID regression results with robustness checks.

A Data

The list of sanctuary cities is Table A.1. According to Kittrie (2006), sanctuary policies fall into three types (or combinations of these types): (1) *don't ask*, (2) *don't enforce*, and (3) *don't tell*. A *don't ask* policy limits inquiries related to nationality or immigration status. A *don't enforce* policy limits arrests or detention for immigration offenses. A *don't tell* policy limits information sharing with federal officials. I categorize each policy based on the description in National Immigration Law Center (2008). For example, Executive Order 41 in New York prohibits police officers from inquiring about or disclosing a person's immigration status, including witnesses and victims of crimes, except during the investigation of illegal activity other than a violation of immigration law. The policy is considered as the combination of *don't ask* and *don't tell*.

Moreover, sanctuary policies take one of four legislation types: (1) resolution, (2) ordinance, (3) executive order, and (4) police order. Although all of these legislation types put a restriction on cooperation with the federal government, sanctuary policies could in theory have a different effect by legislation type. However, my data lacked sufficient observations to identify differences across the types, so I focus on the effect of sanctuary policies regardless of the legislation type.

Sanctuary policies also have variations depending on the committed crime, and minor crime offenders could be more likely to be protected. For example, Special Order 40 in Los Angeles allows sharing an offender's information (possibly with ICE), when the offender committed a serious offense or multiple offenses.

For some cities in the list, the actual year of implementation is different from that in the list by O'Brien et al. (2019). This is because these cities implemented a policy before the year, and reconfirmed or amended the policy later. I use the year when the

policy was originally implemented. For example, the year of sanctuary status for San Francisco is set as 2002 in O'Brien et al. (2019), but San Francisco originally became a sanctuary city in 1989, and the policy was reaffirmed in 2002.

Table A.1: List of sanctuary cities

City	State	Year	Legislation Type	Don't ask	enforce	tell
Anchorage	Alaska	2003	Resolution	0	1	0
Chandler†	Arizona	2006	Police order	1	1	1
Berkeley*	California	2007	Resolution	0	1	1
Fresno†	California	2003	Police order	0	1	0
Garden Grove†	California	2007	Police order	0	1	0
Los Angeles	California	1979	Police order	0	1	0
Oakland†	California	2007	Resolution	0	1	0
Richmond*	California	2007	Resolution	0	1	1
San Diego*	California	2008	Police order	0	1	1
San Francisco	California	1989	Resolution	1	1	0
San Jose*	California	2007	Resolution	0	1	0
Hartford†	Connecticut	2007	Resolution	1	1	0
New Haven	Connecticut	2006	Police order	1	1	1
Washington	District of Columbia	1984	Police order	1	0	0
Chicago	Illinois	1989	Executive order	1	0	1
Baltimore	Maryland	2003	Resolution	0	1	0
Boston	Massachusetts	2006	Resolution	0	1	1
Cambridge	Massachusetts	2002	Resolution	0	1	0
Ann Arbor†	Michigan	2003	Resolution	0	1	0
Detroit	Michigan	2002	Resolution	0	1	0
Lansing†	Michigan	2004	Resolution	1	1	1
Minneapolis	Minnesota	2003	Ordinance	1	1	0
St. Paul†	Minnesota	2004	Ordinance	1	1	0
St. Louis	Missouri	2004	Resolution	0	1	0
Jersey City	New Jersey	1996	Resolution	0	1	0
Newark†	New Jersey	2006	Resolution	1	0	0
Albuquerque*	New Mexico	2000	Police order	1	1	0
New York	New York	2003	Executive order	1	0	1
Syracuse†	New York	2003	Resolution	0	1	0
Durham*	North Carolina	2003	Resolution	1	1	0
Cleveland	Ohio	1987	Resolution	-	-	-
Portland	Oregon	2003	Resolution	0	1	0
Salem*	Oregon	1997	Resolution	1	1	0
Philadelphia	Pennsylvania	2002	Resolution	0	0	1
Pittsburgh†	Pennsylvania	2004	Resolution	1	1	0
Austin	Texas	1997	Resolution	1	0	0
Houston	Texas	1992	Police order	1	1	1
Alexandria	Virginia	2007	Resolution	1	1	1
Virginia Beach†	Virginia	2007	Police order	1	0	0
Seattle	Washington	2002	Police order	1	0	0
Madison†	Wisconsin	2002	Resolution	0	1	0
Milwaukee†	Wisconsin	2004	Resolution	0	1	0

Note: All cities in the list have more than 100,000 population. Cities with * do not match the ACS data for public use in IPUMS and hence the cities appear in the baseline sample only. Cities with † are used for the matching DID. A *don't ask* policy limits inquiries related to nationality or immigration status. A *don't enforce* policy limits arrests or detention for immigration offenses. A *don't tell* policy limits information sharing with federal officials. I categorize each policy based on the description in National Immigration Law Center (2008).

Table A.2: Summary statistics (baseline sample)

288 cities, 2981 obs.	Mean	SD	Min	Max
Crime rate				
Violent crime	819.53	433.40	55.3	2743
Homicide	10.70	8.95	0	94.7
Rape	40.02	22.07	1.48	291
Robbery	319.27	190.66	9.98	1024
Assault	467.74	266.85	25.8	1899
Property crime	4709.55	1912.74	1288	12837
Burglary	971.17	501.69	130	3339
Larceny	3026.71	1265.48	700	8331
Auto theft	712.20	453.38	30.3	3137
Sanctuary policy				
Sanctuary city	0.10	0.30	0	1
Don't ask	0.05	0.22	0	1
Don't enforce	0.07	0.26	0	1
Don't tell	0.02	0.15	0	1
Demographics				
Population (thousand)	1538.59	2385.52	100	8401

Note: The crime rate is defined as the number of reported crimes per 100,000 population. Crime rates and demographics are weighted by city population. "Sanctuary city" takes a value of one if an observation is after the sanctuary policy has employed. In total, there are 2969 observations, but there are some missing values in the UCR except homicide and robbery. The number of observations for each crime category is assault (2980), auto theft (2975), burglary (2979), larceny (2971), property (2964), rape (2903), and violent (2902).

Table A.3: Summary statistics (sample with time-variant characteristics)

170 cities, 1749 obs.	Mean	SD	Min	Max
Crime rates				
Violent crime	845.48	433.15	55.3	2596
Homicide	11.48	9.01	0	79.4
Rape	38.42	22.76	1.48	291
Robbery	341.96	188.28	13.4	1021
Assault	478.49	261.90	25.8	1899
Property crime	4553.53	1917.31	1288	12837
Burglary	926.83	500.00	207	3339
Larceny	2917.86	1263.24	832	7992
Auto theft	709.37	474.01	55.7	3137
Sanctuary policy				
Sanctuary city	0.14	0.34	0	1
Don't ask	0.07	0.26	0	1
Don't enforce	0.10	0.30	0	1
Don't tell	0.03	0.18	0	1
Demographics				
Population (thousand)	1959.71	2728.87	100	8401
Female	0.51	0.01	.439	.566
Age (15–29)	0.23	0.03	.143	.596
Age (30–44)	0.23	0.02	.0823	.319
Age (45–59)	0.18	0.02	.0695	.256
Age (60+)	0.15	0.03	.0592	.242
Race and Ethnicity				
White	0.58	0.16	.103	.965
Black	0.23	0.18	.00121	.85
Asian	0.08	0.06	.000518	.456
Pacific islander	0.00	0.00	0	.0311
Native American	0.01	0.01	0	.126
Other race	0.13	0.09	.0014	.518
Hispanic	0.26	0.19	.00225	.957
Education				
Less than High School	0.38	0.07	.156	.653
High School degree	0.42	0.05	.249	.607
College degree	0.19	0.07	.04	.587
Foreigners	0.23	0.13	.00983	.606
Poverty	0.19	0.06	.0298	.445
Unemployed	0.06	0.02	.0135	.172

Note: The crime rate is defined as the number of reported crimes per 100,000 population. Crime rates and demographics are weighted by city population. Homicide and violent crime do not count the September 11 attacks. The sample period is from 2000 to 2010. Demographics in 2001–2004 are by linear interpolation of 2000 Census and 2005 ACS. The statistics are based on 170 cities and 1749 observations, but there are some missing values. The number of observations for each crime category is assault (1749), auto theft (1746), burglary (1748), homicide (1749), larceny (1746), property (1742), rape (1712), robbery (1749), and violent (1712).

Table A.4: Sanctuary and Non-Sanctuary cities (2010)

	Non-sanctuary		Sanctuary	
	Mean	SD	Mean	SD
Crime rates				
Violent crime	628.44	358.49	797.40	377.96
Homicide	7.37	6.10	11.46	8.49
Rape	36.32	19.81	29.78	21.26
Robbery	213.62	135.87	348.72	158.89
Assault	371.12	234.65	429.26	214.33
Property crime	4263.30	1616.54	3418.64	1488.39
Burglary	1046.05	551.51	716.13	483.34
Larceny	2815.48	1131.30	2245.80	901.60
Auto theft	415.37	209.81	456.71	320.72
Demographics				
Population (thousand)	385.67	393.10	3478.68	3233.68
Female	0.51	0.01	0.51	0.01
Age (15–29)	0.24	0.04	0.25	0.03
Age (30–44)	0.20	0.02	0.22	0.02
Age (45–59)	0.19	0.02	0.19	0.01
Age (60+)	0.16	0.03	0.16	0.02
Race and Ethnicity				
White	0.68	0.16	0.52	0.13
Black	0.18	0.17	0.28	0.17
Asian	0.07	0.07	0.11	0.07
Pacific islander	0.00	0.00	0.00	0.00
Native American	0.02	0.01	0.01	0.01
Other race	0.09	0.09	0.12	0.07
Hispanic	0.29	0.23	0.27	0.14
Education				
Less than High School	0.36	0.07	0.35	0.06
High School degree	0.45	0.05	0.41	0.04
College degree	0.18	0.07	0.24	0.08
Foreigners	0.19	0.12	0.27	0.12
Poverty	0.20	0.07	0.22	0.05
Unemployed	0.08	0.02	0.08	0.02

Note: This table compares the summary statistics for sanctuary and non-sanctuary cities as of 2010. The sample used for Table A.3 is divided into the two groups, and this table uses the characteristics in 2010.

B DID

B.1 Specification

The method in this section is based on a difference-in-differences approach. Specifically, I use the following specification:

$$\log y_{it} = b \times Sanctuary_{it} + \alpha_X X + \eta_i + \eta_t + \gamma_i t + \epsilon_{it}. \quad (2)$$

The outcome variable y_{it} is the crime rate of city i at year t. $Sanctuary_{it}$ is a binary variable that indicates the sanctuary status of city i at year t. Its value is one after the year of policy implementation. η_i and η_t are city and time fixed effects, respectively. γ_i captures city-specific linear time trends. The regression coefficient of interest is b and the coefficient is the difference-in-differences estimates of the effect of sanctuary policies. The regressions are weighted by city population, and standard errors are clustered at the city level.

B.2 Main results

The DID results are summarized in Table B.1 and B.2. Table B.1 and B.2 have three columns for each specification, and each b shows the effect of sanctuary policies on the crime type. The first column is the regression result with city and year fixed effects as well as the post-implementation dummy, but other covariates are not included. The result shows a negative effect on violent crime as a broad category. The violent crime rate decreases by 8% compared to the rate before the sanctuary policy, but the coefficient is statistically insignificant. All subcategories have negative coefficients, but none of them show a statistically significant decrease at the 5% level.

Table B.2 shows that sanctuary policies lead to a decrease in the property crime rate by 9.2%. For subcategories, the coefficients show negative signs, but none of them show significance at the 5% level. The burglary rate declines 18.7 %, and the larceny rate declines 4.5% compared to the crime rates before the sanctuary policy. The effect

Table B.1: Baseline regression (violent crime)

	(1)	(2)	(3)
Violent crime			
b	-0.0799	-0.0136	-0.0614
S.E.	(0.0785)	(0.0396)	(0.0644)
# Cities	285	285	285
Obs.	2902	2902	2902
Homicide			
b	-0.0307	-0.0830*	0.0067
S.E.	(0.0677)	(0.0483)	(0.0743)
# Cities	288	288	288
Obs.	2848	2848	2848
Rape			
b	-0.1457	-0.0107	-0.0905
S.E.	(0.0909)	(0.0541)	(0.0676)
# Cities	285	285	285
Obs.	2903	2903	2903
Robbery			
b	-0.1222	-0.0065	-0.0593
S.E.	(0.0788)	(0.0226)	(0.0582)
# Cities	288	288	288
Obs.	2981	2981	2981
Assault			
b	-0.0339	-0.0224	-0.0508
S.E.	(0.0890)	(0.0588)	(0.0917)
# Cities	288	288	288
Obs.	2980	2980	2980
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	N	Y	N
State-Year FE	N	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Dependent variables are log of crime rates.

Table B.2: Baseline regression (property crime)

	(1)	(2)	(3)
Property crime			
b	-0.0924***	-0.0628**	-0.0685
S.E.	(0.0335)	(0.0274)	(0.0416)
# Cities	288	288	288
Obs.	2964	2964	2964
Burglary			
b	-0.1865*	-0.0587*	-0.0721
S.E.	(0.1071)	(0.0329)	(0.0512)
# Cities	288	288	288
Obs.	2979	2979	2979
Larceny			
b	-0.0446*	-0.0375	-0.0777**
S.E.	(0.0227)	(0.0277)	(0.0324)
# Cities	288	288	288
Obs.	2971	2971	2971
Auto theft			
b	-0.2415*	-0.1229**	-0.0532
S.E.	(0.1396)	(0.0586)	(0.0927)
# Cities	288	288	288
Obs.	2975	2975	2975
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	N	Y	N
State-Year FE	N	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Dependent variables are log of crime rates.

on the auto theft rate is negative but insignificant.

The second column controls for the city-specific linear time trend; this is the preferred specification. Most of the point estimates become small in an absolute sense, but this change reduces the standard errors of the estimates as well. As a result, the property crime rate still shows a significant difference induced by the policy and decreases by 6.3%. The homicide rate decreases by 8.3%, a larger effect than the estimator without trend terms.

Finally, the third column controls for the time trend as state-year dummies instead of linear trends. The state-year dummies cause the effects on the property crime and auto theft rates to vanish but make the larceny rate significant. The property crime rate shows a similar effect in terms of magnitude, but the standard error is relatively high. The auto theft rate becomes insignificant due to a smaller point estimate and the high standard error.

The baseline results show negative coefficients in general, but most of them are not statistically significant. However, although the effects on subcategories are not evident, the sanctuary policy leads to a significant decrease in property crime by 6.3%. Moreover, the point estimates and the standard errors give the upper bounds of the magnitude. In the worst case within the 95% confidence interval, the rape rate increases by 9%, but all other crimes can have at most 5% increases.

To investigate the effect on population composition, the UCR data is merged with the ACS data. Hence, this section uses the subsample of the main analysis. Since the sample is different from the main analysis, I check the DID results with the subsample.[29] Table B.3 and B.4 show the results with time-variant characteristics. Each column is the same specification as in Table B.1 and B.2. Because of this sample selection, the significance of coefficients changes for property crime.[30] Property crime still shows a negatively significant effect in the model without linear trends but becomes

[29]Since the demographic data is available from 2000, the sample period for Table B.3 and B.4 is from 2000 to 2010.

[30]This difference is probably from dropping San Diego. Since San Diego has the largest population among dropped sanctuary cities and shows a sharp decrease in the property crime rate after the policy adoption, its impact on the coefficient may be large.

insignificant with linear trends.[31] The coefficients for auto theft become insignificant. The estimated coefficient is smaller than that of the baseline result. The coefficient on the rape rate is 2%, which is larger (in the absolute value) than the baseline result (1%). The point estimate for the robbery rate is less than 1%. The effect on property crime is smaller than the result in the previous section. Overall, although some coefficients change the statistical significance, the subsample analysis gives similar results.

In summary, a sanctuary policy seems to have a negative effect on the general category of property crime in both samples, and some subcategories also show a negative effect. The results for homicide and robbery imply no effect of the policy, while Martínez-Schuldt and Martínez (2017) found no effect on homicide but a negative effect on robbery. A sanctuary policy also seems to reduce some of the remaining categories of crime not analyzed by Martínez-Schuldt and Martínez (2017).

B.3 Efficient government

One concern about the main result is that the negative effects on the crime rate in sanctuary cities may come from better enforcement operations. In other words, the reduction may not be because of the sanctuary policies but because efficient governments induce lower crime rates as well as the adoption of sanctuary policies. To explore this possibility, I use two measures of the efficiency of local governments—the government expenditure on education and interest payments for municipal bonds—as a control variable. First, an efficient government would have better school quality. Since public schools rely primarily on local governments for their budgets, education expenditure is a good measure of school quality. I assume that expenditure on education is unrelated to crimes, at least in the short run,[32] but is related to the efficiency of governments. Another measure is the interest payment for municipal bonds. The

[31] In Table B.4, the effect becomes insignificant under the model with linear trends, probably due to overcontrolling. Goodman-Bacon (2021) pointed out that, when the policy effect changes over time, adding unit-specific trends may bias the DID estimator. The DIDM results suggest that the effect on property crime is increasing over time, and hence the model with linear trends is biased toward zero.

[32] Better education reduces crime (Lochner and Moretti, 2004; Lochner, 2020). However, the main channel argued in the literature is through human capital accumulation, and so the response is not immediate.

Table B.3: Baseline regression with the subsample (violent crime)

	(1)	(2)	(3)
Violent crime			
b	-0.0671	-0.0400	-0.0556
S.E.	(0.0870)	(0.0424)	(0.0730)
# Cities	169	169	169
Obs.	1712	1712	1712
Homicide			
b	-0.0086	-0.0641	0.0420
S.E.	(0.0752)	(0.0587)	(0.0771)
# Cities	169	169	169
Obs.	1687	1687	1687
Rape			
b	-0.1650*	-0.0101	-0.1176
S.E.	(0.0886)	(0.0558)	(0.1020)
# Cities	169	169	169
Obs.	1712	1712	1712
Robbery			
b	-0.1192*	-0.0056	-0.0979
S.E.	(0.0684)	(0.0310)	(0.0546)
# Cities	170	170	170
Obs.	1749	1749	1749
Assault			
b	-0.0127	-0.0515	-0.0268
S.E.	(0.1072)	(0.0611)	(0.0989)
# Cities	170	170	170
Obs.	1749	1749	1749
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	N	Y	N
State-Year FE	N	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note: Linear trend is the city-specific trend. State-Year-FE is to capture state-specific time trend non-parametrically. The subsample is the cities in the UCR merged with the demographic information in the ACS.

Table B.4: Baseline regression with the subsample (property crime)

	(1)	(2)	(3)
Property crime			
b	-0.0809***	-0.0330	-0.0774**
S.E.	(0.0304)	(0.0206)	(0.0315)
# Cities	170	170	170
Obs.	1742	1742	1742
Burglary			
b	-0.1692*	-0.0190	-0.0844
S.E.	(0.1021)	(0.0304)	(0.0614)
# Cities	170	170	170
Obs.	1748	1748	1748
Larceny			
b	-0.0395	-0.0205	-0.0775***
S.E.	(0.0266)	(0.0224)	(0.0275)
# Cities	170	170	170
Obs.	1746	1746	1746
Auto theft			
b	-0.2363**	-0.0769	-0.1062
S.E.	(0.1123)	(0.0754)	(0.0706)
# Cities	170	170	170
Obs.	1746	1746	1746
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	N	Y	N
State-Year FE	N	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note: Linear trend is the city-specific trend. State-Year-FE is to capture state-specific time trend non-parametrically. The subsample is the cities in the UCR merged with the demographic information in the ACS.

Table B.5: Regression of the expenditure on education and the interest payment for municipal bonds

VARIABLES	(1) Education	(2) Interest
Sanctuary	-0.0397	0.00260
	(0.0885)	(0.00361)
Year FE	Y	Y
City FE	Y	Y
Linear Trend	Y	Y
Number of Cities	267	267
Observations	2521	2508

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Education is city government expenditure on education per capita. Interest is the difference of the interest payment over total debt outstanding from the previous year. The data source for Education and Interest is the Annual Survey of State and Local Government Finances.

intuition is that, given the same amount of outstanding debt, an efficient government should pay less for the interest. In particular, I use the difference from the last year,

$$I_{it} = \frac{interest_{it}}{debt_{it}} - \frac{interest_{it-1}}{debt_{it-1}}, \tag{3}$$

where $debt_{it}$ is the amount of total outstanding debt in a city i at time t, and $interest_{it}$ is the interest payment for the outstanding debt at year t. The expenditure and debt data are collected from the Annual Survey of State and Local Government Finances.

The regression results in Table B.5 do not show an evident effect on efficiency. The adoption of the policy is associated with a negative coefficient for the education expenditure and a positive coefficient for the interest payment. However, both of the two efficiency measures have a statistically insignificant coefficient. Hence, the results support that the efficiency is not the source of endogeneity.[33]

[33] I also check endogeneity by using the two measures as additional control variables. The results are similar to the baseline, and the magnitude of the effect is slightly larger.

B.4 Count data

Instead of the log of crime rates, this section uses the counts of each crime as a dependent variable. Table B.6 and Table B.7 use the log of crime count as the dependent variable, and the log of the population is added as an additional covariate. The results are similar to the main results but the coefficients have larger point estimates. For example, the effect on the burglary rate is -5.9% in the main regression, but the log of counts regression indicates -6.4%, which is statistically different from zero at the 5% level.

Table B.6 and Table B.7 show the results of Poisson and negative binomial (NB) regression results. In terms of statistical significance, both Poisson and NB give similar results except for robbery. Under the Poisson model, the sanctuary policy has no effect on robbery, but the NB model with linear trends supports a negative effect of sanctuary policies. Since the standard errors for each crime are high relative to the mean in the summary table, there is an overdispersion problem. Hence, the NB model would be appropriate. The estimator in the NB model $-.0853$ is interpreted as the robbery rate being 8.2% $(= 1 - exp(-.0853))$ lower after the adoption of sanctuary policy.

B.5 Other immigration policies

This section considers county-level or state-level immigration policies.[34] Especially, state-level or county-level sanctuary policies, the Secure Communities (SC) program, and the 287(g) program.

B.5.1 State-level and county-level sanctuary policies

This paper performed city-level analysis, and the sanctuary status of a city is sorely based on the city policy. However, sanctuary policies can be adopted at the state or county level as well. (I call them as upper-level policies.) Although the city police

[34] I control state-level policies in this section. However, if state-level policies have any effect, the policy effect would be captured by the model with state-year fixed effects. Hence, the result tables do not have the model with state-year FEs.

Table B.6: Count data regression (violent crime)

	(1) Log-Linear	(2) Log-Linear	(3) Poisson	(4) Poisson	(5) NB	(6) NB
Violent crime						
b	-0.0913	-0.0173	-0.0394	-0.0448	0.0015	-0.0667*
S.E.	(0.0791)	(0.0399)	(0.0771)	(0.0716)	(0.0340)	(0.0358)
# Cities	285	285	285	285	285	285
Obs.	2902	2902	2902	2902	2902	2902
Homicide						
b	-0.0471	-0.0855*	0.0340	0.0000	0.0222	-0.0093
S.E.	(0.0668)	(0.0466)	(0.0621)	(0.0656)	(0.0379)	(0.0524)
# Cities	288	288	288	288	288	288
Obs.	2848	2848	2981	2981	2981	2981
Rape						
b	-0.1556*	-0.0146	-0.0443	-0.0521	0.0029	-0.0421
S.E.	(0.0899)	(0.0543)	(0.0714)	(0.0651)	(0.0561)	(0.0593)
# Cities	285	285	285	285	285	285
Obs.	2903	2903	2903	2903	2903	2903
Robbery						
b	-0.1329*	-0.0105	-0.0995	-0.0872	-0.0465	-0.0853**
S.E.	(0.0782)	(0.0207)	(0.0715)	(0.0581)	(0.0332)	(0.0352)
# Cities	288	288	288	288	288	288
Obs.	2981	2981	2981	2981	2981	2981
Assault						
b	-0.0389	-0.0261	0.0215	-0.0098	0.0334	-0.0585
S.E.	(0.0913)	(0.0593)	(0.0866)	(0.0900)	(0.0479)	(0.0547)
# Cities	288	288	288	288	288	288
Obs.	2980	2980	2980	2980	2980	2980
Year FE	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y
Linear Trend	N	Y	N	N	N	N
State-Year FE	N	N	N	Y	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note: Linear trend is the city-specific trend. State-Year-FE is to capture state-specific time trend non-parametrically.

Table B.7: Count data regression (property crime)

	(1) Log-linear	(2) Log-linear	(3) Poisson	(4) Poisson	(5) NB	(6) NB
Property crime						
b	-0.1022***	-0.0656**	-0.0814***	-0.0834***	-0.0741***	-0.0843***
S.E.	(0.0325)	(0.0259)	(0.0262)	(0.0286)	(0.0188)	(0.0216)
# Cities	288	288	288	288	288	288
Obs.	2964	2964	2964	2964	2964	2964
Burglary						
b	-0.2066**	-0.0635**	-0.1051	-0.0914**	-0.0442	-0.0540
S.E.	(0.1032)	(0.0304)	(0.0753)	(0.0448)	(0.0333)	(0.0338)
# Cities	288	288	288	288	288	288
Obs.	2979	2979	2979	2979	2979	2979
Larceny						
b	-0.0512**	-0.0402	-0.0536**	-0.0794***	-0.0858***	-0.1030***
S.E.	(0.0239)	(0.0277)	(0.0224)	(0.0249)	(0.0217)	(0.0241)
# Cities	288	288	288	288	288	288
Obs.	2971	2971	2971	2971	2971	2971
Auto theft						
b	-0.2530*	-0.1242**	-0.1141	-0.0964	-0.0690	-0.0683
S.E.	(0.1387)	(0.0582)	(0.0835)	(0.0694)	(0.0467)	(0.0493)
# Cities	288	288	288	288	288	288
Obs.	2975	2975	2975	2975	2975	2975
Year FE	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y
Linear Trend	N	Y	N	N	N	N
State-Year FE	N	N	N	Y	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note: Linear trend is the city-specific trend. State-Year-FE is to capture state-specific time trend non-parametrically.

department deals with crime cases within a city, residents might care about the upper-level regulations. This section confirms the main result by controlling for the upper-level sanctuary policies. This section uses the list of sanctuary jurisdictions in NILC to define sanctuary states and counties. In the list, there are only 4 states and 7 counties identified as sanctuary jurisdictions.

I consider two ways to deal with upper-level sanctuary policies. The first way is that upper-level policies may have an additive impact on the effect of the city-level policy. In this case, the regression is performed with the upper-level sanctuary dummy, which takes a value of one when either the state or county adopts sanctuary policies. The second way is that cities in the jurisdiction may be considered as sanctuary cities whenever the state, county, or city adopts the policies. A city need not have its own sanctuary policies in this case to be a sanctuary city.

The regression results are in Table B.8 and B.9. In the first column, upper-level sanctuary policies are controlled as additional dummy variables in the regression. Alternatively, the second column defines sanctuary status at any jurisdiction level. Compared with the main result, the two specifications show similar point estimates, and property crime and auto theft have statistically negative coefficients. For example, the estimator suggests that the property crime rate decreases by 6.3% if there is a sanctuary policy at any jurisdiction level.

B.5.2 Secure Communities program and 287(g) program

This section considers the two immigration policies adopted by local governments: the SC program and the 287(g) program. The two programs assist the federal government in enforcing immigration law. The SC program started in 14 jurisdictions in 2008, and all local jurisdictions participated in the SC program by January 22, 2013. Under the SC program, U.S. Immigration and Customs Enforcement (ICE) obtains the fingerprints of the county and state arrestees. The first 287(g) agreement was signed with the state of Florida in 2002, and 72 agreements were signed as of October, 2010 (Capps et al., 2011). Section 287(g) in the Immigration and Nationality Act enables local officers to enforce

Table B.8: Sanctuary policies at state and county level (violent crime)

	(1)	(2)
Violent crime		
b (Sanctuary: City)	-0.0134	
S.E.	(0.0399)	
b (Sanctuary: Any)		-0.0167
S.E.		(0.0386)
# Cities	285	285
Obs.	2902	2902
Homicide		
b (Sanctuary: City)	-0.0894*	
S.E.	(0.0470)	
b (Sanctuary: Any)		-0.0808*
S.E.		(0.0482)
# Cities	288	288
Obs.	2848	2848
Rape		
b (Sanctuary: City)	-0.0104	
S.E.	(0.0548)	
b (Sanctuary: Any)		-0.0142
S.E.		(0.0528)
# Cities	285	285
Obs.	2903	2903
Robbery		
b (Sanctuary: City)	-0.0104	
S.E.	(0.0222)	
b (Sanctuary: Any)		-0.0085
S.E.		(0.0224)
# Cities	288	288
Obs.	2981	2981
Assault		
b (Sanctuary: City)	-0.0255	
S.E.	(0.0584)	
b (Sanctuary: Any)		-0.0252
S.E.		(0.0575)
# Cities	288	288
Obs.	2980	2980
Sanctuary (State&County)	Y	N
Year FE	Y	Y
City FE	Y	Y
Linear Trend	Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note: The sanctuary status of cities depends on the state, county, and city level. "b (Sanctuary: City)" is the coefficient for the sanctuary dummy at the city level that takes a value of one if the city itself adopts sanctuary policies. "b (Sanctuary: Any)" is the coefficient for the sanctuary dummy at any level that takes a value of one if the state, county, or city adopts sanctuary policies. "Sanctuary (State&County)" takes one if the state or county that contains the city adopts sanctuary policies. Linear trend is the city-specific trend.

Table B.9: Sanctuary policies at state and county level (property crime)

	(1)	(2)
Property crime		
b (Sanctuary: City)	-0.0661**	
S.E.	(0.0271)	
b (Sanctuary: Any)		-0.0632**
S.E.		(0.0271)
# Cities	288	288
Obs.	2964	2964
Burglary		
b (Sanctuary: City)	-0.0637**	
S.E.	(0.0312)	
b (Sanctuary: Any)		-0.0576*
S.E.		(0.0327)
# Cities	288	288
Obs.	2979	2979
Larceny		
b (Sanctuary: City)	-0.0391	
S.E.	(0.0280)	
b (Sanctuary: Any)		-0.0388
S.E.		(0.0275)
# Cities	288	288
Obs.	2971	2971
Auto theft		
b (Sanctuary: City)	-0.1323**	
S.E.	(0.0561)	
b (Sanctuary: Any)		-0.1209**
S.E.		(0.0585)
# Cities	288	288
Obs.	2975	2975
Sanctuary (State&County)	Y	N
Year FE	Y	Y
City FE	Y	Y
Linear Trend	Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note: The sanctuary status of cities depends on the state, county, and city level. "b (Sanctuary: City)" is the coefficient for the sanctuary dummy at the city level that takes a value of one if the city itself adopts sanctuary policies. "b (Sanctuary: Any)" is the coefficient for the sanctuary dummy at any level that takes a value of one if the state, county, or city adopts sanctuary policies. "Sanctuary (State&County)" takes one if the state or county that contains the city adopts sanctuary policies. Linear trend is the city-specific trend.

a part of federal immigration law, such as screening people and issuing detainers to hold them until the federal officers take custody.

Table B.10 and B.11 summarize how these immigration policies change the main result. The table contains three columns: the result with (1) the SC dummy, (2) the 287(g) dummy, and (3) both the SC and 287(g) dummies. Overall, the estimated coefficients are similar, and the statistical significance of the estimators remains the same. For example, when both SC and 287(g) are controlled, the estimator suggests a 5.8% reduction of property crime, which is slightly lower than the estimator in the main result (6.3%). Since all sanctuary jurisdictions in the data passed the policy by 2008 and the SC program had started in 2008, the SC dummy partially captures the negative effect of sanctuary policies. The estimation results suggest that SC and 287(g) programs do not affect the estimators of sanctuary policies.

B.6 Labor market performance of LUMEX

The section focuses on the labor market outcomes of LUMEX. Specifically, I check the effect on the fraction of the unemployed, the labor force participation rate, the total hours of work, and wages of LUMEX.

In Table B.12, although no effect is found in the model with linear trends, sanctuary policies increase the labor force participation rate in both the baseline model and the model with state-year fixed effects. In particular, the point estimates show a 5-14 percentage point increase in the labor force participation rate. However, the policies do not affect other outcomes. The percentage of unemployed, total hours worked, and the logarithm of wages remain unchanged after the implementation of sanctuary policies. The DIDM estimators in Figure B.1 also find similar results. Thus, while sanctuary policies appear to encourage labor participation, they do not improve the performance of LUMEX in the labor market.

Table B.10: Secure Communities and 287(g) (violent crime)

	(1)	(2)	(3)
Violent crime			
b	-0.0026	-0.0156	-0.0045
S.E.	(0.0319)	(0.0385)	(0.0309)
# Cities	285	285	285
Obs.	2902	2902	2902
Homicide			
b	-0.0713*	-0.0832*	-0.0715*
S.E.	(0.0422)	(0.0483)	(0.0422)
# Cities	288	288	288
Obs.	2848	2848	2848
Rape			
b	0.0023	-0.0109	0.0022
S.E.	(0.0471)	(0.0537)	(0.0468)
# Cities	285	285	285
Obs.	2903	2903	2903
Robbery			
b	0.0038	-0.0078	0.0026
S.E.	(0.0229)	(0.0223)	(0.0228)
# Cities	288	288	288
Obs.	2981	2981	2981
Assault			
b	-0.0124	-0.0257	-0.0156
S.E.	(0.0503)	(0.0571)	(0.0486)
# Cities	288	288	288
Obs.	2980	2980	2980
SC	Y	N	Y
287(g)	N	Y	Y
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	Y	Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Dependent variables are log of crime rates. Linear trend is the city-specific trend.

Table B.11: Secure Communities and 287(g) (property crime)

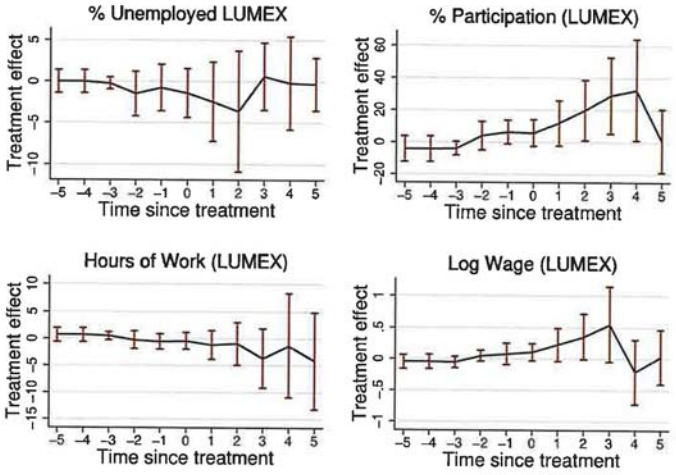
	(1)	(2)	(3)
Property crime			
b	-0.0550**	-0.0653**	-0.0575**
S.E.	(0.0276)	(0.0272)	(0.0277)
# Cities	288	288	288
Obs.	2964	2964	2964
Burglary			
b	-0.0538	-0.0611*	-0.0561
S.E.	(0.0339)	(0.0329)	(0.0341)
# Cities	288	288	288
Obs.	2979	2979	2979
Larceny			
b	-0.0308	-0.0397	-0.0329
S.E.	(0.0294)	(0.0277)	(0.0296)
# Cities	288	288	288
Obs.	2971	2971	2971
Auto theft			
b	-0.1088**	-0.1261**	-0.1118**
S.E.	(0.0477)	(0.0578)	(0.0471)
# Cities	288	288	288
Obs.	2975	2975	2975
SC	Y	N	Y
287(g)	N	Y	Y
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	Y	Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Dependent variables are log of crime rates. Linear trend is the city-specific trend.

Table B.12: Effects on labor market performance of LUMEX

	(1)	(2)	(3)
% Unemployed (Mean: 5.3%)			
b	-1.064	0.377	-0.407
S.E.	(0.653)	(0.807)	(0.843)
# Cities	170	170	170
Obs.	1707	1707	1707
Labor force participation rate (Mean: 69.9%)			
b	5.819***	6.541	13.83***
S.E.	(2.221)	(4.238)	(4.039)
# Cities	170	170	170
Obs.	1707	1707	1707
Weekly hours of work (Mean: 40.3)			
b	0.688**	0.160	-1.144
S.E.	(0.347)	(0.436)	(0.894)
# Cities	168	168	168
Obs.	1691	1691	1691
Log of annual wage income (Mean†: 13,875)			
b	0.179***	0.148	0.169
S.E.	(0.0312)	(0.0982)	(0.107)
# Cities	168	168	168
Obs.	1672	1672	1672
Year FE	Y	Y	Y
City FE	Y	Y	Y
Linear Trend	N	Y	N
State-Year FE	N	N	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
† The mean value is the value before logarithm transformation.
Note: The coefficients for % Unemployed and the labor force participation rate represent the percentage point change.



Note: The graphs are based on the DIDM estimator introduced by de Chaisemartin and D'Haultfœuille (2020) with 95% confidence interval. The standard errors are based on bootstrapping (100 iterations). The estimations consider state-specific time trends nonparametrically.

Figure B.1: The DIDM estimators over time (labor market outcomes)

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Is Immigration Responsible for the Crime Drop? An Assessment of the Influence of Immigration on Changes in Violent Crime Between 1990 and 2000*

Tim Wadsworth, *University of Colorado at Boulder*

Objectives. The idea that immigration increases crime rates has historically occupied an important role in criminological theory and has been central to the public and political discourses and debates on immigration policy. In contrast to the common sentiment, some scholars have recently questioned whether the increase in immigration between 1990 and 2000 may have actually been responsible for part of the national decrease in crime during the 1990s. The current work evaluates the influence of immigration on crime in urban areas across the United States between 1990 and 2000. *Methods.* Drawing on U.S. Census and Uniform Crime Report data, I first use ordinary least squares regression models to assess the cross-sectional relationship between immigration patterns and rates of homicide and robbery among U.S. cities with populations of at least 50,000. Second, I employ pooled cross-sectional time-series models to determine how changes in immigration influenced changes in homicide and robbery rates between 1990 and 2000. *Results.* In the ordinary least squares models, immigration is associated with higher levels of homicide and robbery. However, the pooled cross-sectional time-series models suggest that cities with the largest increases in immigration between 1990 and 2000 experienced the largest decreases in homicide and robbery during the same time period. *Conclusion.* The findings offer insights into the complex relationship between immigration and crime and suggest that growth in immigration may have been responsible for part of the precipitous crime drop of the 1990s.

The relationship between immigration and crime has garnered broad interest from academics, policymakers, and the general public since the late 1800s and early 1900s brought successive waves of immigrants to the United States. Within a short time, the public and their elected officials began associating the new urban residents with drunkenness, violence, and criminal behavior. Many scholars have argued that these negative depictions of immigrants fueled a variety of policies, including anti-immigration

*Direct correspondence to Tim Wadsworth, Department of Sociology, UCB 327, Boulder, CO 80309 (tim.wadsworth@colorado.edu). I will share all data and coding information with those wishing to replicate the study. An earlier version of this article was presented at the 2007 American Criminological Society Annual Meeting. I thank Robert Brame, Richard Rogers, and Jerald R. Herting, as well as the anonymous reviewers, for comments on earlier drafts of the article.

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legislation, Prohibition, and the Harrison Act, one of the first drug regulation laws in the United States (Hagan and Palloni, 1999; Martinez and Lee, 2000). The association between immigration and crime, while remaining strong in the public consciousness for over a century, has never received consistent empirical support. In fact, studies showing that immigrants participated in crime at a lower rate than their native-born counterparts began to emerge as far back as the early 20th century (Immigration Commission, 1911).

Despite a lack of empirical evidence for the relationship, the public's perception of the connection has changed little over time. The general belief that crime is a negative side effect of immigration seems to have become a broadly accepted "social fact." The blame is sometimes aimed only at illegal immigrants and, at other times, all immigrants are considered suspect. Butcher and Piehl (1998:458) cite a 1993 *Time Magazine* poll in which 59 percent of respondents felt that recent immigrants "add to the crime problem" (Nelan, 1993:11). Although it is unclear which came first, public perceptions have been accompanied by political rhetoric and legislation that draws on the supposed connection between immigration and crime. In 1996, presidential hopeful Pat Buchanan stressed the cost of illegal immigration in terms of housing, welfare, and the increase in crime (Butcher and Piehl, 1998). California's Proposition 187 (a stringent anti-illegal-immigrant bill) explicitly pointed to the connection between undocumented immigrants and crime.

Despite concerns over the supposed immigration and crime relationship, and consequent attempts to stem the flow of foreign-born migrants, immigration continued at record high levels throughout the 1990s, with more than 13 million immigrants entering the United States between 1990 and 2000 (Urban Institute, 2002). In a 1993 article in the *Journal of Social, Political, and Economic Studies*, Tanton and Lutton concluded that:

> The alien-related crime that plagues the United States is one of the consequences of its failure to enact and enforce sensible immigration controls .... Since the early days of the Republic, foreign criminal elements have viewed this as a "land of opportunity" ... In past years, American legislators passed laws to try to keep criminals out. But its current leaders lack the same will. (1993:234)

And this, many would argue, is why crime rates should have soared at the end of the 20th century.

But crime rates did not soar. In contrast to the predictions of Tanton and Lutton (1993), and others, the early 1990s brought on the beginning of the most precipitous decline in crime that the United States has ever experienced. Between 1970 and 1990, national homicide rates vacillated between 8 and 10 per 100,000. By 1999, they were below 6 per 100,000 (Blumstein and Wallman, 2000:3). According to National Crime Victimization Survey data, the decreases for other offenses were even greater. National robbery

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victimizations dropped from over 600 per 100,000 in 1990 to less than 200 in 2000, and aggravated assault from about 1,000 per 100,000 in 1990 to about 500 in 2000.

Several books (Blumstein and Wallman, 2000; Conklin, 2003; Zimring, 2007) and dozens of articles have explored a variety of explanations for the drop in crime. The most prominent hypotheses have focused on the growth of incarceration, shifts in the age distribution, changing drug markets, the availability of handguns, economic trends, new policing strategies, and the legalization of abortion. Although empirical tests of the various explanations have faced various methodological challenges, they have generally received mixed support. Most agree that incarceration has contributed to the decrease, but estimates of how much vary greatly (for examples, see Useem and Piehl, 2008; Spellman, 2000). There is also general consensus that shifts in the age distribution (a decrease in the proportion of the population aged 15–29) (Fox, 2000; Zimring, 2007), the stabilization of drug markets (Grogger, 2000; Baumer et al., 1998), and a strong economy (Grogger, 2000) contributed to the decline, though, again, the size of their respective contributions is contested. Agreement is less widespread concerning explanations focusing on firearms (Wintemute, 2000) and the legalization of abortion (Joyce, 2003; Levitt and Donohue, 2001).

Given the popular conception that immigration fuels crime, many were surprised in 2006 when sociologist Robert J. Sampson published an op-ed piece in the *New York Times* questioning whether the U.S. drop in crime could be the result of an increase in immigrants. Sampson wrote:

> social scientists have put forth many explanations for the astonishing drop in crime rates in America over the last decade or so, and yet we remain mystified ... Perhaps we have been overlooking something obvious—something that our implicit biases caused us not to notice ... evidence points to increased immigration as a major factor associated with the lower crime rate of the 1990s .... (2006:A27)

The current research explores the question of whether patterns of immigration influenced changes in violent crime. Specifically, U.S. Census and Uniform Crime Report data are used to examine the cross-sectional and longitudinal relationships between immigration and homicide and robbery in U.S. cities in 1990 and 2000. Two central questions are addressed: (1) Controlling for a host of other factors, do cities with more immigrants and/or more new immigrants demonstrate higher levels of violent crime? and (2) Can the changes in crime rates between 1990 and 2000 be explained in part by immigration patterns? In other words, as some critics have suggested, were cities that received more immigrants during the 1990s more likely to experience increases, or smaller decreases, in violent crime? Or is Sampson correct, in which case cities with the greatest increases in immigration should have experienced the greatest decreases in crime. It is important to note that this analysis does not examine the rate at which immigrants or native-born

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citizens commit crime. Given the nature of aggregate crime data, it cannot be determined whether changes in crime rates are the result of immigrants committing more or less crime than nonimmigrants. Such an analysis would be informative, but it may also miss part of the story. While many have argued that immigrants increase the crime rate by participating in more crime than natives, others have suggested that the process by which immigration leads to crime is driven in part by the strains that immigration places on the native-born population. For instance, cities with high levels of immigration may experience increasing levels of cultural and social disorganization, a flood of low-skill workers, and other stresses on finite resources (Mears, 2001). The current research takes a broad view of the possible connections between immigration and crime and attempts to determine whether, controlling for other factors, patterns of immigration are associated with patterns of violent crime across place and time.

Immigration and Violent Crime: Theoretical Perspectives and Empirical Research

The link between immigration and crime has been largely taken for granted in media and policy circles (Butcher and Piehl, 1998; Hagan and Palloni, 1999; Martinez and Lee, 2000; Mears, 2001). The popular discourse surrounding anti-immigrant legislation rests on the assumption that encouraging, allowing, or not doing enough to prohibit poor, unskilled, and uneducated individuals to immigrate increases crime rates and the danger of victimization. Sometimes the concerns focus on all immigration, other times only illegal immigration, and in much of the discourse a clear distinction is not made. Many theoretical traditions make similar claims (Martinez and Lee, 2000; Reid et al., 2005). Although its recent emphasis has been more on poor black communities, social disorganization theory (Shaw and McKay, 1942) originally claimed that neighborhoods with high levels of immigration also experienced high rates of crime. This influence was not thought to be caused by ethnic predispositions, but the result of population heterogeneity. Shaw and McKay and others argued that ethnic heterogeneity weakened community organizations and informal controls, leading to higher crime rates. The theory also points to the likelihood that the children of immigrants may be more likely to engage in crime (Mears, 2001).

This line of reasoning, which focused on the illicit behavior of the second generation, was supported by reports published in 1931 by the National Commission on Law Observance and Enforcement, often referred to as the Wickersham Commission (Tonry, 1997), and the "it's not the immigrants, it's their kids" argument dominated much of the work of criminologists in the 1920s–1940s. While scholarly interest in the immigration/crime relationship waned with the onset of World War II and a booming economy,

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when contemporary scholars returned to the issue some questioned this longstanding interpretation. In a multi-level analysis of racial and ethnic differences in violent offending, Sampson, Morenoff, and Raudenbush (2005) found that both first- and second-generation Mexican Americans demonstrated lower rates of participation in violence than whites. Much of this disparity was explained by the fact that first- and second-generation immigrants were more likely to come from two-parent households and neighborhoods with high concentrations of immigrants, raising the question of whether high concentrations of foreign-born residents can act as a protective factor against violence by providing extended kinship and enclave-based community networks of supervision and support. To the degree that such enclaves remain for second- and even third-generation immigrants (a process that some have argued distinguishes contemporary immigrants from their European counterparts a century ago), such protective factors may continue to reduce criminal propensity beyond the first generation.

Opportunity theories (Cloward and Ohlin, 1960; Merton, 1938) implicitly address the immigration-crime link by focusing on objective and relative deprivation, along with increased criminal opportunities in the disadvantaged communities where immigrant groups tend to settle (Hagan and Palloni, 1999; Martinez and Lee, 2000; Sampson and Lauritsen, 1997). Given the experience of deprivation, some immigrants may turn to crime as an opportunity for upward mobility. Although Merton's (1938) original version of structural strain theory pointed to financially-motivated crime as an adaptation to blocked legitimate opportunities, subsequent scholars extended the theory by demonstrating how blocked legitimate opportunities could result in non-financially-motivated crime as well (Cloward and Ohlin, 1960). If immigration patterns give rise to marginalized ethnic and cultural communities, these communities will be more likely to develop neighborhood-based subcultures in which either violent or more organized criminal activity can flourish. Sullivan's (1989) and Jankowski's (1991) ethnographic work on Latino peer groups and gangs lend strong support to this possibility.

Both economic and social control theories offer additional explanations for a relationship between immigration and crime. In economic models, individuals in weaker labor market positions have lower opportunity costs and therefore less to lose by participating in crime (Becker, 1968). Similarly, in social control theory, those with weaker bonds to the labor market experience fewer restraints on their behavior and are thus more likely to participate in crime (Crutchfield and Pitchford, 1997; Wadsworth, 2006). To the degree that immigration floods the market with unskilled workers (some of whom will work for lower wages due to their illegal status), it may weaken the labor market positions, and thus increase the criminal propensities, of both immigrant and native workers alike.

A key weakness in how traditional criminological theories address the role of immigration stems from a fairly simplistic conceptualization of the immigration process—one that may have been more accurate in the late

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19th and early 20th centuries than it is today. Immigration is no longer a unidimensional process in which, upon arrival, poor migrants with few job skills join the ranks of economically and socially disadvantaged U.S. citizens. Scholars have noted the great diversity among immigrants in education, skill levels, access to employment networks, and the degree to which they face prejudice in the United States. Segmented assimilation (Portes and Zhou, 1993) describes a process by which the children of some immigrants assimilate into upwardly mobile social positions while others assimilate into less favorable positions, often characterized by concentrated disadvantage and residential segregation. This distribution is largely driven by characteristics of the migrants (including legal status), their social networks, and their country of origin, but can also be significantly influenced by the context of their host community. Although this process is impossible to measure using aggregate-level data, such patterns have important implications when considering the theoretical mechanisms that relate immigration to crime.

Both the macro- and micro-level theories discussed above predict a positive relationship between immigration and crime; however, others have proposed the potential for inverse relationships. At the individual level, proponents of the "healthy immigrant thesis" suggest that individuals choose or are selected by family and community to emigrate based on their likelihood of success (Stephen et al., 1994). The emotional, psychological, and dispositional factors that are likely used to make such a prediction are apt to be inversely correlated with participation in crime and other problematic behaviors. Others have argued that a variety of culturally-based protective factors (extended family, religion, community integration) that are often prominent in immigrant enclaves may help buffer against the strains of poverty, unemployment, and social inequality (Wadsworth and Kubrin, 2007). Both these perspectives suggest that foreign-born migrants should display lower criminal propensities than native-born residents and, as a result, that areas with large concentrations of immigrants should exhibit lower rates of crime.

Others have proposed more macro-level mechanisms. Logan, Alba, and McNulty (1994) note that immigrants often fill important roles in niche economies geared toward certain services or products. To the degree that the low-skill economy is able to absorb workers through these niche markets, communities may experience lower levels of un- or underemployment. Related, Martinez (2002) has argued that relatively low levels of violence in the Latino community, despite high levels of poverty, may be driven by higher than expected levels of (often informal) employment, the result of strong ethnic job networks. Kotkin (2000) has suggested that niche markets and the existence of large numbers of immigrants to work in and support them can have a stimulating effect on other local industries. As noted by Reid et al. (2005), the growth of retail and wholesale trade in ethnic enclaves can increase the need for outside workers in the areas of transportation, warehousing, and building—other key low-skill employment markets.

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The above theoretical arguments have proposed both positive and negative relationships between immigration and crime at both the micro and macro level, but those supporting the "more immigration-more crime" thesis have dominated both the theoretical and the public discourses for decades. The same cannot be said for the empirical literature. In fact, there has been very little support for the claim that more immigration leads to more crime (Butcher and Piehl, 1998, 2006; Hagan and Palloni, 1999; Martinez and Lee, 2000; Mears, 2001). In contrast, the bulk of the findings suggest that immigration provides a protective effect, with strong community ties generating a cultural commitment to conformity and guarding against the criminogenic effects of social disorganization, strain, and economic and social marginalization (Martinez and Lee, 2000). It is also possible that these processes influence both the immigrant and native-born populations by increasing the overall level of stability in the community.

In addition to the work of Sampson, Morenoff, and Raudenbush (2005) discussed above, the few other individual-level studies that addressed the propensities of offending and victimization found that immigrants exhibited similar or lower levels than their native-born counterparts. Martinez (1997) found that Latino immigrants in Miami had a homicide victimization rate less than half of what would be expected given their population. Given the intragroup nature of most crime (Wadsworth and Kubrin, 2004), this suggests disproportionately low levels of offending as well. Similarly, Martinez and Lee (2000) found that Haitian, Jamaican, and Mariel Cuban immigrants in Miami had lower rates of homicide offending than native-born individuals. Drawing on the role of community-based protective factors, Desmond and Kubrin (2009) found that adolescents living in communities with significant concentrations of immigrants are less likely to report participating in violence.

The macro-level studies have also failed to support the commonly assumed positive relationship between immigration and crime, though the findings have been more mixed. Butcher and Piehl (1998) found a positive relationship between both the overall and the violent crime rates and the relative size of the foreign-born population in their sample of 43 metropolitan areas. However, when they removed the largest three cities from their sample, Miami, Los Angeles, and New York, the effect disappeared (1998:469). Reid et al. (2005) found that the proportion of the population comprised of new immigrants has virtually no effect on rates of crime. The one exception to this pattern was homicide, in which case the percentage of new immigrants was negatively correlated with the homicide rate. Focusing on El Paso, Miami, and San Diego, Lee, Martinez, and Rosenfeld (2001) found no consistent evidence for a relationship between growth in immigrant populations and neighborhood homicide rates. In later analyses that also included Chicago and Houston, Martinez (2002) found that immigration had either no effect, or an inverse effect, on most types of homicide among Latinos. In the one piece of research to date that examined the

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relationship between changes in immigration and changes in crime, Butcher and Piehl (1998) found no relationship between changes in crime rates from 1979 to 1990 and changes in the fraction of the population that had immigrated in the previous year. This absence may be due to a small sample of cities ($N = 43$) or the difficulty in identifying patterns of change over very short periods of time.

Theory and public discourse are weighted heavily toward the proposition that immigrants are more likely to commit crime, that areas with more immigrants will have higher crime rates, and that as more immigrants arrive in the United States crime rates will go up. Yet, the empirical evidence at the individual level supports the opposite assertion—that criminal behavior is less prevalent among immigrants, and the evidence at the aggregate level is mixed. The question of whether patterns of immigration influence changes in crime rates over time has received little attention in the empirical literature, and the one study that did explore this question was limited to a relatively small number of geographic areas and very short time periods (Butcher and Piehl, 1998). This incongruity between theory, public opinion, and findings, the absence of longitudinal analyses during a time of explosive immigration and dropping crime rates, and the recent proposal that immigration may have played an important role in the crime drop motivates the current research questions—Do cities with more immigrants experience more violent crime? and Do patterns of immigration influence changes in rates over time? By addressing these issues, the current research can contribute to both our understanding of the aggregate-level relationship between immigration and crime and to the ongoing effort to explain the crime drop of the 1990s.

Data, Methods, and Analyses

Dependent Variables

To measure rates of violent crime, I use 1989–1991 and 1999–2001 UCR data to generate average annual robbery and homicide rates for 1990 and 2000. I focus on homicide and robbery primarily due to reliability issues. The most common critique of UCR data is that many incidents are not reported to the police and thus are excluded from official statistics. This is rarely the case with homicide. Due to both the seriousness of the crime and the presence of a body, it is unlikely that many homicides avoid police detection. While more of a problem, robbery likely suffers less in this respect compared to assault or rape. Robbery is more likely to be viewed as a crime by the victim and to occur between strangers, and is less likely to generate potential feelings of guilt or shame on the part of the victim. All these reasons make it more likely that victims of robbery, more so than victims of rape or assault, will alert the police (Blumstein, 2000).

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Rates based on three years of data are preferable to one year as they account for minor temporal fluctuations and dampen the effects of any individual years with unusually high or low rates. This is especially important when studying relatively small geographic areas. Consistent with much research on violent crime, the unit of analysis is the city. Parker (1989) suggests that the city is the most appropriate level of analysis for two primary reasons. First, given the geopolitical process, larger aggregations such as MSAs can be very heterogeneous. Cities are more homogenous, thus making explanations based on structural forces more meaningful. Second, violent crime is mostly an urban phenomenon. Parker's (1989) analysis shows that roughly 80 percent of homicides took place in cities. The present analyses include cities with at least 50,000 residents in both 1990 and 2000. The U.S. Census published data for 487 cities that met these qualifications. UCR data were available for 459 of these.[1] As homicide and robbery rates are heavily skewed and violate the assumption of a normal distribution, the natural log of the rates is used in the cross-sectional and time-series models. This transformation addresses the problem of nonnormality in the data.

Independent Variables

Given the focus of the article, the key variables of interest are indicators of immigration. The first measure, foreign born, is the proportion of the population that was born outside of the United States. The second indicator, new immigrants, is the proportion of the population comprised of foreign-born individuals who arrived in the United States in the last five years. While it may be informative to disaggregate documented and undocumented immigrants, this is not possible as the Census does not distinguish between the two.[2] To effectively isolate the role of immigration, a number of demographic and structural variables commonly found in macro-level models of violent crime are included in the analyses. Demographic variables

[1]1990 UCR data were missing for the following cities: Encinitas, CA, Rancho Cordova, CA, San Buena Ventura, CA, Santee City, CA, Vista City, CA, Pontiac, MI, Rochester Hills, MI, Santa Fe, NM, Charleston, WV, Huntington, WV, Taylorsville, UT, and Port Arthur, TX. In 2000, UCR data were missing for Nashua, NH, Wheaton, IL, Del Plaines, IL, Elyria, OH, Bloomington, IL, Elgin, IL, Evanston, IL, Decatur, IL, Waukegan, IL, Champaign, IL, Anderson, IN, and Kansas City, KS. An analysis of these cases identified very few differences in regard to key independent variables between these cities and those for which UCR data are available.

[2]Some have also suggested that using the Census to measure immigrants is problematic in that the foreign-born population, especially those who are undocumented, will be undercounted by the Census. While a justifiable concern, in their work aimed at evaluating the degree to which both documented and undocumented immigrant populations are undercounted, Deardorff and Blumerman (2001) suggest that the undercount of undocumented immigrants is likely less than 15 percent and that the undercount of the documented population is significantly less. These estimates are based in part on known undercounts of other hard-to-count groups. While not small enough to be considered negligible, the Census undercount does not appear to be as problematic as some have suggested.

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include the population size (total population of the city), young males (the percentage of the population that is male and between the ages of 15–34), and racial and ethnic composition (the percentage of the population that is black and Latino). Structural variables include poverty (the percentage of the population living below the poverty line), per-capita income, mobility (the percentage of the population over the age of five that has moved in the last five years), divorce (the percentage of the population age 25 and over that has gone through a divorce), educational achievement among young adults (the percentage of the population age 18–24 with at least a high school education), unemployment (the percentage of the population age 25 and over that is unemployed), and job access. The job access variable represents employment opportunities for low-skilled persons and is computed as the ratio of the number of jobs in low-skill industries to the number of people in the population aged 25 and over with a high school diploma or less.[3] These indicators of labor market involvement are especially important as they are likely related to the process by which immigrants decide to migrate to some cities rather than others. Also included are two indicators of residential segregation. The first one measures segregation between blacks and whites (white/black index of dissimilarity), and has usually been found in past research to be positively correlated with violent crime (Wadsworth and Kubrin, 2004). The second is an indicator of segregation between Latinos and whites (white/Latino index of dissimilarity). While this measure is rarely included in models of non race/ethnic-specific crime rates, given the current focus on patterns of immigration, the residential segregation of ethnic enclaves may be an important factor. Last, I include measures of geographic region to account for higher levels of violence in the southern and western regions of the United States.

Not surprisingly, collinearity diagnostics indicate that including poverty, unemployment, and per-capita income would potentially reduce the efficiency of the parameter estimates. As these variables are serving primarily as controls, and I am less interested in their relative contributions, I followed the common practice of using factor analysis to combine the indicators into a composite measure of economic disadvantage. Factor loadings are as follows: poverty (0.831), unemployment (0.895), and per-capita income (-0.857). The factor has an eigenvalue of 2.225.[4] Finally, due to high

[3]I use Kasarda's (1989) definition of an industry group as low skill. Ten of the 17 major industry groups can be classified as being dominated by low-skill jobs. They are (1) agriculture, forestry, and fisheries; (2) mining; (3) construction; (4) manufacturing, nondurable; (5) manufacturing, durable; (6) transportation; (7) wholesale trade; (8) retail trade; (9) personal services; and (10) entertainment and recreational services. It is important to note that this group of industries includes those that most commonly employ un- and semi-skilled immigrants.

[4]Given their correlation, including both the percent foreign born and the percent new immigrant may decrease the efficiency of the parameter estimates. However, excluding one of them would likely lead to a misspecification of the model. Statistically, the main issue with multicollinearity is that it can inflate the standard errors of highly correlated variables. The

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levels of skewness, I use the natural log of the population, young males, percent black, percent Latino, and job access variables.

Analyses

The analyses were carried out in two stages. First, I estimated ordinary least squares regression models to examine the effect of the city-level characteristics on 1990 and 2000 homicide and robbery rates. These models identify the demographic and structural factors that help explain variation in robbery and homicide rates across U.S. cities during the two time periods. Second, I estimated pooled cross-sectional time-series equations in which each city is treated as a distinct unit. Because each city contributes a single observation for each year to the data set, it follows that each city appears in the data set twice.[5] These two observations per city cannot be regarded as independent and the pooled cross-sectional time-series model takes this into account. More importantly, these models assess the degree to which changes in the independent variables are associated with changes in the dependent variables. Time-series models allow for much stronger causal claims as they help control for unobserved time-stable characteristics of the cities and unobserved year-specific influences by including dummy variables for each city and year. This process eliminates potential bias resulting from the exclusion of any variable that either does not change over time or whose influence on the dependent variable does not change over time. Pooled cross-sectional time-series analyses can be modeled as either fixed or random effects. Fixed effects are most appropriate when omitted variables may vary across cases but are constant across time. Random effects offer more efficient estimators and can be useful when some omitted variables are suspected to be constant over time but vary across cases, while others are thought be fixed between cases but vary over time. The Wu-Hausman specification test can be used to determine whether the fixed effects (more conservative) or random effects (more efficient) models are more appropriate (Greene, 1997). I discuss the findings from this test below.

inflation leads to imprecision in the parameter estimates and will likely reduce the power to see significant relationships (Fox, 1991).

[5]In their analysis of the immigration/crime relationship during the 1980s, Butcher and Piehl (1998) suggest that it is best to model changes over one-year periods as it allows for the identification of short term fluctuations. As Census data are only available for each decennial, incorporating yearly measures requires using Current Population Survey (CPS) data. This comes at a cost. CPS data are only consistently available for a small number of cities. Butcher and Piehl (1998) were able to include 49 cities in their analysis) and are collected using sampling procedures that may introduce significant error for counts of small populations. For these reasons, the current analysis foregoes assessing yearly changes in order to include a much larger number of cities.

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Findings

Cross-Sectional Models of Structural Characteristics, Immigration, and Violent Crime

Models 1 and 2 in Table 1 show the influence of structural characteristics on 1990 homicide and robbery rates, respectively. The effects of many of the characteristics are consistent with previous research. Cities with larger total populations, larger black populations, more economic disadvantage, fewer educated young adults, fewer job opportunities for unskilled workers, higher divorce rates, and higher levels of segregation between blacks and whites all experienced more homicide. Turning to the influence of immigration, the relative size of the foreign-born population was a significant factor—cities with more immigrants experienced more homicide. Controlling for the size of the immigrant population, the size of the new immigrant population was not a significant predictor of homicide rates.

The findings for robbery are similar. Again, cities with larger total and black populations, more divorce, less access to low-skill jobs, and higher levels of white/black segregation had higher rates of robbery. Also similar, cities with more immigrants had higher rates of robbery and the proportion of new immigrants was not a significant predictor. There were also some differences between the homicide and robbery models. Cities with larger Latino and young male populations experienced more robbery, while those with more white/Latino segregation had less. Unlike in the homicide model, economic disadvantage and the educational achievement of young adults were not significant predictors of robbery.

Turning to the analyses of homicide and robbery in 2000, there are several important similarities and a few noticeable differences. Model 3 in Table 1 shows the influence of structural characteristics on 2000 homicide rates. Similar to the 1990 homicide models, the overall size of the population, the percentage of the population that was black, the level of economic disadvantage, and the divorce rate are all correlated with homicide in the expected direction. In contrast to the 1990 models, neither of the immigration variables are significant predictors of homicide. Also in contrast to the 1990 models, in 2000, neither job access, the educational achievement of young adults, or white/black segregation is influential, but white/Latino segregation demonstrates a positive relationship to homicide rates.

The 2000 robbery analysis (Model 4 in Table 1) suggests similar processes. Cities with large populations, large black populations, large young male populations, more economic disadvantage and divorce, fewer low-skill jobs, and more white/black segregation reported higher rates of robbery. Also similar to the 1990 models, controlling for other factors, cities with larger immigrant populations had more robberies, while the proportion of new immigrants was not a significant correlate. Unlike the other models, residential mobility was negatively related to 2000 robbery rates.

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TABLE 1

OLS Regression Results for Structural Characteristics on 1990 Homicide and Robbery Rates[a] ($N = 458$)

	1990		2000	
Variables	Model 1—Homicide	Model 2—Robbery	Model 3—Homicide	Model 4—Robbery
Population (LN)	0.153***	0.140***	0.176***	0.097**
	(0.033)	(0.035)	(0.034)	(0.034)
Percent young males (LN)	0.210	0.485*	0.136	0.640**
	(0.203)	(0.216)	(0.213)	(0.216)
Percent black (LN)	0.220***	0.278***	0.212***	0.258***
	(0.021)	(0.022)	(0.023)	(0.023)
Percent Latino (LN)	−0.021	0.068*	−0.057	−0.028
	(0.029)	(0.031)	(0.030)	(0.030)
Job access (LN)	−0.213*	−0.304***	−0.204	−0.271*
	(0.107)	(0.114)	(0.124)	(0.125)
Economic disadvantage	0.169***	0.044	0.340***	0.214***
	(0.035)	(0.037)	(0.043)	(0.044)
Young adult education	−1.189***	−0.623	0.533	−0.273
	(0.309)	(0.329)	(0.348)	(0.353)
Residential mobility	−0.312	−0.738	−0.986	−1.330*
	(0.363)	(0.387)	(0.533)	(0.541)
Divorce	6.057***	12.234***	4.844***	12.156***
	(1.148)	(1.223)	(1.293)	(1.312)
Percent foreign born	1.878*	2.708**	0.530	2.165***
	(0.824)	(0.878)	(0.479)	(0.486)
Percent new immigrants	−0.3489	1.027	−0.626	0.378
	(2.621)	(2.791)	(1.878)	(1.905)
White/black dissimilarity	0.005**	0.012***	0.003	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)
White/Latino dissimilarity	0.004	−0.005*	0.005*	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
West	−0.015	0.0141*	0.151*	0.272***
	(0.067)	(0.071)	(0.069)	(0.070)
Northeast	−0.413***	0.154	−0.292***	0.057
	(0.081)	(0.086)	(0.081)	(0.082)
Central	−0.240***	0.020	−0.054**	0.013
	0.064	0.068	0.066	0.067
Constant	−0.095	3.522	−0.169	3.940
Adjusted R^2	0.770	0.811	0.733	0.794

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

[a]Unstandardized coefficients and standard errors (in parentheses).

Together, the 1990 and 2000 analyses point to a number of significant relationships and the findings are generally consistent with previous research on violent crime. Each of the four models explains a large amount of the

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variation (ranging from 73–81 percent) in homicide and robbery across a large sample of U.S. cities. Most importantly, the findings call into question the recent claims that increases in immigration may be an important contributor to the decline in crime between 1990 and 2000. In contrast, in three of the four models representing the correlates of violent crime rates across two different time periods, the proportion of immigrants in the population is positively correlated with homicide and robbery. However, in trying to understand the relationship between patterns of immigration and patterns of crime over time, these models suffer from two important weaknesses common to cross-sectional analyses—they are unable to model processes that take place over time, and they are vulnerable to biases in making causal claims. Although the above analyses suggest with some certainty that cities with more immigrants experienced more violent crime during time periods with relatively high (1990) and relatively low (2000) crime rates, these analyses are not able to directly address the question of whether cities that experienced more immigration over time experienced greater changes in crime rates. Thus we are on questionable ground to suggest that immigration caused changes in crime rates between 1990 and 2000.

More important perhaps is the vulnerability of cross-sectional models to missing variable bias. Although a variety of city characteristics that previous research has demonstrated to be influential are included in the models, it is entirely possible that there are additional factors that are associated with both immigration patterns and crime rates. This is of special concern when modeling the result of a process (immigration) that is likely shaped by various characteristics of geographic areas. Immigrants move to some cities more so than to others. If the characteristics of the cities that encourage (or discourage) immigrant migration are correlated with crime rates, but are not included in the model, the observed relationship may be spurious. For these reasons, I use pooled cross-sectional time-series analyses to assess changes in immigration and other structural characteristics and violent crime between 1990 and 2000.

Time-Series Analyses of Changes in Structural Characteristics and Violent Crime 1990–2000

Table 2 presents the results of the time-series models examining the factors related to changes in homicide and robbery rates between 1990 and 2000. As discussed above, pooled cross-sectional time-series analyses can be modeled as fixed or random effects models. In the current research, the chi-square values generated from the Wu-Hausman tests indicate that the coefficients estimated in the fixed and random effects models are significantly different and that using the random effects models could be problematic. Therefore, I estimated both the models using fixed effects.

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TABLE 2

Pooled Cross-Section Time-Series Regression Models of Homicide and Robbery Rates[a] ($N = 458$)

	Regression Results	
Variables	Model 1—Homicide	Model 2—Robbery
Population (LN)	0.052	0.127
	(0.136)	(0.111)
Percent young males (LN)	0.852**	0.465
	(0.317)	(0.259)
Percent black (LN)	0.156**	0.076
	(0.060)	(0.049)
Percent Latino (LN)	−0.019	0.179**
	(0.073)	(0.060)
Job access (LN)	0.552*	0.695***
	(0.239)	(0.196)
Economic disadvantage	0.245***	0.154**
	(0.068)	(0.056)
Young adult education	−0.626	−0.170
	(0.615)	(0.503)
Residential mobility	0.516	−0.283
	(0.548)	(0.448)
Divorce	6.375**	5.491**
	(0.235)	(1.922)
Percent foreign born	0.852	−1.631*
	(0.947)	(0.775)
Percent new immigrants	−3.599*	1.677
	(1.671)	(1.367)
White/black dissimilarity	0.005	0.002
	(0.004)	(0.004)
White/Latino dissimilarity	0.002	−0.001
	(0.004)	(0.003)
Constant	8.570	37.182

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

[a]Unstandardized coefficients and standard errors (in parentheses).

Many of the significant effects displayed in Model 1 of Table 2 would be expected from the cross-sectional models presented above. Growth in economic disadvantage and divorce and the size of the black and young male populations resulted in sharper increases, or smaller decreases (more likely given the overall pattern of falling crime rates between 1990 and 2000), in homicide. There were also a number of findings that diverged from the patterns established in the cross-sectional models. Most importantly, while the relative size of the foreign-born population was no longer a significant predictor, the size of the new immigrant population was, and in the opposite

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direction. Cities in which the relative size of the new immigrant population grew between 1990 and 2000 exhibited sharper declines in homicide over the 10-year period. Also, in contrast to the cross-sectional models of homicide, increases in the availability of low-skill jobs served to increase homicide.

The results of the time-series analysis for robbery are similar in many respects to homicide. The influence of economic disadvantage, divorce, and access to low-skill jobs is the same for robbery as it was for homicide. As levels of these indicators rose, so too did robbery. Most importantly, given the focus of the research, immigration demonstrates an inverse relationship with violent crime. Controlling for other factors, increases in the proportion foreign born resulted in smaller increases, or greater decreases, in robbery. In addition to these similarities, there is a notable distinction between the homicide and robbery models. The logged proportion of Latinos in the population had a significant positive effect on robbery.

Together, the time-series analyses of changes in homicide and robbery rates between 1990 and 2000 paint an interesting picture—and one that casts doubt on the implications the cross-sectional models have for understanding changes over time. For the most part, well-established criminogenic structural characteristics exert the expected influence on violent crime. Including immigration in the models, a factor that has often been ignored in empirical analyses despite the attention it has received in public and political discourse, offers new insight into both the influence of immigration and our explanations for the crime drop of the 1990s. In contrast to the results from the cross-sectional models, the pooled cross-sectional fixed effects time-series models, which offer a much stronger approach to assessing causal relationships, offer qualified support to Sampson's and others' claims. Cities that experienced greater growth in immigrant or new immigrant populations between 1990 and 2000 tended to demonstrate sharper decreases in homicide and robbery. Thus, the suggestion that high levels of immigration may have been partially responsible for the drop in crime during the 1990s seems plausible.

Further Tests and Alternative Model Specifications

However, there are a number of potential issues that deserve further examination. The first pertains to the composition of the immigrant population. One concern is the overlap between changes in the immigration variables and changes in the Latino population. As many of the immigrants since 1990 have migrated from Latin American countries, it is possible that controlling for changes in the Latino population is masking the true influence of Latino immigrants. To examine this possibility, I reran the analyses, first excluding the percent Latino to assess the effect of immigration and new immigration without controlling for the size of the Latino

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population, and then excluding the proportions of immigrants and new immigrants to examine the effect of the size of the Latino population without controlling for immigration. The findings are stable when either the immigration variables or the percent Latino variable is removed from the model. Thus, we can be fairly confident that including percent Latino in the model is not masking the true effect of Latino immigration.

The second issue relates to variation in the composition of immigrant populations. As noted above, there are important distinctions across immigrant populations pertaining to SES, skill level, documentation status, and the degree to which they have assimilated. These characteristics may shape how the growth of the immigrant population influences violent crime. Unfortunately, immigrant-specific measures of these concepts are not available in aggregate Census data. However, an indicator of linguistic isolation (percentage of households in which no one age 15+ speaks English well), which is available in the Census data, may serve as a reasonable proxy for some of the unobservable characteristics of the immigrant population. Linguistically isolated households are less likely to be of higher socioeconomic status or to contain adults working in professional occupations (U.S. Census Bureau, 2006).[6]

To examine the role of language ability in the immigrant population, I reran the time-series models including the indicator of linguistic isolation. It was neither statistically significant, nor did its inclusion exert much influence on the other covariates in the model. I also examined whether linguistic isolation conditioned the effect of either of the immigration measures on crime by creating interaction terms. I included each of the multiplicative terms separately in the homicide and robbery models. None of the coefficients representing these effects were significant. While linguistic isolation is not a perfect proxy for socioeconomic status and other factors related to the assimilation process, these findings do suggest that the role of immigration on crime rates is not heavily conditioned by the educational achievement, occupational status, and general level of assimilation of the various immigrant groups.

The third issue concerns variation in the reliability of the Census estimates. Cities with smaller populations may be more vulnerable to biased estimates. To explore this possibility, I reran both time-series models using weighted least squares regression, which included weights for the logged size of the population. The findings were virtually identical to those presented above, suggesting that the role of the immigration variables is not significantly shaped by variations in the reliability of the Census estimates.

[6]It is important to note that the Census defines linguistically isolated households as those in which no members over the age of 15 speak English well. So while there may be long-established households in which some adults, especially grandparents or older parents, do not speak English, those in which no adults speak English are much more likely to be comprised of new immigrants who are on the fringes, or in low-skill sectors, of the labor market. Such individuals are more likely to experience segregation, prejudice, and other barriers to social and economic equality.

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Discussion

The current research was driven by two primary questions that have not been adequately addressed to date. First, controlling for other factors, do cities with larger foreign-born and new immigrant populations experience higher rates of violent crime? And, second, can patterns of immigration help explain the dramatic drop in violent crime rates that occurred between 1990 and 2000? The findings suggest that the answer to both questions is yes, but that the relationships are not straightforward.

In three of the four cross-sectional models there is a positive association between immigration and crime. These findings could be explained by a variety of criminological theories. However, given the findings in the time-series analyses, there is strong evidence that the cross-sectional relationship between immigration and violent crime is spurious. While I have controlled for a variety of known correlates of crime, it is likely that the demonstrated relationships are driven by unobservable city characteristics that are related to both immigration and violent crime. Immigrants are not randomly or equally distributed. The percentage of the city population that is comprised of immigrants ranged from 1 percent to 60 percent in 2000. Cities with large percentages of immigrants have some geographic, economic, historical, or demographic characteristics that shape immigration patterns. These factors may also influence levels of violent crime.

When examined over time, using techniques that control for unobserved time-stable city characteristics and unobserved year-specific influences, there is no longer a positive association between immigration and crime. In fact, increases in the proportions of the foreign born and of new immigrants appear to decrease, not increase, robbery and homicide rates. These findings are more supportive of theoretical perspectives that propose an inverse relationship between immigration and crime. These perspectives include the "healthy immigrant thesis" and the role of protective cultural and neighborhood factors. If relatively healthy, well-adjusted, "motivated" individuals infuse a population with noncriminogenic characteristics, we would expect a decline in crime. If immigrant communities bring with them culturally-based protective factors that buffer between strains, such as poverty, assimilation, and marginalization, and antisocial outcomes such as crime, we would anticipate that as communities grow, rates of violent crime will shrink. From a social disorganization/community efficacy perspective, to the degree that the immigrant communities engendering these protective and stabilizing forces are located in ethnically heterogeneous areas, the effects may spread beyond immigrants to the native populations by increasing the overall stability and organization of the community. It is also possible, as Kotkin (2000) and Reid et al. (2005) have suggested, that the growth of immigrant communities has been accompanied by the growth of niche markets, the economic benefits of which extend beyond the foreign-born community. All these possibilities are consistent with the findings and seem

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theoretically plausible. However, there is no way to test the different causal mechanisms or arbitrate between them given the limitations of the data. Unfortunately, it is also not possible to disaggregate the effects of legal and illegal immigration. As such, while there is some evidence from a sample of released offenders that the recidivism rates of documented and undocumented immigrants are similar (Hickman and Suttorp, 2008), we are unable to examine whether the effects of immigration on crime are unique for documented versus undocumented immigrants.

One puzzling finding concerns the positive effect of job access on robbery. Job access exhibited a consistent negative effect on both homicide and robbery in the cross-sectional models, but it appears to have a positive relationship with robbery across time. One explanation concerns the migration patterns of nonimmigrants. Cities that experienced a larger increase in low-skill jobs between 1990 and 2000 may have served as migratory destinations for a larger proportion of low skilled and less educated young men—a group that is more likely to participate in violent crime. Although the time-series models control for changes in the young male population, as well as for the population of young adults aged 18–24 who have graduated from high school, these measures may not entirely control for the proportions of this population who are not participating in the legitimate labor market and pursue economic gains through violent crime.

The evidence that cities that experienced the largest growth in the proportion of foreign-born and newly arrived immigrant populations experienced larger decreases in violent crime between 1990 and 2000 suggests that Sampson (2006) may be right—that immigration may be partly responsible for the decrease in violent crime. Computing estimates based on the pooled cross-sectional time-series models discussed above suggests that, controlling for a variety of other factors, growth in the new immigrant population was responsible, on average, for 9.3 percent of the decline in logged homicide rates, and that growth in total immigration was, on average, responsible for 22.2 percent of the decrease in logged robbery rates.[7]

Conclusion

Criminological theory, conventional wisdom, and public and political discourse have long suggested a positive relationship between immigration and crime. Although early disorganization theorists made clear that this connection was not driven by the proclivities of the foreign born, but was caused by the structural contexts created by immigration patterns, they did

[7]These computations are based on taking the differences in the means of the independent variables (percent foreign born and percent new immigrant) across the two time periods (value of mean in 1990 minus value of mean in 2000), multiplying them by their regression coefficients, and dividing the product by the 1990–2000 differences in the logged homicide and robbery rates.

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not deny the association. From the late 1800s to the present, the association between immigration and crime has been a center point of anti-immigrant discourse and public policy. Although there has been scant empirical research to support such claims, they have persisted with little debate. It has only been in the last decade or two that scholars have begun to empirically question these longstanding assertions, and little support has been uncovered.

In 2006 Robert Sampson proposed that not only have immigrants not increased crime, but they may be partly responsible for one of the most precipitous declines in crime that the United States has ever experienced. The current findings offer empirical support to this claim. Time-series models suggest that the widely-held belief that has motivated much of the public and political discourse about immigration and crime is wrong. In contrast, the research offers initial support for the idea that the increase in immigration was partially responsible for the decrease in homicide and robbery in urban areas between 1990 and 2000.

The current work offers strong evidence concerning the relationship between immigration and crime; however, given the nature of the data, it is impossible to evaluate the relative utility of the various theoretical propositions. The data do not include neighborhood-based indicators that would allow a thorough examination of many of the proposed theoretical mechanisms, nor do they tell us whether it is the criminal behavior of immigrants or that of natives that is shaping overall patterns of homicide and robbery. This is one of the fundamental weaknesses of centrally collected aggregate-level crime data. Although using local police and self-report data to examine patterns of offending across different immigrant and native-born populations over time introduces new methodological challenges, it would likely be a useful avenue for future research. In the meantime, however, this and other research that contradicts popularly held notions that high levels of immigration result in more crime should play an important role in challenging the public discourse as we begin to shape new immigration policy for the 21st century.

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Neighborhood Immigration, Violence, and City-Level Immigrant Political Opportunities

Christopher J. Lyons,[a] María B. Vélez,[a] and Wayne A. Santoro[a]

Abstract

Using a multilevel comparative framework, we propose that politically receptive city contexts facilitate the viability of marginalized neighborhoods. To illustrate this proposition, we examine the relationship between immigrant concentration and neighborhood violence. Drawing on political process and minority incorporation theories, we argue that favorable immigrant political opportunities will strengthen the often-found inverse relationship between immigration and crime at the neighborhood level. Unique data from the National Neighborhood Crime Study (Peterson and Krivo 2010a) provide demographic and violence information for Census tracts in a representative sample of 87 large cities. We append this dataset with city-level measures of immigrant political opportunities, such as the extent of minority political incorporation into elected offices and pro-immigrant legislation. Multilevel instrumental variable analyses reveal that the inverse relationship between immigrant concentration and neighborhood violent crime is generally enhanced in cities with favorable immigrant political opportunities. We speculate that this occurs because favorable political contexts bolster social organization by enhancing trust and public social control within immigrant neighborhoods. Our findings demonstrate that the fate of neighborhoods marginalized across ethnicity and nativity are shaped by the responsiveness of political actors and structures to their concerns.

Keywords

immigration, violence, political opportunity structures, neighborhoods

The Chicago school of sociology revolutionized the understanding of how neighborhood conditions pattern the uneven spatial distribution of social problems. Rather than viewing social problems as the product of the clustering of certain kinds of people, urban sociologists highlighted the costs of social and economic marginalization for neighborhoods' social organization (Shaw and McKay 1942). According to prevailing perspectives, extensive poverty and other adverse conditions disrupt the ability of marginalized communities to organize around common interests. A focus on the dynamics of neighborhood social organization has proven helpful in explaining such diverse aggregate outcomes as rates of infant mortality, low birth weight, child maltreatment, teenage childbearing, dropping out

[a]University of New Mexico

Corresponding Authors:
Christopher J. Lyons and María B. Vélez, Department of Sociology, University of New Mexico, MSC05 3080, 1 University of New Mexico, Albuquerque, NM 87131-0001
E-mail: clyons@unm.edu; mvelez@unm.edu

of high school, suicide, and violence (Brooks-Gunn, Duncan, and Aber 1997a, 1997b; Bursik and Grasmick 1993; Krivo and Peterson 1996; Park and Burgess 1924; Sampson 2010; Sampson, Morenoff, and Gannon-Rowley 2002; Sampson, Raudenbush, and Earls 1997; Shaw and McKay 1942). Yet because the vast majority of research on neighborhoods has been conducted *within* a given municipality, urban ecology scholarship has been left with an empirical and theoretical blind spot: neighborhoods are not islands unto themselves. Rather, they are nested within cities and city environments shape neighborhood outcomes. How larger structural forces at the city level worsen or ameliorate social problems within marginalized neighborhoods remains under-analyzed and under-theorized.

We move beyond neighborhood boundaries by investigating how city-level political environments influence the distribution of social problems across neighborhoods. Our central thesis is that the fate of neighborhoods marginalized across class, race, ethnicity, and nativity partly depends on the receptivity of local political actors and structures to their needs. Politically receptive cities can engender trust in the political system and thus encourage residents of marginalized neighborhoods to become civically engaged, develop a sense of attachment to and ownership of their neighborhoods, and mobilize on behalf of neighborhood concerns (Williams 1998). Moreover, politically responsive cities can provide resources and opportunities across a myriad of domains—education, jobs, policing, city services, neighborhood festivals, community outreach, political participation, and health—that shore up internal community processes and shape neighborhood viability.

To illustrate the role of city politics in shaping intra-neighborhood dynamics, we chose a politically divisive issue as our case study: the contemporary relationship between neighborhood immigrant concentration and violence. For more than a century, public opinion in the United States has expressed concern over immigration. Public opinion and political officials' discourse often equate immigrants with a plethora of social problems, especially in regard to deviance and crime (Martinez 2006). Consistent reference to the criminal menace of the immigrant population implies that stricter policies limiting immigration or curbing immigrant access to resources are part and parcel of crime control. Indeed, supporters of recent anti-immigrant policies, including Arizona's SB 1070, Georgia's HB 87, and Alabama's HB 56, argue that such laws will make communities safer (Morse 2011; Redmon 2011).

Decades of sociological research temper these xenophobic fears (Cohen 1931; Martinez 2006; Sampson 2006; Taft 1933). In fact, social science research commonly finds an *inverse* relationship between immigrant concentration and local violence rates (Desmond and Kubrin 2009; Feldmeyer 2009; Kubrin and Ishizawa 2012; Lee, Martinez, and Rosenfeld 2001; Martinez 2002; Martinez, Lee, and Nielsen 2004; Reid et al. 2005). Scholars most often argue that rather than destabilize neighborhoods, immigrants invigorate local areas and fortify crime control processes.

To investigate how city-level political environments shape the immigration-violence relationship, we reach beyond standard criminological theories by drawing on social movement scholarship on political opportunity structures and political science research on minority incorporation (Browning, Marshall, and Tabb 1984; Meyer 2004). Combining insights from these traditions, we propose the concept of "immigrant political opportunities," which refers to the political receptivity or vulnerability of cities to meeting the needs and demands of immigrant communities. Cities have open, or favorable, immigrant opportunities when supportive politicians are in office, the broader community is receptive to immigrant issues, cities adopt pro-immigrant legislation, minorities are bureaucratically incorporated into police departments, and the form of government grants immigrants and their allies institutional access to and influence in municipal decision-making. We argue that these city-level factors condition the link between immigrant concentration and neighborhood violence: immigration will be especially helpful in reducing

neighborhood violence in cities that are sympathetic to the needs of immigrant communities. We test this multilevel thesis and speculate that favorable immigrant political opportunities bolster social organization by setting in motion a "spiral of trust" (Gay 2002:718; see also Williams 1998) among immigrants and the broader community and enhancing public social control within immigrant neighborhoods. In contrast, city regimes hostile to immigrant needs engender mistrust of civic institutions (Kirk et al. 2012), undercutting the ability of neighborhoods with higher concentrations of immigrants to engage in crime control. City contexts with few immigrant political opportunities should weaken the protective association between immigrant concentration and violence at the neighborhood level.

Little research has linked city contextual factors within a multilevel framework to elucidate the neighborhood-level connection between immigrant concentration and violence. We employ unique data from the National Neighborhood Crime Study (NNCS) that provide, for the first time, large-scale multilevel demographic and violent crime data for 8,931 Census tracts nested within a representative sample of 87 large cities (Peterson and Krivo 2010a). We append this dataset with city-level measures of immigrant political opportunities culled from secondary sources. These multilevel data allow us to assess variation in the association between immigrant concentration and neighborhood violence across a large sample of cities and to test whether the often-found inverse relationship between immigration and violence is indeed enhanced in cities with open immigrant political opportunities.

IMMIGRANT REVITALIZATION AND NEIGHBORHOOD VIOLENCE

Whereas studies in a few cities report null or positive relationships between immigrant concentration and violence, the lion's share of contemporary macro research finds immigrant concentration is inversely associated with violence (Feldmeyer 2009; Kubrin and Ishizawa 2012; Lee et al. 2001; Martinez 2002, 2006; Martinez et al. 2004; Martinez, Rosenfeld, and Mares 2008; Martinez, Stowell, and Lee 2010; Ousey and Kubrin 2009; Reid et al. 2005; Wadsworth 2010).[1] Neighborhoods with large shares of immigrants often have lower levels of violence and drug-related crime than do similarly situated areas with fewer immigrants (Kubrin and Ishizawa 2012; Lee et al. 2001; Martinez et al. 2004; Martinez et al. 2008; Stowell and Martinez 2007). At the municipal level, cities that experienced growth in immigrant concentration saw greater reductions in violence between 1990 and 2000, net of common controls (Stowell et al. 2009; Wadsworth 2010). Despite a number of classic challenges to causal inference relating to endogeneity and selection bias, these results are relatively robust across cross-sectional and longitudinal designs.[2]

The predominant explanation as to why immigrant concentration reduces neighborhood violence is known as the "immigrant revitalization perspective" (Feldmeyer 2009; Lee et al. 2001; Martinez 2002; Martinez et al. 2004; Reid et al. 2005).[3] Immigrants' capacity to revitalize communities stems from at least two factors. First, immigrants help fortify social organization. In particular, intact two-parent families and strong ties among families and neighbors characterize contemporary immigrant communities. Strong ties and family cohesion bolster processes of informal social control and may represent a key mechanism though which immigration benefits social organization (Ebaugh and Curry 2000; Feldmeyer 2009; Ousey and Kubrin 2009). Moreover, an influx of immigrants can expand and strengthen community institutions, including churches, schools, and immigrant-focused agencies (Chinchilla, Hamilton, and Loucky 1993; Ley 2008; Theodore and Martin 2007). Community institutions facilitate crime control efforts and help defend community interests because they organize activities that

create networks among residents, provide programming for community youth, connect communities to mainstream individuals and institutions, and help recruit external resources for the community (Ley 2008; Theodore and Martin 2007).

Second, because immigrants typically settle in poorer neighborhoods (Vélez 2009), an influx of recent immigrants in large numbers may invigorate local economies and redevelop urban cores. Specifically, immigration can alleviate or reverse depopulation trends and the withdrawal of business capital (Reid et al. 2005). Research on ethnic enclaves echoes the claim that immigration can stimulate economic revitalization. Ethnic enclaves have extensive ethnic divisions of labor and immigrants with sufficient social capital to create jobs and higher wages (Portes and Zhou 1993). Although many residents in ethnic enclaves may be poor, the majority work and should have greater attachment to the labor market. High neighborhood employment levels signal integration into conventional life that may make crime less lucrative and viable (Martinez 2002).

IMMIGRANT POLITICAL OPPORTUNITIES

Sociologists have long emphasized the importance of local political settings for the well-being of communities with concentrations of marginalized residents. Lieberson (1980), for example, contends that governments played a decisive role in aiding the assimilation of Southern, Central, and Eastern European immigrants across industrial centers in the first half of the twentieth century. Perhaps most important, governments granted voting rights to immigrants and actively recruited them as voters. Likewise, Dahl (1961) suggests that Italian American success in securing local political positions in New Haven, Connecticut assisted the employment of co-ethnics in civil service positions. Other research documents the experience of Black neighborhoods and reveals how hostile political climates can set marginalized neighborhoods on a path of disorder and decline. Local governments played vital roles in creating segregated Black neighborhoods by, for instance, passing ordinances in the early 1900s making it illegal for Blacks to move into White neighborhoods and ignoring widespread racial discrimination in mortgage lending (Farley, Danziger, and Holzer 2002; Hirsch 1998; Massey and Denton 1993). Paralleling this attention to political context, immigration scholars note that policies of the receiving government influence immigrant incorporation patterns (Ellis and Almgren 2009; Portes and Zhou 1993; Winders 2012).

We extend this logic to argue that political contexts of reception shape the ability of neighborhoods with immigrant concentration to control crime. Specifically, we contend that processes of immigrant revitalization should be enhanced in cities with favorable political opportunities, such as Washington, DC, where policies limit local enforcement of immigration laws and minorities have high levels of political representation. In contrast, immigrant revitalization may be attenuated in cities with unfavorable, or closed, political opportunities, such as Phoenix, where the local police department works closely with federal immigration officials embedded in the agency, and the mayor commissioned a new policy in 2008 mandating that police question all arrestees about citizenship status (Hoffmaster et al. 2010; Newton and Kiefer 2008).

To elaborate on our multilevel thesis, we offer the concept of immigrant political opportunities to capture the extent that a city is politically receptive or vulnerable to immigrant demands and concerns. We develop immigrant political opportunities conceptually and operationally by referencing two literatures. First and most directly relevant is political process theory within the study of social movements. Political process theory argues that the broader political environment, termed the political opportunity structure, profoundly shapes social movement mobilization and policy outcomes (for a review, see Meyer 2004). Politically open contexts encourage constituencies to mobilize and facilitate government response to their concerns.[4] Scholars operationalize political opportunities in various ways, including receptivity of elected

officials to movement demands (Jenkins, Jacobs, and Agnone 2003; Meyer and Minkoff 2004), the extent that elites have made policy decisions favorable to constituency needs (Costain 1994; McAdam 1982), the presence of institutional aspects of the polity that grant challengers access and influence over policy decisions (Amenta and Poulsen 1996; Eisinger 1973), and amenability of the audience to movement issues (Gamson 1975; McCammon et al. 2001; Santoro 2008).

Second, we employ insights from the racial/ethnic politics literature focusing on the political and bureaucratic incorporation of minorities (Browning, Marshall, and Tabb 1984, 1990; de la Garza 1988; Marrow 2009; Marschall and Ruhil 2007; Mindiola and Gutierrez 1988; Mladenka 1989; San Miguel 1984; Santoro 1995, 1999). The bulk of this work focuses on Black, and to a lesser degree Latino, incorporation and examines the benefits of in-group representation. Specifically, this research examines how minorities, through electoral and protest strategies, have secured elected positions, become a part of governing coalitions, or become incorporated into civil service jobs. This line of work highlights substantive benefits of political incorporation for minority residents, including passage of policies favorable to minorities, like civilian police review boards and affirmative action, as well as appointment of minorities to important public sector jobs (Browning et al. 1984, 1990; de la Garza 1988; Mindiola and Gutierrez 1988; Mladenka 1989; San Miguel 1984; Santoro 1995, 1999). Similarly, minority incorporation into civil service positions, or bureaucratic incorporation, can lead to adoption of a wide range of policies and administrative procedures beneficial to minority residents, including bilingual educational programs, access to health care, and public school curricula emphasizing Latino history (Jones-Correa 2008; Lewis and Ramakrishnan 2007; Marrow 2009; Theobald and Haider-Markel 2008). Minority political and bureaucratic incorporation also can have symbolic value by enhancing minorities' trust and perceptions of legitimacy in local political actors, law enforcement, and the government more broadly (Gay 2002; Mansbridge 1999; Marschall and Ruhil 2007; Theobald and Haider-Markel 2008). Such social-psychological consequences can increase subsequent levels of minority participation in political affairs (Bobo and Gilliam 1990; Tate 1993).

From these literatures, we conceptualized five dimensions of immigrant political opportunities at the city level. Our models test the extent to which each of these immigrant political opportunities conditions the association between immigration and violence at the neighborhood level.

Latino and Asian American political incorporation. Representation of Latinos and Asian Americans in municipal elected offices represents the political incorporation of immigrant constituents. Among the foreign-born population in 2000, 78 percent were born in Latin American or Asian countries (52 and 26 percent, respectively; Malone et al. 2003). Cities that incorporate these populations into municipal offices should be especially responsive to immigrant needs and demands. A wealth of data show that Latino politicians, for example, are more likely than their Anglo counterparts to advocate for legislation beneficial to constituency concerns (Mindiola and Gutierrez 1988; Mladenka 1989; San Miguel 1984; Santoro 1999). Santoro (1999), for instance, found that Latino politicians were vocal critics of English-only laws and helped defeat their passage.

Minority bureaucratic incorporation. Incorporation of minorities into mainstream public service and bureaucratic institutions reflects another immigrant political opportunity. Compared to elected political officials, civil servants may have more in common with the public, and therefore may be especially responsive to the public's needs (Kranz 1976; Krislov 1974; Meier, Stewart, and England 1991; Theobald and Haider-Markel 2008). Particularly salient for our analysis of violence, minority incorporation into police departments should institutionalize immigrant-friendly law-enforcement and administrative procedures, and, in turn, improve everyday

interactions with residents (Jones-Correa 2008; Lewis and Ramakrishnan 2007; Marrow 2009; Theobald and Haider-Markel 2008). Minority bureaucratic incorporation into the police force helps guard against hostility between the police and minority/immigrant residents.[5]

Pro-immigrant legislation. Immigrant-related policy climates vary considerably across cities. Some jurisdictions have sought to enhance the reach of immigration enforcement via 287(g) and "secure communities" legislation and restrict immigrant access to public education, driver's licenses, health care, and other amenities. Other governments have taken pro-immigrant stances by declaring themselves "sanctuary cities" and limiting cooperation with immigration enforcement efforts (Lewis and Ramakrishnan 2007; National Immigration Law Center 2006). In the latter case, adoption of immigrant-friendly sanctuary policies represents a clear marker of cities with favorable immigrant political opportunities.

Audience receptivity to immigrant issues. The audience, defined as third parties within the relevant political system who are initially uninvolved in a conflict (Santoro 2008; Schattschneider 1960), crucially determines movements' ability to mobilize and secure policy concessions. A supportive audience creates incentives for elites to respond favorably to challenger claims and encourages a challenging group to mobilize. A number of scholars have measured the presence of a supportive audience for liberal causes by using the electorate's voting record for liberal politicians (Okamoto and Ebert 2010; Santoro 1999; Santoro and McGuire 1997). Liberal voting patterns suggest a local political ideology relatively amenable to immigrant mobilization and concerns. Okamoto and Ebert (2010), for example, found that immigrants were more likely to organize and protest in cities that cast a greater percentage of votes for the Democratic presidential candidate in 2000.

Governmental structure. Social movement and political science research notes that governmental structure shapes challengers' access and influence in policymaking decisions (Amenta and Poulsen 1996; Kitschelt 1986). At the municipal level, numerous scholars—including Eisinger (1973), who coined the term political opportunity structure—assert that compared to council-manager forms of government, a mayor-council government grants minorities greater access and influence in local politics. According to this argument, other municipal forms, particularly council-manager governments, tend to "mute the demands" of minorities (Karnig 1975:92) by stressing "professional rather than patronage style(s) of operation" (Slack and Sigelman 1987:674) and therefore are likely to oppose pro-immigrant policies that benefit immigrant constituencies.

IMMIGRANT POLITICAL OPPORTUNITIES, SOCIAL ORGANIZATION, AND VIOLENCE

Why should favorable city-level immigrant opportunities strengthen the inverse relationship between immigration and crime at the neighborhood level? Although we lack data to test the mechanisms attendant to our multilevel thesis, we discuss them because theoretical propositions should make causal mechanisms clear. With social disorganization as a guiding framework, we argue that cities with open immigrant political opportunities should bolster social organization—the basis for effective crime control—within neighborhoods with concentrations of immigrants. Social organization refers to local neighborhoods' ability to solve commonly experienced problems and realize common goals, including safety from crime (Bursik 1988). Immigrant political opportunities should enhance neighborhood social organization in two ways. First, cities with open immigrant political opportunities can generate a "spiral of trust" that improves communication between officials and immigrants, promotes legislation protecting immigrant interests, and generates greater system-level trust in government on the part of immigrant

groups (Williams 1998). By strengthening the trust that immigrants place in the civic process, receptive places defend against social isolation and cynicism of the legal and political system (Kirk et al. 2012). Research indicates that trust can encourage minorities' participation in political affairs (Bobo and Gilliam 1990; Tate 1993) and forms a key foundation for collectively efficacious organization against crime (Sampson et al. 1997). Immigrant trust in the political structure can facilitate involvement in social and political life, attachment to and ownership of their neighborhoods, and mobilization on behalf of neighborhood concerns.

Second, favorable political regimes may enhance the capacity of neighborhoods with greater concentrations of immigrants to exert public social control. Public social control refers to residents' ability to forge ties to local government and economic actors and thereby gain the resources needed to control crime (Bursik 1989; Bursik and Grasmick 1993; Kubrin and Weitzer 2003; Squires and Kubrin 2006; Vélez 2001). Neighborhoods with strong ties to the police and local public officials, for instance, tend to have high levels of informal social control (Silver and Miller 2004), low levels of household and personal victimization (Vélez 2001), and success in collective crime-control efforts (Carr 2005). In cities with open immigrant political opportunities, public officials should be more willing to broker ties with communities containing immigrant concentrations and allocate them resources. Correspondingly, immigrant neighborhoods located in open regimes should be relatively trusting of local politicians and police, view them as more legitimate, and thus be more likely to work with them (Theobald and Haider-Markel 2008).

Case studies provide illustrations of how favorable regimes can enhance immigrants' trust in public officials and fortify social organization via public social control. Lewis and Ramakrishnan (2007) report that a police department with significant minority police representation, notably a Black police chief, adopted sympathetic policies when interacting with an immigrant community, including mandating cultural diversity training for officers, hiring bilingual officers, and implementing a "don't ask, don't tell" strategy for enforcing immigration law. The authors contend that the department's immigrant-friendly policies sent a message to the immigrant community that law enforcement was responsive to their concerns and thus could be trusted. As such, the immigrant community was more likely to interact and engage in problem solving with local police officers.

In his study of an immigrant neighborhood in Los Angeles, Sandoval (2009) claims it took the election of a Latino mayor and city council-members to convince neighborhood residents they could trust city officials and work with them on local crime-control efforts. For example, only after Latinos became part of the dominant governing coalition were residents willing and able to work with city officials to reduce crime that plagued a neighborhood park. Residents joined forces with the police to enact a community-policing program in which citizens regularly met with police and city bureaucrats to strategize about community crime-control efforts. This stimulated residents' informal surveillance of the neighborhood park and mobilization of the law when residents spotted illegal behaviors. The result was the park's transformation from "LA's violent epicenter of drugs and crime" to a place relatively free of crime (Sandoval 2009:154).[6]

DATA AND METHODS

We used unique data from the NNCS (Peterson and Krivo 2010a), the first large-scale dataset that includes crime and sociodemographic information for tracts nested within a representative sample of U.S. cities. The full NNCS dataset includes 91 cities (with populations of more than 100,000 in the year 2000) and 9,593 Census tracts. Missing data on key variables (including spatial lags of violent crime) reduced our analytic sample to 8,931 tracts nested in 87 cities.[7] The large sample of cities generalizes to most large urban places in terms of crime rates, racial/ethnic composition and segregation, and economic disadvantage

(Krivo, Peterson, and Kuhl 2009; Peterson and Krivo 2010b). These data also include cities with varied histories of immigrant settlement, including traditional gateway and non-gateway cities (Singer 2004). We supplemented these data with measures of immigrant political opportunities at the city level collected from a variety of secondary sources.

Dependent variables. We examined two forms of violent crime: the number of homicides and the number of robberies reported to police departments between 1999 and 2001 at the tract level.[8] We used multiyear counts to minimize the impact of annual fluctuations, especially for smaller units (Peterson and Krivo 2010a). Homicide is the most serious and reliably reported violent crime and also the focus of much previous immigration-crime research (Graif and Sampson 2009; Martinez 2002; Martinez et al. 2010; Reid et al. 2005; Wadsworth 2010).[9] Although less reliably reported to police, robbery is more common. Ideally, we would also include other types of violence (e.g., assault and rape), but missing data for these crimes in the NNCS warrant limiting our focus to homicide and robbery to maximize the number of cities in our analysis. We present separate models for homicide and robbery in light of the possibility that the nature of the immigration-crime relationship may vary by type of violence (Hagan and Palloni 1999; Stowell 2007; Stowell et al. 2009).

Tract-level covariates. The majority of our tract-level predictors came from the 2000 Census. Our main focus at the tract level is the relationship between immigrant concentration and violence. Previous research measures immigrant concentration in varied ways, typically with some combination of Census indicators of the total foreign-born population, recent foreign born, linguistic isolation, and percentage Latino (Desmond and Kubrin 2009; Graif and Sampson 2009; Kubrin and Ishizawa 2012; Martinez et al. 2010; Ousey and Kubrin 2009; Peterson and Krivo 2010b; Reid et al. 2005; Wadsworth 2010). Following previous research, we gauged immigrant concentration by indexing two measures (average *z*-scores; $\alpha = .92$): recent foreign born, which represents the percentage of the population that is foreign born and arrived in the United States since 1990, and linguistic isolation, or the percentage of households where no one age 14 years or older speaks English well. In sensitivity analyses, we explored a variety of single-item measures of immigration (percentage foreign born, recent foreign born, and linguistic isolation) and another multi-item index of immigrant concentration (percentage Latino, recent foreign born, and linguistic isolation). These alternative measures produced very similar patterns of violent crime to those discussed here using our two-component index of immigrant concentration.

We included a number of well-established predictors of neighborhood violence. Concentrated disadvantage is an index (average *z*-scores) of six items: the poverty rate, extent of joblessness (percentage of persons age 16 to 64 years who were unemployed or out of the labor force), low-wage jobs (percentage of workers in the six occupations with the lowest average incomes), professional jobs (percentage of employed civilian population age 16 years and older in management, professional, and related occupations [reverse coded]), high school graduates (percentage of adults age 25 years and older with at least a high school degree), and percentage of households that were single-mother families ($\alpha = .91$). We measured racial/ethnic composition of neighborhoods with a set of dummy variables that contrast predominately White (reference group) tracts with Black, Latino, minority (Black and Latino), or integrated tracts. Following Krivo and colleagues (2009), we classified predominately White, Black, or Latino tracts as those where the respective group comprised 70 percent or more of the tract population in 2000. Minority tracts represent areas where Blacks and Latinos combined for 70 percent or more of the population, but neither group alone was more than 70 percent. All other tracts indicate integrated areas, or tracts with relatively more balanced population compositions. We measured residential instability

with an index (average *z*-scores) of the percentage of renter-occupied units and the percentage of residents age 5 years and older who lived in a different dwelling in 1995 ($\alpha = .69$). We accounted for the percentage of the population that was male and between 15 and 34 years of age to control for the crime-prone population. We also measured population tract size to address issues of over-dispersion in our Poisson models.

Finally, we constructed spatial lags for each of our dependent variables to control for spatial autocorrelation in violence. The spatial lags represent the average of each violent crime count (homicide or robbery) for Census tracts that are geographically adjacent to the immediate tract. We computed spatial lags by multiplying tract characteristic values by a row standardized first-order spatial contiguity matrix that utilized a queen criterion (see Peterson and Krivo 2009, 2010b). The diagonal of the matrix is filled with zeros, indicating that a tract is not a neighbor of itself.

City-level covariates. We used five measures of immigrant political opportunities, each corresponding to one of our five dimensions of the concept.[10] To capture minority political incorporation, we tallied the total number of Latino and Asian elected municipal officials, such as the mayor and members of the city council. These data came from the *National Directory of Latino Elected Officials* (2000) and the *National Asian and Pacific American Political Almanac* (2000–01). Because the number of Latino and Asian municipal elected officials in a city is at least partly a function of each group's population size, we divided the number by the population of Latinos and Asians in the city. Given the resulting small number, we multiplied by 100,000 to represent the *rate* of elected officials per 100,000 Latinos and Asians. We measured the extent of minority bureaucratic incorporation into the police force by constructing a ratio of the percentage of the police force (sworn officers) that was Hispanic, Black, or Asian and the percentage of the city population that was Hispanic, Black, or Asian. Values below one indicate underrepresentation in the police force relative to the minority population in the city, whereas values over one represent over-representation. We include Black police officers in our measure because political science research on this topic suggests that representation of Black officers and chiefs sets the stage for tolerance toward other racial/ethnic minorities (Browning et al. 1984, 1990; Lewis and Ramakrishnan 2007; Saltzstein 1989).[11] Data for minority bureaucratic incorporation into the police force came from the Law Enforcement Management and Administrative Statistics (2000).

Drawing from the National Immigration Law Center (NILC 2006), the Congressional Research Service (Seghetti, Vina, and Ester 2004), and related research (Carro 1989), we determined favorable immigrant legislation based on whether a municipality was a sanctuary city due to at least one law or formal resolution limiting local enforcement of immigration laws as of 2001. As one example, in 2000 the San Diego Police Department instituted Procedure 6.18, which states, "officers shall not make an effort to look for violations of immigration law" (NILC 2006:3). The procedure also prohibits officers from releasing undocumented persons to the U.S. Immigration and Naturalization Service (INS) or Border Patrol agents when they are victims or witnesses of crime, involved in traffic violations, or seeking medical treatment. According to a Congressional Research Service report on sanctuary policies, there are "conflicting views as to what constitutes a sanctuary policy" (Kim and Garcia 2008:4). We relied on listings provided by the NILC (2006), Seghetti and colleagues (2004), and Carro (1989) to identify cities with at least one *formal* sanctuary-level law or resolution as of 2001. Our resulting measure may miss more informal or de facto practices. Our focus on formal policies, however, is consistent with our theoretical framework because formal policies arguably represent the visible face of government.

We measured audience receptivity to immigrant issues with electorate voting records.

Specifically, we calculated the percentage of votes cast for the Democratic presidential candidate (Gore) in 2000 to gauge the extent that the broader audience was likely supportive of pro-immigrant and other liberal causes. Data for this measure came from the 2000 *County and City Data Book* published by the U.S. Census Bureau (2001).[12] Finally, to measure governmental structure, we compared cities with a mayor-council form of government (coded as one) versus a council-manager form of government (coded zero) using data from the *Municipal Yearbook* (2000).[13]

Using 2000 Census data for cities, we controlled for several other city-level characteristics associated with different levels of neighborhood violence: the city-level disadvantage index, measured in a parallel fashion to the neighborhood indicator; residential mobility (percentage of the population age 5 years and older who lived in a different residence in 1995); percentage non-Latino Black; percentage Latino; percentage of the total city population that was foreign born; and percentage of young males age 15 to 34 years. We also included dichotomous variables for South and West regions, with cities in other regions as the referent. We controlled for non-Latino White and non-Latino Black segregation with an index of dissimilarity across Census tracts within a city (Krivo et al. 2009). Table 1 lists means, standard deviations, and minimum and maximum values for all city- and tract-level variables.

ANALYTIC STRATEGY

Given the multilevel structure of our data, we estimated a series of hierarchical generalized linear models (HGLM) that correctly account for the non-independence of observations, with 8,931 tracts (level-one units) nested within 87 cities (level-two units). We grand-mean centered all continuous variables. Because we analyze relatively rare events within small units, we fitted Poisson models with homicide or robbery counts as the outcome. Given Poisson model assumptions, we tested whether the mean and variance of the dependent variables were equal and found that the variance for both homicide and robbery were considerably larger than their means, indicating significant over-dispersion. Hence, we accounted for over-dispersion in the level-one variance. In the HGLM framework, a Poisson model with over-dispersion is analogous to a negative binomial model (Raudenbush and Bryk 2002; Snijders and Bosker 1999). We specified that these counts had variable exposure by tract population and thereby transformed the outcome to violent crimes per capita rates (Osgood 2000).

We explored whether the neighborhood association between immigrant concentration and violence varied substantially across cities by allowing for random variation in the slope of immigrant concentration. We then attempted to account for any random variation in the estimate of immigrant concentration on violent crime with cross-level interactions that assessed the moderating potential of city-level immigrant political opportunities. Following prior work by Peterson and Krivo (Krivo et al. 2009; Peterson and Krivo 2010b), we also let the slope of concentrated disadvantage vary randomly in all models.

Although NNCS data provide unique advantages for studying how city-level opportunities influence local neighborhood processes, the cross-sectional nature of these data poses some challenges to causal inference. Specifically, we acknowledge classic concerns about endogeneity and selectivity when determining the causal effect of immigrant concentration on tract-level violence. For example, selection of immigrants into less violent-prone areas may account for the inverse association between immigration and violence. Furthermore, immigrants' characteristics, such as criminal propensity, may guide selection into tracts. If this is the case, observed cross-sectional relationships between immigration and violence would be misleading.

We attempted to address endogeneity and selectivity concerns in a number of ways. First, we controlled for prior (i.e., 1990) foreign-born population in sensitivity models to account for the fact that immigrants move into places that already have high immigrant concentration and possibly lower violence

Table 1. Means and Standard Deviations of Dependent and Independent Variables

	Mean	SD	Min.	Max.
Tract Level (N = 8,931)				
Homicide (1999 to 2001)	1.53	2.53	.00	33.00
Robbery (1999 to 2001)	44.79	49.46	.00	997.00
Immigrant concentration	.04	1.97	−1.60	11.88
Immigrant concentration 1990	.02	1.95	−1.36	10.38
Population	3930.39	2112.07	301.00	23960.00
Disadvantage	−.01	.87	−1.67	3.71
Residential instability	.03	.87	−2.13	2.74
Latino tract	.08	.27	.00	1.00
Black tract	.16	.37	.00	1.00
Minority tract	.07	.26	.00	1.00
Integrated tract	.34	.47	.00	1.00
Percent young males (15 to 34 years)	15.87	5.75	.00	55.92
Percent foreign born 1990	12.23	15.15	.00	86.90
Spatial lag of homicide	1.60	1.78	.00	18.00
Spatial lag of robbery	47.20	37.04	.00	346.40
City Level (N = 87)				
Disadvantage	.01	.88	−2.02	2.43
Residential mobility	52.74	5.89	31.93	66.52
Percent Black	18.83	16.58	.53	81.02
Percent Latino	19.04	18.56	1.09	90.46
Percent foreign born	15.52	12.36	1.58	72.11
Percent young males (15 to 34 years)	16.00	2.42	11.24	24.09
South	.36	.48	.00	1.00
West	.28	.45	.00	1.00
White/Black segregation	47.55	18.22	14.28	85.19
Latino/Asian elected officials (per 100,000)	.08	.25	.00	2.32
Police minority incorporation	.52	.22	.08	1.13
Sanctuary city	.14	.35	.00	1.00
Percent votes for Gore	52.00	.10	.27	.85
Mayor-council government	.44	.50	.00	1.00

rates. Second, we utilized a multilevel instrumental variable approach that attempts to purge our measure of immigrant concentration of endogeneity with violence (see Ebbes, Bockenholt, and Wedel 2004; Wooldridge 2002). This required finding a theoretically justified instrument that is strongly correlated with immigrant concentration yet not correlated with the residuals from our immigration-violent crime models. We followed previous work (MacDonald, Hipp, and Gill forthcoming; see also Card 2001) and used prior levels of immigrant concentration in 1990, specifically an index of the percent of foreign born and percent of linguistically isolated persons, as an instrumental variable for immigrant concentration in 2000. Research clearly indicates that immigrants settle into areas with other immigrants (Alba and Nee 2005; Card 2001), so we have ample reason to suspect that previous immigrant concentration in 1990 will correlate strongly with immigrant concentration in 2000. Indeed, across 8,931 Census tracts the bivariate correlation between the 1990 and 2000 measures is strong and positive (.853). Furthermore, as Table A1 in the Appendix shows, immigrant concentration in 1990 significantly predicts

immigrant concentration in 2000, net of the covariates used in our violence models. In contrast, immigrant concentration in 1990 does not correlate with the residuals of our base models in Table 2 for homicide or robbery. It is important to note that we cannot directly test the requirement of exogeneity (i.e., no correlation between the disturbance term and our instrumental variable) because it is an unobservable concept (Wooldridge 2002). However, given that prior immigration was measured a decade prior to our measures of violence, it is a predetermined variable that is exogenous by construction. As such, we can think of no reason to suspect prior levels of immigrant concentration would violate the assumption of exogeneity (MacDonald et al. forthcoming).

Selection concerns may arise at the city level as well. If we find that city context moderates the neighborhood relationship between immigration and violence, we must contend with the alternative explanation that immigrant political opportunities guide the selection of immigrants into cities. Our large sample of 87 cities captures varied immigrant realities in terms of immigrant supply and demand and likely mitigates some of the concern about endogeneity and selection at the city level. In all models, we controlled for region to account for the fact that immigrants from various sending countries have unique geographic settlement patterns. We also controlled for overall levels of foreign born because immigrants may select into cities with established co-ethnics. Furthermore, as reported in Table A2 in the Appendix, we found few systematic sociodemographic differences in characteristics of politically open versus closed cities. As one example, sanctuary cities are no more likely to have higher concentrations of foreign-born populations than are cities without sanctuary policies. The lack of systematic differences in structural characteristics across cities by immigrant opportunities decreases the concern that opportunity structures guide the selection of certain kinds of immigrants.

We first show baseline models for homicide and robbery (Table 2). The first model tests their relationship with immigrant concentration, net of controls, with slopes allowed to vary randomly for tract-level immigrant concentration and disadvantage. Subsequent models allow for the possibility that tract immigrant concentration is an endogenous predictor and account for endogeneity via a two-stage residual inclusion (2SRI) process (Terza, Basu, and Rathouz 2008), in which residuals from a first-stage equation are included with the endogenous predictor in second-stage models. For nonlinear models like ours, Terza and colleagues (2008) demonstrate the superior consistency of 2SRI compared to traditional two-stage predictor substitution (2SPS), where predicted values from the first-stage equation replace the endogenous variable in second-stage models. In the first-stage multilevel models (Table A1 in the Appendix), immigrant concentration in 1990 predicts immigrant concentration in 2000 along with other tract- and city-level covariates. In the second stage, we followed Terza and colleagues (2008) and included level-one residuals from the first stage along with the measure of immigrant concentration in 2000. Including residuals in these models adjusts the coefficient of immigrant concentration (purging endogeneity) and provides a Hausman-style test of the significance of endogeneity.

RESULTS

Table 2 presents base-line models of the association between immigrant concentration and homicide and robbery counts. For both outcomes, Model 1 reveals that neighborhoods with greater immigrant concentration had fewer homicides and robberies. This represents the average effect of immigration on homicide and robbery for 8,931 tracts across 87 large cities and is consistent with expectations of the immigrant revitalization hypothesis. Both models also show that the slope for immigrant concentration varies substantially across cities, as evidenced by the significant immigrant concentration variance components (bottom panel of Table 2). The average association across our

Table 2. Multilevel Poisson Models (with Variable Exposure) of Neighborhood Homicide and Robbery, National Neighborhood Crime Study 2000

	Homicide				Robbery					
	Model 1		Model 2		Model 1		2SRI Model 1		2SRI Model 2	
	b	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE
Tract Level (N = 8,931)										
Disadvantage	.707***	.033	.705***	.030	.650***	.044	.571***	.035	.632***	.044
Residential instability	.109***	.022	.108***	.015	.272***	.014	.256***	.019	.266***	.014
Percent young males	.005	.004	.005	.003	.002	.003	.004	.004	.001	.003
Latino tract[a]	.683***	.129	.676***	.062	.099	.086	.131	.123	.036	.088
Black tract[a]	.863***	.097	.866***	.051	.211**	.060	.322***	.069	.251***	.060
Minority tract[a]	.826***	.101	.825***	.051	.155**	.063	.254**	.078	.140**	.063
Integrated tract[a]	.557***	.062	.556***	.040	.204**	.058	.266***	.064	.186**	.060
Spatial lag	.077***	.015	.077***	.004	.006***	.001	.006***	.001	.006***	.001
Immigrant concentration	−.066***	.016	−.064***	.019	−.057**	.019	−.061**	.022	−.025	.021
Stage-one residuals			−.004	.017			−.036	.026	−.042**	.012
City Level (N = 87)										
Disadvantage	−.057	.093	−.057	.090	−.064	.079	−.048	.072	−.060	.079
Residential mobility	.008	.010	.008	.012	−.003	.012	.004	.010	−.003	.012
Percent Black	.012**	.004	.012**	.004	.015***	.004	.009**	.003	.015***	.004
Percent Latino	.007	.005	.007	.005	.004	.005	.002	.004	.005	.005
Percent foreign born	−.015†	.008	−.016**	.006	−.002	.006	−.004	.005	−.004	.006
Percent young males	.015	.021	.015	.023	.011	.019	−.020	.018	.011	.020
South[b]	−.040	.091	−.040	.109	−.133	.091	−.121	.080	−.131	.091
West[b]	.253**	.109	.254**	.119	.136	.103	.084	.099	.151	.103
White/Black segregation	.016***	.003	.016***	.003	.016***	.003	.014***	.002	.016***	.003
Intercept	−8.96***	.03	−8.986***	.083	−4.998***	.071	−5.031***	.072	−4.991***	.072
Variance Components (SD)										
Intercept	.297***		.297***		.364***		.307***		.367***	
Tract disadvantage	.143***		.143***		.391***		.304***		.393***	
Immigrant concentration	.075***		.076***		.156***				.158***	

Note: SE refers to robust standard errors; tract population 2000 as variable exposure.
[a]Referent = White tract.
[b]Referent = other region.
†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests); 2SRI refers to two-stage residual inclusion models.

sample is clearly negative. Yet, the random slope for immigrant concentration indicates that for tracts in some cities, immigrant concentration was inversely associated with violence, whereas for tracts in other cities, immigrant concentration had the opposite (positive) association. The estimated association between immigration and homicide ranged from negative (–.216) to positive (.084) for 95 percent of cities in our sample; for robbery, the variation was greater (from –.369 to .255).[14] In additional analyses not shown, we introduced the percentage of the population that was foreign born in 1990 as a crude control for selection of immigrants into tracts. Controlling for foreign born in the previous decade does little to change the estimate of immigrant concentration on homicide or robbery.

We now address the likelihood that immigrant concentration is an endogenous regressor. As explained earlier, the 2SRI models include residuals from the first-stage equation (Table A1 in the Appendix) along with immigrant concentration. The 2SRI model for homicide allows the slopes for immigrant concentration and disadvantage to vary randomly across cities. Results mimic those presented in Model 1: immigrant concentration was inversely associated with homicide on average, and the slope for immigration varied substantially across cities. Although we caution that our instrumental variable approach cannot provide incontrovertible evidence that immigration *causes* less homicide on average, consistency across our models, as well as previous research, buttress this claim.

For robbery, the first 2SRI model restricts between-city variation in the slope of immigrant concentration to zero. The coefficient for immigrant concentration in this model is negative and significant. The second 2SRI model for robbery specifies the slope for immigrant concentration to vary randomly across cities. As indicated in the lower panel, the chi-square test of the variance component for immigration is statistically significant and relatively large. In contrast to homicide, after accounting for this substantial variation across cities in the relationship between immigration and robbery, the average point estimate for immigration is negative but no longer significant at traditional levels.

Next, we examined the extent to which city-level immigrant political opportunities moderate the immigrant concentration-violence relationship. Empirically, we did so by testing for cross-level interactions between tract-level immigrant concentration and each of the five measures of city-level immigrant political opportunities. Table 3 presents results for homicide; Table 4 presents results for robbery.[15] In terms of homicide, we found evidence of significant moderating effects in the manner we predicted for all but one of our five indicators of political opportunity. Political incorporation of Asians and Latinos into elected municipal offices (Model 1) and bureaucratic incorporation of minorities into police departments (Model 2) both moderate the immigration-homicide relationship. In particular, the negative association between tract-level immigrant concentration and neighborhood homicide was strengthened in cities with minority incorporation into both elected offices and the police force. In Model 3, the negative interaction between sanctuary cities and tract immigration suggests that the protective association between immigration and homicide is greater in cities with pro-immigrant legislation. This implies that tracts benefit most from immigrant concentration in terms of homicide reduction in cities that limit local enforcement of federal immigration laws. Model 4 reports a negative and significant interaction between the percentage of votes cast in the 2000 general election for Gore and tract-level instrumented immigrant concentration, suggesting that Democratic support enhances the inverse relationship between immigration and homicide. The lone nonsignificant interaction with city-level immigrant political opportunities is found in Model 5: mayor-council governments do not significantly moderate the effect of immigrant concentration on homicide compared to council-manager governments.

For illustration, Figure 1 graphs the conditional relationship between immigrant concentration, political incorporation of Asians and Latinos, and homicide. High or low values

Table 3. Multilevel Poisson 2SRI Models (with Variable Exposure) of Neighborhood Homicide with Cross-Level Interactions, NNCS 2000

	Model 1		Model 2		Model 3		Model 4		Model 5	
	b	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE
Tract Level ($N = 8{,}931$)										
Disadvantage	.709***	.037	.707***	.036	.707***	.038	.710***	.037	.705***	.038
Residential instability	.109***	.022	.108***	.021	.108***	.022	.109***	.021	.109***	.022
Percent young males	.004	.004	.004	.004	.004	.004	.004	.004	.004	.004
Latino tract[a]	.678***	.139	.650***	.141	.670***	.152	.669***	.145	.676***	.141
Black tract[a]	.865***	.102	.859***	.096	.866***	.103	.857***	.102	.866***	.102
Minority tract[a]	.827***	.104	.816***	.100	.819***	.108	.812***	.108	.824***	.104
Integrated tract[a]	.554***	.065	.540***	.062	.554***	.066	.551***	.065	.556***	.065
Spatial lag homicide	.077***	.015	.077***	.014	.077***	.015	.077***	.015	.077***	.015
Immigrant concentration	−.073**	.027	−.061**	.024	−.051	.032	−.061**	.026	−.059	.032
x Latino/Asian elected officials	−.154**	.046								
x Police minority incorporation			−.233***	.048						
x Sanctuary city					−.057**	.025				
x Percent votes Democrat							−.304***	.076		
x Mayor-council									−.009	.027
Stage-one residuals	−.004	.029	−.016	.030	−.006	.032	−.003	.029	−.004	.030
City Level ($N = 87$)										
Disadvantage	−.043	.091	−.062	.092	−.063	.094	−.074	.089	−.045	.093
Residential mobility	.010	.010	.007	.011	.007	.010	.006	.010	.011	.010
Percent Black	.013**	.004	.015**	.004	.013**	.004	.014**	.004	.012**	.004
Percent Latino	.008	.005	.010†	.005	.007	.005	.007	.005	.006	.005
Percent foreign born	−.016†	.008	−.017**	.007	−.017†	.009	−.015†	.008	−.015†	.009
Percent young males	.015	.022	.026	.023	.017	.021	.016	.020	.011	.022
South[b]	−.044	.086	.034	.096	−.038	.092	−.064	.092	−.025	.093
West[b]	.241**	.102	.265**	.116	.266**	.114	.275**	.111	.262**	.107
White/Black segregation	.016***	.003	.015***	.003	.017***	.003	.017***	.003	.015***	.003
Latino/Asian elected officials	−.127	.115								
Police minority incorporation			.007	.198						
Sanctuary city					−.051	.100				
Percent votes Democrat							−.471	.325		
Mayor-council									.075	.086
Intercept	−8.984***	.086	−9.004***	.099	−8.982***	.093	−8.980***	.091	−9.028***	.101

Note: SE refers to robust standard errors; tract population 2000 as variable exposure.
[a]Referent = White tract.
[b]Referent = other region.
Tract disadvantage and immigrant concentration specified with random slopes: †$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table 4. Multilevel Poisson 2SRI Models (with Variable Exposure) of Neighborhood Robbery with Cross-Level Interactions, NNCS 2000

	Model 1		Model 2		Model 3		Model 4		Model 5	
	b	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE
Tract Level ($N = 8,931$)										
Disadvantage	.633***	.044	.632***	.043	.638***	.045	.637***	.045	.632***	.044
Residential instability	.266***	.014	.266***	.014	.267***	.014	.268***	.013	.266***	.014
Percent young males	.001	.003	.001	.003	.001	.003	.001	.003	.001	.003
Latino tract[a]	.250***	.060	.249***	.060	.234***	.057	.231***	.055	.247***	.058
Black tract[a]	.139**	.063	.139**	.063	.131	.069	.115	.072	.138**	.065
Minority tract[a]	.185**	.060	.186**	.060	.195**	.058	.186**	.060	.189**	.059
Integrated tract[a]	.006***	.001	.006***	.001	.006***	.001	.006***	.001	.006***	.001
Spatial lag robbery	.035	.087	.035	.088	.018	.105	.031	.097	.035	.090
Immigrant concentration	−.024	.021	−.025	.021	−.009	.023	−.034	.023	−.010	.028
x Latino/Asian elected officials	−.007	.266								
x Police minority incorporation			−.019	.087						
x Sanctuary city					−.131**	.036				
x Percent votes Democrat							−.292**	.093		
x Mayor-council									−.036	.027
Stage-one residuals	−.042**	.012	−.043**	.012	−.046**	.016	−.032**	.013	−.041**	.012
City Level ($N = 87$)										
Disadvantage	−.063	.080	−.041	.079	−.083	.077	−.053	.078	−.054	.077
Residential mobility	−.004	.012	−.002	.013	−.005	.011	−.001	.011	.000	.011
Percent Black	.015***	.004	.014**	.004	.015***	.004	.014**	.004	.015**	.004
Percent Latino	.004	.005	.003	.005	.006	.005	.005	.005	.004	.005
Percent foreign born	−.004	.006	−.003	.006	−.005	.006	−.004	.006	−.003	.006
Percent young males	.010	.020	.012	.021	.015	.020	.010	.019	.008	.020
South[b]	−.111	.098	−.120	.094	−.132	.091	−.130	.096	−.111	.091
West[b]	.178	.110	.157	.106	.157	.103	.138	.103	.171†	.102
White/Black segregation	.017***	.003	.016***	.003	.017***	.003	.016***	.003	.015***	.003
Latino/Asian elected officials	.084	.067								
Police minority incorporation			.202	.143						
Sanctuary city					−.066	.106				
Percent votes Democrat							.153	.307		
Mayor-council									.092	.073
Intercept	−5.005***	.076	−4.999***	.073	−4.986***	.071	−4.990***	.072	−5.047***	.083

Note: SE refers to robust standard errors; tract population 2000 as variable exposure.
[a]Referent = White tract.
[b]Referent = other region.
Tract disadvantage and immigrant concentration specified with random slopes: †$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

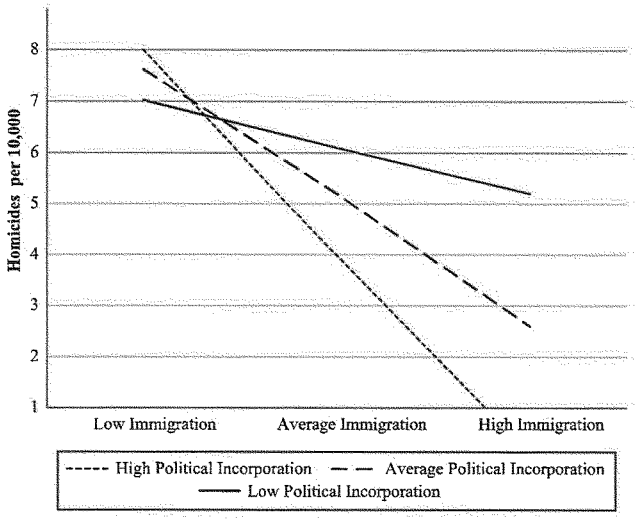

Figure 1. Predicted Values for Cross-Level Interaction between Minority Political Incorporation, Neighborhood Immigrant Concentration, and Homicide

refer to 1.5 standard deviations above or below the mean (see Table 1). Figure 1 shows that neighborhood homicide decreases as immigrant concentration increases and that this inverse relationship is most notable in cities with greater political incorporation of Asians and Latinos into municipal offices.

How do these patterns for homicide compare to those for robbery? Table 4 presents whether each of the five city-level political contexts moderates the relationship between tract-level instrumented immigrant concentration and robbery counts. The overall pattern of results for robbery is similar to that for homicide in terms of direction of product terms, although only two of the four significant interactions for homicide reach statistical significance for robbery. For robbery, neighborhoods benefit most from immigrant concentration in cities that have sanctuary laws/resolutions (Model 3) and that have a more receptive audience in the form of support for Gore (Model 4).

To illustrate one of these relationships, Figure 2 graphs the conditional association between immigrant concentration, sanctuary city policy, and robbery. High or low values for immigrant concentration refer to 1.5 standard deviations above or below the mean. Figure 2 shows that neighborhood robbery decreases as immigrant concentration increases and that this relationship is most precipitous in cities with formal policies that limit the enforcement of immigration laws (14 percent of our sample).

In contrast with homicide, neither political incorporation of Asians and Latinos into elected offices (Model 1) nor bureaucratic incorporation into the police force (Model 2) moderate the relationship between neighborhood immigration and robbery. The product terms are negative but not significant at traditional levels. As with the homicide models, mayor-council form of government does not interact significantly with immigrant concentration to influence robbery counts (Model 5). Although the nonsignificant moderating effect of mayor-council form of government across both violence outcomes is unexpected, we are not the first to find that mayor-council governments are not significantly more responsive to minority populations (e.g., Santoro 1995).

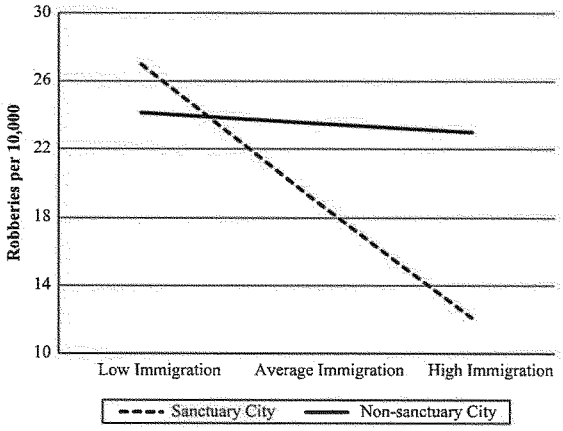

Figure 2. Predicted Values for Cross-Level Interaction between Sanctuary City Policies, Neighborhood Immigrant Concentration, and Robbery

Perhaps some mayor-council governments do indeed allow for immigrant influence, whereas others may be more attuned to an anti-immigrant constituency; the operation of mayor-council governments may thus be inconsistent.

In summary, despite some exceptions, we find overall statistical support that immigrant political opportunities enhance the inverse neighborhood-level association between immigrant concentration and violence, especially for homicide.

CONCLUSIONS

Urban research typically focuses on neighborhood outcomes within one or a small number of cities. As a result, researchers rarely adopt a multilevel comparative approach that appreciates the embedded ecology of neighborhoods within varied city contexts. We therefore know little about how larger structural forces at the city level worsen or ameliorate neighborhood dynamics related to the uneven distribution of social problems across neighborhoods. To address this gap, we examined how receptivity of city-level political actors and structures to immigrant concerns shapes the relationship between neighborhood immigrant concentration and violence. Educing insights from social movement and political science literatures, we proposed the concept of immigrant political opportunities to capture the extent that the local political setting is favorable to immigrant concerns. We hypothesized that the protective relationship between neighborhood immigration and violence does not apply to all neighborhoods equally but rather is contingent upon city-level immigrant political opportunities. Specifically, cities that are more receptive to immigrants' needs and demands should be best able to assist the revitalization faculty of immigrant concentration.

Our multilevel instrumental variable models support the immigrant revitalization hypothesis and a growing number of studies that point to the protective association between immigrant concentration and neighborhood violence. Rather than destabilizing communities and contributing to social disorganization, we found that tract-level immigrant concentration was inversely associated with neighborhood homicide and robbery, on average, across 8,931 Census tracts. Moreover, we found that this average relationship varies significantly across cities and that immigrant

political opportunities account for some of this variation. Specifically, cities with immigrant political opportunities enhance the protective association between neighborhood immigrant concentration and violence, particularly for homicide. For homicide, neighborhoods benefit more from immigration when cities have greater political incorporation of Asians and Latinos into city offices and bureaucratic incorporation of minorities into the police force. For both homicide and robbery, pro-immigrant legislation and Democratic electoral strength enhance the inverse relationship between immigration and violence.

To explain these conditional relationships, we proposed that favorable immigrant political opportunities reinforce social organization within immigrant neighborhoods by enhancing the trust immigrants place in civic processes and immigrants' capacity to exert public social control. That is, political opportunities can reduce immigrants' marginalization and social isolation and instead encourage their participation in civic life and collective organization around common goals, including safer neighborhoods. In these ways, political opportunities can facilitate the revitalization capacity of immigrant concentration for neighborhoods. In less receptive contexts, however, immigrants may face greater barriers to incorporation and likely greater odds of social and political marginalization. In fact, our results imply that relatively closed regimes may foster processes that disrupt social organization, including immigrant distrust of local government, detachment from neighborhoods, and social isolation.

All macro immigration-crime research confronts methodological challenges in isolating the aggregate effect of immigration on crime rates. Issues of selectivity in immigrant settlement patterns are difficult to dismiss. But the data we used and the analytic strategy we adopted offer considerable leverage. Peterson and Krivo's (2010a) pioneering work has made available data unparalleled in scope to assess how contexts across 87 large municipalities influenced the correlates of crime for nearly 9,000 neighborhoods. Our models took seriously issues of selectivity by controlling for prior immigrant settlement patterns at the tract level and foreign-born population size at the city level. Our results are also robust to attempts to account for endogeneity with 2SRI instrumental variable models. The most fervent skeptic may remain unconvinced that immigration ameliorates neighborhood criminogenic processes, but our research, in conjunction with a large body of supportive work, challenges nativist fears about the dangers of immigration for community viability. Rather, the vilification of immigrants strikes us as representing symbolic crusades driven by fear and anxiety about the changing racial fabric of our communities (Hagan, Levi, and Dinovitzer 2008; Hagan and Palloni 1999).

We offer four avenues for future research. First, like most macro quantitative research on immigration, we cannot directly observe with our data the mechanisms by which city-level political opportunities influence the neighborhood-level relationship between immigration and violence. A full test of intervening mechanisms would require (1) neighborhood-level indicators of social organization across various cities, including trust in and ties to civic leaders and institutions, mobilization, and civic engagement; and (2) sufficient variation across cities in key dimensions of immigrant political opportunities. To date, no data repository exists to examine these processes related to social organization for the large sample of cities and neighborhoods that we examine with the NNCS. We thus are unable to measure the dimensions of neighborhood social organization that we suggest emanate from favorable immigrant political opportunities at the city level. A comparative ethnographic approach (e.g., Abu-Lughod 1999; Hyra 2008) may present a fruitful design for illuminating how differences in immigrant political opportunities interact with community social organization in shaping crime. Future research, for instance, could compare dynamics related to immigrant concentration, social organization, and crime within a small

number of cities with substantively different immigrant political opportunities.

Second, immigrants have considerable heterogeneity in terms of language, home country, and racial background, which likely complicates the immigration-crime relationship. Some scholars suggest addressing this complexity by moving beyond immigrant concentration, a measure of homogeneity that treats all immigrants as a monolithic category, and examining neighborhood levels of language diversity (see, in particular, Graif and Sampson 2009). In addition, other scholars find that criminal justice contact among immigrants differs by country of origin (Nielson and Martinez 2011), perhaps due to different experiences with racialization in the United States (Provine and Doty 2011). Additional research along these lines would provide more nuance to our understanding of the immigration-violence link.

Third, we concentrate on political dimensions of city context. Research should continue to explore the moderating potential of additional cultural, social, and economic contexts of reception. Segmented assimilation theory, for example, emphasizes the role of ethnic enclaves in aiding immigrant economic mobility (Portes and Zhou 1993). Others note that immigrant destinations with long histories of immigrant settlement provide an array of cultural and social arrangements that facilitate immigrant incorporation (Waters and Jimenez 2005). These include the presence of legal-aid bureaus and other immigrant-serving organizations, bilingual and health services, job placement centers, and co-ethnic social organizations. Yet in new places of immigrant settlement, such arrangements are typically less well established (Waters and Jimenez 2005). Recent work provides suggestive evidence of the implications of immigrant settlement patterns for violence at the county (Shihadeh and Barranco 2010), city (Painter-Davis 2012), and tract levels (Ramey 2011; Vélez and Lyons 2012). Shihadeh and Barranco (2010), for example, found that linguistic isolation correlates with reductions in Latino homicide victimizations in traditional immigrant counties but not in new destination counties where immigrants face greater economic marginalization. Our approach suggests that cities with vibrant ethnic enclaves and preexisting immigrant institutional arrangements will condition the presence of social problems within immigrant-concentrated neighborhoods. Exploring these and similar questions acknowledges the multiple levels of urban ecologies that shape neighborhood outcomes. A multilevel analytic framework promises to expand our understanding of urban processes and the fate of marginalized neighborhoods in both scope and scale.

Fourth, although we expect the patterns we uncovered circa 2000 to hold for U.S. cities and neighborhoods in recent years, since 2000 the United States has experienced continued, yet shifting and uneven, patterns of immigration. As recent controversies in Arizona, Georgia, Alabama, and elsewhere suggest, state governments have responded in different ways to the evolving face of immigration. We encourage additional research to examine the implications for crime of changing immigrant concentrations and political contexts of reception. A second wave of the NNCS would enable scholarship to consider the unfolding and interrelated dynamics of political opportunity structures, immigration, and neighborhood violence.

Our theoretical claim that city-level opportunity structures condition neighborhood-level processes should extend beyond the case of immigration and crime. Opportunity structures likely moderate the relationship between other well-established correlates of neighborhood violence. For instance, decades of research has so often noted the strong correlation between minority composition and neighborhood violence that many take the association to be invariant across place. From a social disorganization framework, larger minority proportions may lead to greater rates of violence because of the concentration of economic disadvantages and resulting social isolation associated with minority neighborhoods. Yet, as we have shown, neighborhoods are embedded in city structures that partly

shape the well-being of marginalized communities. Cities with greater political, social, and economic opportunities for minorities may be able to counter the social isolation and disorganization faced by minority neighborhoods and therefore attenuate the relationship between racial composition and violence.

In conclusion, contrary to much public opinion and political rhetoric, our research joins a chorus of others in suggesting that immigration generally makes neighborhoods safer. However, the ability of immigration to reduce violence partly depends on the political climate of immigrant reception. Places with favorable immigrant political opportunities seem best able to support the protective association of immigration and crime, presumably because they facilitate revitalization processes. Our results imply that the punitive anti-immigrant policies enacted in many jurisdictions decrease the benefits of immigration for our communities. These exclusionary tendencies can engender mistrust and suspicion on the part of newcomers that erodes involvement in social and political life, attachment to and ownership of neighborhoods, and mobilization on behalf of neighborhood concerns (Aranda and Vaquera 2011). The ability of immigrants to revitalize their communities may depend on how attached immigrants feel toward receiving communities. By marginalizing newcomers, creating political cynicism, and instilling mistrust of the police and local authority, hostile regimes may set in motion the very processes they fear.

APPENDIX

Table A1. First-Stage Model of Immigrant Concentration, NNCS 2000

	Model 1		Model 2	
	b	SE	*b*	SE
Tract Level (*N* = 8,931)				
Disadvantage	.400***	.048	.381***	.048
Residential instability	.211**	.063	.201**	.060
Percent young males	.019**	.007	.019**	.007
Spatial lag homicide	.0003	.016		
Spatial lag robbery			.002**	.001
Latino tract	1.091**	.312	1.078**	.303
Black tract	−.679***	.137	−.731***	.156
Minority tract	.428**	.154	.380**	.164
Integrated tract	.244**	.068	.221**	.067
Immigration concentration 1990	.611***	.025	.607***	.025
City Level (*N* = 87)				
Disadvantage	−.222***	.065	−.216**	.064
Residential mobility	.019†	.010	.018†	.010
Percent Black	.002	.004	.001	.004
Percent Latino	.001	.006	.0003	.006
Percent foreign born	.014**	.006	.014**	.006
Percent young males	−.003	.016	−.003	.016
South	−.160†	.084	−.154†	.084
West	−.482***	.098	−.489***	.100
White/Black segregation	.001	.003	.0003	.003
Intercept	.163**	.077	.201**	.083
Variance Components (SD)				
Intercept	.301***		.298***	

Note: SE refers to robust standard errors; tract population 2000 as variable exposure.
†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table A2. Descriptives across Political Opportunity Regimes ($N = 87$)

	Latino/Asian Elected Officials				Police Minority Incorporation				Sanctuary City			
	Below Average		Above Average		Below Average		Above Average		No		Yes	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
City Level ($N = 87$)												
Disadvantage	−.04	.83	.19	1.10	−.13	.89	.17	.85	.02	.88	−.07	.85
Residential mobility	52.31	6.11	54.17	5.00	53.26	5.85	52.18	5.97	52.67	5.86	53.16	6.35
Percent Black	19.70	16.50	15.93	16.96	14.96*	13.58	22.99*	18.56	18.46	14.94	21.20	25.32
Percent Hispanic	16.90*	16.89	29.22*	20.67	18.51	16.12	19.60	21.06	18.70	19.10	21.16	15.32
Percent young males	16.00	2.33	16.15	2.78	15.77	2.42	16.25	2.43	15.82	2.38	17.14	2.49
Percent foreign born	14.91	11.65	17.58	14.66	15.26	9.80	15.80	14.75	15.16	12.74	17.76	9.84
South	.37	.49	.30	.47	.38	.49	.33	.48	.37	.48	.25	.45
West	.22	.42	.45	.51	.31	.47	.24	.43	.24*	.43	.50*	.52
White/Black segregation	49.49	18.01	41.07	17.86	40.98*	15.65	54.60*	18.31	45.87	17.85	58.04	17.72
Average tract homicide count	1.08	.83	1.08	.91	.88*	.65	1.29*	.98	1.27	1.98	1.96	3.20
Average tract robbery count	32.92	20.92	35.03	22.69	25.51*	16.84	41.86*	22.33	39.59	43.80	53.51	56.65
N	67		20		45		42		75		12	

	Percent Votes for Gore				Mayor-Council Government			
	Below Average		Above Average		No		Yes	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
City Level ($N = 87$)								
Disadvantage	.19	.89	−.13	.85	−.18*	.91	.26*	.77
Residential mobility	51.19*	6.49	53.94*	5.14	54.58*	5.54	50.37*	5.54
Percent Black	24.64*	19.18	14.33*	12.72	14.83*	13.92	24.00*	18.41
Percent Hispanic	17.46	16.85	20.26	19.88	21.03	18.15	16.46	19.02
Percent young males	15.97	2.32	16.03	2.52	16.13	2.58	15.84	2.23
Percent foreign born	15.32	10.20	15.68	13.92	16.95	11.43	13.68	13.41
South	.24*	.43	.45*	.50	.43	.50	.26	.45
West	.29	.46	.27	.45	.37	.49	.16	.37
White/Black segregation	51.87	17.84	44.20	17.98	40.81*	18.13	56.24*	14.42
Average tract homicide count	1.21	.95	.98	.75	.65*	.68	1.38*	.95
Average tract robbery count	37.45	20.40	30.26	21.52	26.89*	18.64	41.80*	21.63
N	40		47		49		38	

*Denotes a statistically significant difference based on t-tests for equality of means.

Acknowledgments

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Notes

1. We follow the vast majority of studies and spotlight the relationship between immigrant concentration and violence. However, the focus on concentration (or homogeneity) may differ from the concern about ethnic and cultural *heterogeneity* in seminal Chicago-school scholarship on immigration (Graif and Sampson 2009). Within the early social disorganization perspective, most Chicago-school scholars argued that greater proportions of foreign born contributed to neighborhood heterogeneity, which, in turn, diminished the capacity for social organization (Park and Burgess 1924; Shaw and McKay 1942; Thomas and Znaniecki [1918–1920] 1984; cf. Cohen 1931). Although we return to this issue of conceptualization and measurement in our conclusion, our goal in this article is to test the generalizability of the most common current operationalization of neighborhood immigration (concentration) vis-à-vis immigrant political opportunities.
2. Individual and contextual research finds parallel patterns for immigrant individuals. Immigrants have lower offending rates than their native-born counterparts, and living in communities with greater immigrant concentration reduces the odds of engaging in crime (Butcher and Piehl 1998; Desmond and Kubrin 2009; Sampson, Morenoff, and Raudenbush 2005). For a more in-depth review of the complexities of the immigration and crime relationship, including historical accounts, see Bursik (2006); Kubrin and Ishizawa (2012); and Martinez and Lee (2000).
3. Among other explanations, some scholars suggest that the process of immigration itself may select for individuals with relatively high levels of achievement ambition, low criminal propensity, or high aversion to risks associated with crime (Butcher and Piehl 1998; Kubrin and Ishizawa 2012; Tonry 1997). Places of immigrant concentration may thus reflect immigrants' lower aggregate criminal propensity, or immigrants may select into places with lower crime rates. Other scholars suggest that legal surveillance and formal social controls follow immigration flows and dampen local crime rates (Kubrin and Ishizawa 2012; Ousey and Kubrin 2009).
4. A small body of work finds that political *threats* rather than opportunities mobilize movements as well (Meyer 2004). Interestingly, and consistent with our core thesis, these arguments tend to be applied to middle-class movements like the anti-nuclear or pro-choice movements of the 1980s, rather than to movements mobilizing economically and racially marginalized constituencies, like Blacks in the 1950s or farm workers in the 1960s.
5. Recent events in East Haven, Connecticut suggest what might happen to immigrant communities when there is no minority incorporation into the police force. The Justice Department accused White patrol officers and high-ranking police officials in East Haven of embarking on a campaign to terrorize Latino residents. Among other actions, police officers allegedly detained Latinos, particularly immigrants, without reason, assaulted them while they were handcuffed, and arrested residents who attempted to document their behavior (Applebome 2012). This led to widespread fear and suspicion of police; immigrant residents did not believe the police department would enact or pursue immigrant-friendly policies.
6. Sandoval (2009) also provides evidence that political context can influence local economic revitalization. He documents that city authorities had scheduled to redevelop this once blighted area into a high-income neighborhood, a process that would have largely excluded the immigrant community. However, as Latinos became part of the dominant coalition of city government, local residents began to be included in the redevelopment plans, and the plans shifted to benefit existing residents economically. Sandoval (2009) argues that if Latinos were not part of the dominant coalition, such revitalization would have been hampered. Thus, the ability of immigrant concentration to catalyze economic revitalization may depend partly on the openness of political regimes.
7. This is the same number of cities analyzed by Peterson and Krivo (2010b).
8. In some cities, homicide and robbery counts are reported for only one or two of the years between 1999 and 2001. In these cases, the three-year count is an estimate derived from available data. According to Peterson and Krivo (2010a:8), "when two years of crime counts were provided, the estimate was calculated by multiplying the two year count by 1.5. When only a single year's crime count was available, the estimate was calculated by multiplying the single year count by 3."
9. All research using official data known to the police is subject to issues about reporting variations, hence our focus on homicide and (separately) robbery. However, we acknowledge the possibility that the political opportunities we measure may encourage residents in open cities to contact the police in the event of witnessing or experiencing crime, insofar as these contexts facilitate trust with important institutions, including the police. This logic may

lead us to expect positive main effects of political opportunities on violence or positive interactions between these city contexts and immigrant concentration. Although we cannot tease out this plausible mechanism with our data, the results we present are generally inconsistent with this argument.

10. Exploratory factor analysis indicated that these city-level contexts do not operate as a latent construct. Moreover, bivariate correlations among the five variables are weak to moderate. The highest correlation is between percentage voting Democrat in the 2000 general election and sanctuary city status (.396).
11. Although research on police minority representation suggests that Blacks can serve as allies for Latino and Asian constituents, we acknowledge the potential for inter-group hostility between Blacks, Asians, and Latinos (see Bobo and Hutchings 1996; Olzak 1993). The benefit of Black representation may or may not translate into other domains beyond policing, and thus we do not include Blacks in our measure of political incorporation.
12. Most cities in the NNCS pertain to a single county, and thus merging county- and city-level data are straightforward. Four cities in our sample, however, are located in counties that encompass multiple cities. For these cases, we adjusted the percentage Democratic votes by the proportion of the city population that resides in each county.
13. Four cities did not have either a mayor-council or a council-manager form of government. Results from using a three-category dummy variable to capture government form, however, did not meaningfully differ from those we report here.
14. Calculated by –.049 +/– 2 (.071) for homicide, and –.046 +/– 2 (.154) for robbery; see Snidjers and Bosker (1999).
15. We present findings from 2SRI multilevel models, although as noted in Tables 2 and 3, the stage-one residuals are not significant in models predicting homicide, providing little evidence of endogeneity. We see more evidence of endogeneity in models predicting robbery. Regardless of the outcome, however, we found similar patterns to those discussed here when treating immigrant concentration as an exogenous predictor (in noninstrumental variable models available upon request) for both homicide and robbery.

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Christopher J. Lyons is an Associate Professor of Sociology at the University of New Mexico. His work focuses on race/ethnicity and sociolegal control, and integrates insights from social disorganization, public social control, racial politics, and political economy perspectives to account for the spatial distribution of crime across neighborhoods.

María B. Vélez is an Assistant Professor of Sociology at the University of New Mexico. Her work focuses on understanding how racial and economic inequalities pattern urban crime at the individual, neighborhood, and city levels. She also seeks to understand how larger political and economic contexts shape neighborhood dynamics related to crime.

Wayne A. Santoro is an Assistant Professor of Sociology at the University of New Mexico. His work lies at the intersection of race, politics, and social movements. He examines how Blacks and Latinos have mobilized to compel governments to become responsive to community concerns as well as how these populations in turn have been affected by government actions. Other work investigates Mexican American political mobilization and the political dynamics that took place during the decline of the civil rights movement.

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Do sanctuary policies increase crime? Contrary evidence from a county-level investigation in the United States

Marta Ascherio

Department of Sociology, University of Texas at Austin, USA

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ABSTRACT

Immigration and Customs Enforcement (ICE) has rolled out a series of programs that leverage local and state resources to detain and deport undocumented immigrants. There is little understanding, however, about the public safety consequences of mobilizing local police to enforce immigration law. I use ICE administrative records, Uniform Crime Reports, and American Community Survey population estimates to investigate whether and under what circumstances local immigration enforcement is associated with property crime and violent crime. Results show that crime trends in sanctuary and non-sanctuary counties were not significantly different in the first decade of the 2000s. However, after the proliferation of sanctuary practices around 2014, both property crime and violent crime decreased more in sanctuary counties than non-sanctuary counties, net of other predictors of crime. Further, a pooled cross-sectional analysis of 2013–2016 data shows that sanctuary practices strengthen the inverse relationship between proportion foreign-born Latino and property crime, and reverse the positive relationship between proportion native-born Latino and property crime. I theorize that this occurs because sanctuary practices encourage immigrant political integration, have positive spillover effects to non-immigrant Latinx communities, and increase *social harmony*.

1. Introduction

Policy makers and researchers in the United States have increasingly been concerned with the entanglement of immigration control with criminal-legal systems (Gupta and Altman 2021; Kohli et al. 2011; Miles and Cox 2014; Stumpf 2006; Treyger et al. 2014). This became a more pressing issue in 2009, when Immigration and Custome Enforcement (ICE) launched the Secure Communities, a program that linked FBI databases with Department of Homeland Security databases to automatically check the citizenship status of anyone arrested, anywhere in the country (Cox and Miles 2013). By January 2013, ICE had activated such data-sharing technology in all state and local jails in the country, allowing ICE to track potential non-citizens, and request law enforcement agencies to detain potential non-citizens for questioning, or be notified upon their release (ICE, 2018:201; Meissner et al., 2013). These are called detainer requests and notification requests, respectively.

By 2014, however, over 200 jurisdictions had withdrawn from participating in Secure Communities (ICE 2015; Ramos 2014). In practice, this meant declining detainer and notification requests (Hausman 2020). Jurisdictions that declined to participate cited concerns that Secure Communities would penalize immigrant victims or witnesses of crimes and thus undermine community-police relationships (Chen 2016). Other concerns included morality, legality, and legitimacy of the program (Chen 2016). Declining notifications and detainers is one important way in which local government restricts police officers enforcement of immigration laws.

E-mail address: Marta.ascherio@utexas.edu.

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Other types of initiatives include barring investigations into immigration violations, limiting ICE's access to jails, and limiting disclosure of information (Lasch et al., 2018).

Although Secure Communities was designed to improve public safety, there is no evidence that crime has decreased as a result (Miles and Cox 2014; Treyger et al. 2014). Emerging work on the consequences of immigration enforcement has found that immigrant-friendly legislation, referred to colloquially as sanctuary policies, has either a null or negative association with violent crime and property crime (Amuedo-Dorantes and Deza 2019; Gonzalez-O'Brien et al., 2019; Hausman 2020; Kubrin and Bartos 2020; Lyons et al. 2013; Martínez-Schuldt and Martínez 2019, 2019; Wong 2017). This relationship holds despite researchers operationalizing "sanctuary" in different ways, examining varying units of analysis, at different points in time, and using different analytical techniques (Martínez et al. 2018). Although these studies point to the potential for sanctuary policies to decrease crime, a broader assessment that includes smaller jurisdictions is missing. The current study fills this gap and adds an analysis of whether and to what extent the proportion of Latinos moderates sanctuary policies effect on crime, and an improved measure of sanctuary *practices*, which is a better indicator of actual rather than intended enforcement.

I assembled an original dataset using administrative records and survey data to investigate whether and under what circumstances non-compliance with ICE, which I will refer to as sanctuary practices, is associated with aggregate property crime and violent crime. I also test whether the proportion of foreign-born Latinos and the proportion of native-born Latinos moderate sanctuary practices effect on crime. This is important because foreign-born Latinos are the population most targeted by immigration enforcement, and because of the mounting evidence that the consequences of immigration enforcement spillover to native-born Latinos (Asad 2020; Barranco and Shihadeh 2015; Elizabeth Aranda, Cecilia Menjívar, and Katharine M. Donato 2014; Martinez-Aranda 2020; Menjívar et al. 2018).

Through a series of negative binomial regression models, I find that crime trends in sanctuary and non-sanctuary counties were not significantly different in the first decade of the 2000s. However, using a conditional fixed effects negative binomial regression model, I find that sanctuary practices are associated with a decrease in both property crime and violent crime within counties over time, findings that are in the same direction, but slightly weaker, when using an unconditional fixed effects model. Further, a pooled cross-sectional analysis of 2013–2016 data shows that sanctuary counties strengthen the inverse relationship between proportion of foreign-born Latino and property crime, and reverse the positive relationship between proportion native-born Latino and property crime. Building on prior research, I theorize that this occurs via three principle mechanisms: immigrant political integration, positive spillover effects of sanctuary practices, and increased *social harmony*. First, immigrant political integration efforts, like expanding immigrants' rights through sanctuary practices, are likely to be adopted at the tail end of immigrant integration and incorporation more broadly (Marrow 2009) and in areas where sheriffs have more positive attitudes towards immigrants (Farris and Holman, 2017). Immigrant-friendly policies and practices encourage more full participation in social, political, and economic life, which increases social organization and is protective against crime (Kubrin and Weitzer 2003; Lyons et al., 2013; Martínez-Schuldt and Martínez 2019; Sampson and Byron Groves, 1989). Second, there is abundant evidence that because of the racialization of illegality (García, 2017; Menjívar et al., 2018) restrictive immigration policies "spill over" to negatively impact native-born Latinos. Thus, efforts to bolster social and political incorporation of immigrants might spill over in positive ways to impact native-born people and communities that have been racialized as illegal despite holding U.S. citizenship.

Third, *social disharmony* is a Du Boisian analytical concept recently re-articulated by Werth (2022) that provides a generative lens through which to make sense of my findings. Social disharmony was developed by W.E.B. Du Bois, and, complements social disorganization theories by focusing on racial and economic exclusion, paired with racialized systems of oppression as causes of crime, rather than eroded mechanisms of informal social control prioritized by social disorganization theorists (Werth 2022). Thus, in addition to bolstering social organization, I theorize that sanctuary practices may help decrease crime by beginning to dismantle racialized systems that marginalize immigrants and people of color.

2. Literature review

2.1. Secure Communities and spillover effects

The Secure Communities Program is the most extensive federal-local collaboration to detain and deport undocumented immigrants in US history. The federal government had made prior efforts to mobilize state and local law enforcement agencies to enforce immigration issues, but the scale and scope of the current effort at interior immigration enforcement is unprecedented. Most importantly, once activated, the data sharing component of Secure Communities does not require the buy-in or additional labor from local police chiefs, officers, or sheriffs: all arrestees are finger printed, and all finger prints are shared in inter-operable databases that include 1) Automated Biometric Identification System (IDENT) which is the Department of Homeland Security database that tracks immigration history and 2) Integrated Automated Fingerprint Identification System (IAFIS), which is the FBI database for criminal arrests and convictions (ICE 2011).

Although Secure Communities was designed to increased community safety, there is no evidence that Secure Communities Program is uniformly good for public safety or community well-being (Miles and Cox 2014; Stowell et al., 2013). In fact, there is evidence that enhancing local-federal coordination for immigration enforcement negatively impacts mental and physical health of Latino immigrants (Wang and Kaushal, 2019), decreases enrollment in federal public assistance among Latino citizens due to increased fear of deportation (Alsan and Yang, 2019), marginalizes and racializes Latino immigrants (Jones 2019), and potentially exacerbates toxic ties in mixed-status families (Del Real and Deisy, 2019). Further, complying with Secure Communities takes police officers a significant amount of time, time that could otherwise be spent responding to calls for service. While Secure Communities is most likely to impact undocumented immigrants and undocumented immigrant communities because of their precarious positions and lack of the

protections afforded with citizenship, even native-born Latinos or Latinos with other legal status are impacted by immigration laws (Asad, 2020a; 2020b). This has been referred to as the "spillover effects" of restrictive immigration policies and practices. For example, Phoenix, Arizona has such punitive and highly publicized anti-immigrant laws that when surveyed about likelihood of contacting the police in case of need, even U.S.-born Latinos express reluctance (Menjívar et al. 2018). Further, although foreign-born Latinos are more concerned about deportation than native-born Latinos, about half of all Latinos say that they worry about deportation for themselves or their friends and family (Hugo Lopez, Morin, and Taylor, 2010). This is in part because many U.S.-born and legally authorized Latinos are part of mixed-status households or are embedded in community and family networks that include people who are undocumented or have precarious documentation (Menjívar 2006).

By 2014, over 200 jurisdictions had withdrawn, at least partly, from participating in Secure Communities (Ramos 2014), and ICE started keeping track of these withdrawals (ICE 2015:201). In the current study, however, only 125 counties are counted as sanctuary counties, not 200. This is because while some jurisdictions decline detainer requests, many would still notify ICE upon releasing detainees. In the end these arrestees are still transferred from local jail to ICE custody, and local police still take time to address these requests, thus it does not qualify as a sanctuary practice.

Between November 2014 and January 2017, Secure Communities was temporarily replaced by the Priority Enforcement Program (PEP), which operated using the same technology and same mechanisms, but purportedly targeted only people deemed to pose a serious threat to public safety (ICE 2017b; Jácome 2021). However, PEP "has the same policy objectives and relies on a similar level of local cooperation to achieve its purpose" (Jaeger 2016), and there is evidence that under PEP people without criminal records continued to be detained (TRAC 2016). In both programs, if fingerprints match those in the DHS system, ICE's Law Enforcement Support Center (LESC) is notified, and the local ICE agency may request a "detainer hold" which is a request for the jail to hold the person for up to 48 hours. Alternatively, they may request a "notification", which is a request to notify ICE prior the person's release (ICE, 2018; Meissner et al., 2013). That being said, there is evidence that under PEP, the reporting violent and property crimes to police increased by 4% among Latinos (Jácome, 2021), which is one of several reasons that any decreases in crime identified in the current study are likely conservative estimates.

2.2. *Sanctuary policies and crime*

Existing research has found that cities and counties with at least one sanctuary policy have either a null or negative association with crime (Gonzalez-O'Brien et al., 2019; Hausman 2020 ; Lyons et al., 2013; Martínez-Schuldt and Martínez 2019, 2019; Wong 2017), and that state-wide sanctuary policies are also unrelated to crime (Kubrin and Bartos, 2020). Further, evidence from 1999 to 2001 cross-sectional data shows that sanctuary policies strengthen the inverse association between tract-level immigrant concentration with homicide and robbery (Lyons et al., 2013). Subsequent research examining trends over time shows that between 1990 and 2010, each percent increase in the undocumented Mexican population resulted in a 5% decrease in the homicide rate, but only in sanctuary cities (Martínez-Schuldt and Martínez 2019). There is evidence from more recent crime data that sanctuary policies resulted in a decrease of domestic homicide rates among Hispanic women from 2003 to 2017 (Amuedo-Dorantes and Deza 2019). Finally, using data from 2010 to 2015, Hausman (2020) finds that sanctuary policies decrease overall deportations by one-third, and decrease non-criminal deportations by about half, but do not have a significant effect on crime. Unlike previous studies, Hausman (2020) defines sanctuary counties as those that decline immigrant detainers, rather counties that passed one sanctuary legislation. However, some those counties that decline detainer requests still will notify ICE upon releasing a detainee and thus may not be protective.

In the current study, I build on and contribute to existing research in three principle ways. First, by using a dataset that includes 3,105 counties, nearly all counties in the United States, I build on work done by Lyons et al. (2013), Gonzalez-O'Brien et al. (2019), and Martínez-Schuldt and Martínez (2019), who focus on 87 large cities, 55 cities, and 107 cities, respectively. While existing work sheds light on sanctuary policies effect on crime in large cities, I contribute by using a larger and more comprehensive data set that includes less densely populated areas, and have often been excluded from criminological research. Hausman (2020) also uses county-level data, but limits the study to the largest 10% of counties by Hispanic population, thus, a broader assessment is still missing.

Second, I use a more restrictive measure of sanctuary policies, which are counties that refuse detainer requests *and* notification requests. This improves on previous work that designates as "sanctuary" a locality that has passed any type of resolution limiting local cooperation with ICE (Amuedo-Dorantes and Deza 2019; Gonzalez-O'Brien et al., 2019; Lyons et al., 2013; Martínez-Schuldt and Martínez 2019), which risks being too varied in scope and formality to be a meaningful measure. I expect that a stricter operationalization of sanctuary practices will yield a negative relation between sanctuary and crime.

Finally, by testing whether proportion Latino moderates sanctuary policies effect on crime, I estimate whether and to what extent local immigration policies affect both foreign-born and native-born Latino communities. This is important in light of mounting evidence that immigration enforcement has spillover effects to impact non-immigrants in the United States.

2.3. *Operationalizing sanctuary policies*

Sanctuary policies are local efforts to secure rights for undocumented immigrants, or to restrict the extent to which police officers enforce immigration laws. Sanctuary policies can be providing identification cards without proof of citizenship, imposing limits to when a police officer can inquire about immigration status, or refusing to hold non-convicted prisoners for ICE.

Previous studies have focused on city-level measures of sanctuary and have designated any city with at least one immigrant-friendly policy as a sanctuary city (Amuedo-Dorantes and Deza 2019; Gonzalez-O'Brien et al., 2019; Lyons et al., 2013; Martínez-Schuldt and Martínez 2019). *Sanctuary* is a very loose term, however. These policies vary in content and effort, and immigration agents still arrest

and detain people in supposedly sanctuary cities. In addition, there are discrepancies between statements made by politicians regarding sanctuary policies, and the actual practices carried out by police departments. Former Houston Chief of Police Art Acevedo, and Houston Mayor Sylvester Turner, have both made public statements declaring Houston a safe and inclusive place for immigrants (Fernandez 2018). However, in practice, Houston Police Department continues to hold suspected undocumented immigrants past their legal release time (Díaz 2017). This points to the discrepancy between policy and practice, and while policies have important symbolic effects, practice has additional consequences. The reverse is also true: some smaller jurisdictions such as Storm Lake, Iowa, have sanctuary *practices*, but don't publicize their efforts, in order to avoid scrutiny from ICE (Davis 2020; Hoffman 2016).

2.4. Immigrant political incorporation

More expansive rights for immigrants are closely associated with safer communities. For example, traditional immigrant destinations have better and more inclusive infrastructure for immigrants, and have about half the violent crime rate of new immigrant destinations (Ramey 2013). Traditional immigrant destinations are better able to accommodate needs of both established and newly arrived immigrants because they have established systems that help find employment, provide bilingual services, and offer support in navigating educational and political institutions (Ramey 2013; Waters and Jiménez 2005).

Pro-immigrant policies and practices at the county level are an indicator that other local bureaucratic agencies are immigrant-friendly as well. This is because street-level bureaucrats are much faster to respond to needs of new immigrant communities than their colleagues in elected office (Marrow 2009). Public educators and emergency medical personnel are most responsive to the needs of new immigrants, followed by lawyers and law enforcement agents, all of which precede political responses (Lewis and Karthick Ramakrishnan, 2007; Marrow 2009). This means that sanctuary practices are likely adopted at the tail end of broader immigrant integration and incorporation efforts, in areas where immigrants have stronger ties to local institutions. In fact, strong community-institution relationships are key factors in mitigating crime: local police can help protect people from crime, banks can provide loans to start businesses or buy a home, and local politicians that can secure resources for the community (Klinenberg 2004; Vélez 2006).

Although previous studies use social disorganization theories to explain why immigrant concentration, immigrant-friendly policies, and immigrant political opportunities are sometimes protective against crime, I posit that Du Bois's criminological theories of *social disharmony and racial injustice* (Werth 2022) offer additional layers of insight. *Social disharmony*, as principle cause of crime understands crime as a "symptom of wrong social conditions more broadly and from the oppression of Black Americans" (Werth 2022:7). Du Bois was writing about Black Americans, the racial hierarchy born from the legacy of slavery, and the subsequent exclusion of Black Americans from broader social, political, and economic institutions. That being said, the criminalization and incarceration of Latinos in the contemporary United States can also be understood in the context of broader structural histories of oppression and exclusion, for instance, as a result of legacies of colonialism and subsequent migrant labor agreements between the U.S. and Mexico such as the Bracero program (Calavita 1992).

In summary, existing research points to how sanctuary policies and immigrant political representation have either a null or negative association with crime. By using a larger and more comprehensive dataset of U.S. counties, and by using a more restrictive measure, I expect that results will support findings that sanctuary practices have a negative (rather than null) association with crime, an association that extends beyond the large cities studied in previous work. Further, informed by the literature of the spillover effects of immigration enforcement, I expect that sanctuary policies will be more protective against crime in counties that have higher proportions of foreign-born Latinos and native-born Latinos.

The county-level analysis in this paper will complement the existing neighborhood, city, and state-level studies, particularly because county sheriffs and county leadership are the driving force behind policies regarding local police involvement with immigration enforcement (Graber and Marquez 2016). Further, county-level politics shape the distribution of jobs, services, and transportation in a way that is consequential for community wellbeing, but have been somewhat neglected in criminological research because of an "urban bias" (Lee et al. 2003). County-level crime studies might also be sparse because Maltz and Joseph (2002) critiqued the monthly county-level UCR data due to gaps and inconsistent imputation schemes. However, yearly aggregate crime data are more reliable; the UCR has different imputation procedures depending on how much data is missing. For instance, for agencies that do not submit all 12 months of data, the UCR imputes the missing data with the average crime rate of the months submitted (Lynch and Jarvis 2008). When agencies only submit two months or less of data, the UCR replaces the rest with numbers of a similar agency. These agencies represent about 10% of the population (Lynch and Jarvis 2008). Thus, using annual county crime data from the UCR is both appropriate and potentially sheds light on the political structures that increase or decrease safety.

3. Data and methods

3.1. Measures

In order to investigate whether and to what extent sanctuary practice are associated with crime, I assembled an original dataset drawing on multiple sources at the county-level. The main dataset includes 3,105 counties for a total of 12,420 county-year observations from 2013 to 2016, which are over 98% of all 3,143 counties and county-equivalents in the United States. Counties in Alaska, Hawaii, and outlying islands are not included. This period begins in 2013 when the Secure Communities Program was fully implemented, and ends in 2016 before the new presidential administration brought other changes to the national political landscape. I include additional data for 2000 and 2012 in order to estimate pre-policy differences in crime trends. Table 1 shows the source of each

variable, which are described below.

The outcome variables for this study are annual crime counts from the FBI Uniform Crime Reports (UCR) aggregated to the county level. The violent crime index includes robbery, aggravated assault, and homicide. The property crime index includes burglary, motor vehicle theft, and larceny-theft (Kaplan 2017). Rape is excluded from the study because of significant undercounting (Yung 2013). Although the UCR data include only crimes known to the police, and thus are an imperfect source of victimization, it remains the best national source of data on reported crime. Kaplan (2017) compiled a UCR dataset with identifying geographic information to facilitate merging with data from other sources.

The main independent variable, which is sanctuary status, comes from administrative records collected and used by ICE to track compliance with detainer and notification requests. The database was obtained via FOIA request by the Immigrant Legal Resource Center (ILRC). It includes the jurisdiction, county, current detainer/notification acceptance status, and month/year the jurisdiction began declining detainers. I checked the ICE data against three other sources for validity: 1) declined detainers list published by Texas Tribune 2) TRAC Syracuse immigration data tool on detainers and 3) Center for Immigrations Studies update on sanctuary jurisdictions (ICE 2017a; TRAC 2020; Vaughn 2016). Two counties that could not be validated were dropped from the dataset: Jasper County, Illinois, and Alpine County, California. Some of the counties that I designated as sanctuary counties did accept a portion of the detainers requested. However, there are few or no jurisdictions that do not coordinate with ICE under any circumstances, thus, for the purposes of this study, two sources confirming non-coordination with ICE were sufficient. By this measure, about 4% of counties were sanctuary counties by 2016. Table 2 shows the total number of sanctuary counties by year. The administrative records I use include the "date stopped accepting detainers", which means that according to this data, the counties that stopped accepting notifications and detainers did not revert, at least not during the time of the study. By 2016, there are 123 sanctuary counties, 2,511 non sanctuaries, and the remaining 471 counties accept notifications but not detainers. Although these "moderate" sanctuary counties refuse to hold detainees for extra time, they will notify ICE upon the person's release, which results in detainees being transferred to ICE custody. Thus, because notifications are not a protective practice, I created a binary measure of sanctuary practices: does not accept notifications or detainers = 1, rest = 0.

Using the American Community Survey (ACS) 5-year population estimates, I include several county characteristics that prior research has linked to aggregate crime. The ACS data include 5-year averages of the following years: 2008–2012, 2009–2013, 2010–2014, 2011–2015, and 2012–2016. For simplicity, references to these data in the remainder of the manuscript state only the final year of the five-year average (e.g. 2012 refers to the 5-year average from 2008 to 2012). These include the natural log of the total population and the natural log of population density. Residential instability is measured as the percent of the population the moved since the previous year (Boggess and Hipp 2010; Martinez 2000; Sampson et al. 1997). Inequality is measured using the county-level Gini coefficient of income inequality (Pratt and Cullen 2005; Wenger 2019). The 2014 Gini coefficient is used to impute the 2013 values, since it was not included in the 2013 American Community Survey. I replicate Peterson and Krivo's (2000) disadvantage index, which is calculated from average z-scores of six items: The percent of secondary-sector low-wage jobs, jobless rate for the portion of the population aged 16–64, percent professionals and managers (reverse-coded), percent female-headed households, percent over 25 who have graduated from high school (reverse-coded), and percent of population living below poverty. I include percent non-Hispanic Black, percent non-Hispanic Asian, and percent native-born Latino because although "race" is not meaningfully linked to crime, racial residential segregation in the United States has resulted in heightened vulnerability in Black and Latino communities when compared to White communities (Hernandez et al. 2016; Krivo and Peterson 2000; Martínez, Stowell, and Lee, 2010; Phillips 2002). Due to some evidence that higher concentration of foreign born residents is associated with less crime, I also include the percent of the population that is foreign-born Latino (Ousey and Kubrin 2018).

Finally, a higher proportion of votes for democratic candidates may represent political opportunities for immigrants, and act as a

Table 1
Source of variables.

Variables	Description	Date	Source
Part I Index Crimes	County level violent and property crime counts and rates per population	2000–2016	Uniform Crime Reports [Jack Kaplan, ICPSR 37061]
Sanctuary County	Yes Sanctuary, 1: Decline notifications & detainers Not Sanctuary, 0: Accepts one or both	2013–2016	Immigration and Customs Enforcement [Immigrant Legal Resource Center]
% Votes for Democratic Candidate	% of total county votes that went to democratic presidential candidate	2012, 2016	2012 election data obtained from The Guardian, and 2016 data scraped from Townhall.com. [Compiled by Mcgovern (2017)].
Demographic covariates from Census (2000), and American Community Survey 5-year estimates (2012–2016)			
Population (ln)	Natural log of total population		
Logged population Density	Natural log of population density per square mile		
Gini Coefficient	Gini coefficient of income inequality. 0 = perfectly equal 1 = perfectly unequal (only in ACS)		
Disadvantage Index	Average z-scores of: % lowest wage jobs, % unemployed, professionals and managers (reverse-coded), % female-headed households, % graduated from high school (reverse coded), % below poverty		
% Moved	% of total population that moved since last year (only in ACS)		
% Racial/ethnic makeup	% of total population that is non-Hispanic white, non-Hispanic black, non-Hispanic Asian, and native-born Latino		
% Foreign born Latino	% of total population that is foreign born Latino		

Table 2
Number of counties that decline detainers and notifications by year.

	2013	2014	2015	2016
Sanctuary = 1 Yes Sanctuary: decline notifications & detainers	2	104	122	123
Sanctuary = 0 Moderate Sanctuary, accept notifications but not detainers	3	412	469	471
Sanctuary = 0 Not Sanctuary: accepts both notifications & detainers	3100	2589	2514	2511
Total	3105	3105	3105	3105

Note: dates for sanctuary level = 1 is estimated from "date of first or last meeting between Enforcement and Removal Operations (ERO) and local government). Because counties understood participation in Secure Communities as mandatory, the majority complied until it became clear that participation was optional. Thus, starting in 2014, many counties started opting out.

measure of political liberalness and pro-immigrant support (Lyons et al., 2013; Steil and Vasi, 2014). Therefore, following previous research, I include percent votes for a democratic candidate in the 2012 presidential election and the 2016 presidential election to account for political orientation and other cultural aspects of society that are linked to, but separate from, structural and demographic characteristics. I retrieved county-level voting data from *The Guardian Datablog* for the 2012 presidential election, and from Townhall.com for the 2016 presidential election, that was compiled and made available by Mcgovern (2017).

3.2. Analytical strategy

The main empirical question of this paper is: when and under what circumstances are immigrant-friendly practices associated with crime? Using this question as a starting point, the analysis that follows seeks to accomplish four main objectives: A) estimate the association between county-level sanctuary practices and county-level crime at a cross-section, B) test the effect of potential moderators such as population foreign-born Latino and population native-born Latino, C) understand whether and how crime changed within counties after sanctuary policy adoption, and D) establish whether prior crime trends can explain the differences or lack of differences in counties that would eventually adopt sanctuary practices compared to those that would not. In what follows, I outline the analytical strategy used to accomplish each research objective.

Objective A: Estimate the association between sanctuary policies and crime at a cross-section.

To estimate the association between sanctuary policies and crime at a cross-section, I first use a negative binomial regression model on pooled count data, which is a Poisson-based model that accounts for overdispersion (Gardner et al. 1995; Hilbe 2011; Osgood 2000). This is important because although violent crime and property crime are of significant social concern, they are relatively rare events. Aggregate crime rates will be much more variable in areas with a lower population, for instance, in a small county with 60,000 people, an increase in five crimes in one year will increase the crime rate by 8 per 100,000. Comparatively, in Los Angeles County, a county with 10 million people, an increase in five crimes in one year corresponds to a crime rate of 0.05 per 100,000 people. This means that estimated crime rates are much more precise in large counties than small counties, and that the standard error will be larger in small counties than in large counties. This violates an assumption of ordinary least squares regression, which is homogeneity of error variance (Osgood 2000). I address this issue by estimating crime counts, instead of rates, and by using a negative binomial regression model, a poisson-based model that is based on the assumption that underlying data are nonnegative integer counts of events, accounts for overdispersion, and is commonly used to model counts (Gardner et al., 1995; Hilbe 2011; Osgood 2000).

Model 1 includes all 12,420 county-year observations and estimates whether, on average, there are differences in crime in sanctuary vs. non-sanctuary counties. I include population size as the exposure variable, the coefficients therefore could be interpreted as differences in crime per capita. The equation for model 1 is as follows:

$$\text{Model 1:} \quad ln(C_i) = ln(n_i) + \beta_1 S_i + \beta_2 X_i + \varepsilon_{i,t}$$

Where C_i is the crime count in county i, n_i is the population at risk, in this case, total population of county i, S_i is whether county will eventually adopt a sanctuary practice (1 = yes, 0 = no). X_i are covariates: population size, population density, disadvantage, residential instability, racial/ethnic make-up, percent foreign-born Latino, and political liberalism.

Objective B: Test the effect of potential county-level moderators such as proportion foreign-born Latino and proportion native-born Latino.

In models 2 and 3 I include an interaction between eventual adoption of sanctuary policy with percent foreign-born Latino and percent native-born Latino to estimate whether these county characteristics moderate sanctuary policies effect on crime. The equation for these models are as follows:

$$\text{Model 2:} \quad ln(C_i) = ln(n_i) + \beta_1 S_i{}^* \ \beta_2 F_i + \beta_3 X_i + \varepsilon_{i,t}$$

$$\text{Model 3:} \quad ln(C_i) = ln(n_i) + \beta_1 S_i{}^* \ \beta_2 N_i + \beta_3 X_i + \varepsilon_{i,t}$$

Where C_i is the crime count in county i, n_i is the population at risk, in this case, total population of county i, S_i is whether the county

will eventually adopt a sanctuary practice (1 = yes, 0 = no), F_i and N_i are percent foreign-born Latino and percent native-born Latino, respectively. X_i are covariates: population size, population density, disadvantage, residential instability, racial/ethnic makeup, and political liberalism.

Objective C: Understand whether and how crime changed within counties after sanctuary policy adoption.

Model 4 is an conditional fixed effects negative binomial regression to estimate whether crime changes *within counties* in the aftermath of sanctuary practices adopted between 2013 and 2016. The advantage of using panel data is to be able to control for all stable covariates without having to include them in the model. Therefore, with the fixed effects specification, only changes within counties are used to estimate the outcome variable. Previous studies that use fixed effects negative binomial regression models to study aggregate crime include Martínez-Schuldt and Martínez (2019), Hausman (2020), and Payton et al. (2015). As a model sensitivity test, I include model 5, an unconditional fixed effects model in which dummy variables for each year and each county are included. Because Stata cannot process dummy variables for each of the 3105 counties, in this additional model I limit the analysis only to large counties that have a population one standard deviation above the mean, this results in 481 counties, and 1924 county-years.

$$\text{Model 4: } \ln(C_{i,t}) = ln(n_{i,t}) + \beta_0 + \beta_1 S_{i,t} + \beta_2 X_{i,t} + \alpha_{1i} + \varepsilon_{i,t}$$

$$\text{Model 5: } \ln(C_{i,t}) = ln(n_{i,t}) + \beta_0 + \beta_1 S_{i,t} + \beta_2 X_{i,t} + \alpha_{1i} + \alpha_{2i} + \varepsilon_{i,t}$$

Where $C_{i,t}$ is crime count per capita in county i in year t, $n_{i,t}$ is the population of county i in year t, $S_{i,t}$ is whether that county i has a sanctuary policy by year t (1 = yes, 0 = no). $X_{i,t}$ are covariates, α_{1i} is a year fixed effect which are included as dummy variables for each year, with 2013 as the reference year, and α_{2i} is a county fixed effect which are included as dummy variables for each county that has a population one standard deviation above the mean. I cluster the standard errors for the repeated observations per county. The fixed effects specified in this model capture other relatively constant and unobserved characteristics that may affect official crime rates such as region of the country, or racial segregation. This reduces bias from differences across counties because only within-county information is used to estimate the statistical influence of independent variables such as sanctuary status or sociodemographic controls on the property crime or violent crime index. The year fixed effects control for any macro-level events that might impact crime across the country, such as overall perception of economic hardship, which has an independent effect on crime from more "objective" measures of hardship (Rosenfeld and Fornango 2007).

Objective D: Establish whether prior crime trends can explain the differences or lack of differences in crime trends in counties that would eventually adopt sanctuary practices compared to those that would not.

Finally, model 6 estimates the change in crime rates from 2000 to 2012, and model 7 estimates the change in crime rates from 2013 to 2016, in counties that would eventually adopt sanctuary practices and those that would not. Here I estimate rates, and not counts.

$$\text{Model 6: } \Delta P_i = \beta_0 + \beta_1 \Delta S_i + \beta_2 \Delta X_i + \varepsilon_{i,t}$$

$$\text{Model 7: } \Delta R_i = \beta_0 + \beta_1 \Delta S_i + \beta_2 \Delta Y_i + \varepsilon_{i,t}$$

Where ΔP_i is the change in crime rate in county i between 2000 and 2012, ΔS_i is whether county will eventually adopt a sanctuary practice (1 = yes, 0 = no), and ΔX_i are change in sociodemographic indicators between 2000 and 2012. ΔR_i is the change in crime rate in county i between 2013 and 2016, and ΔY_i are change in socio demographic indicators between 2013 and 2016.

Table 3
Descriptive statistics of US Counties in 2016, by Sanctuary Status.

	Non-sanctuary N = 2511		Moderate Sanctuary N = 471		Sanctuary Counties N = 123	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Population	84,648	302,000	133,000	296,000	330,000	657,000
Population Density (log)	3.85	1.62	3.4	2.02	4.81	2.30
Gini Coefficient of Inequality	.44	3.52	.44	3.46	.45	3.28
Disadvantage Index	-.024	.67	-.33	.68	-.146	.70
Residential Instability	7.56	3.45	7.85	3.61	9.47	3.16
Percent Non-Hispanic White	77.56	19.57	78.23	19.96	71.12	19.73
Percent Black	10.13	15.45	3.6	6.7	6.17	11.23
Percent Asian	.98	1.66	1.61	2.81	3.93	5.64
Percent Native-Born Latino	5.7	10.1	8.4	12.7	9.3	8.4
Percent Foreign-Born Latino	2.58	4.28	2.85	4.11	5.25	5.31
Percent Votes Obama (2012)	37.08	14.25	42.80	14.78	49.20	16.55
Percent Votes Clinton (2016)	30.41	14.61	34.68	15.83	45.30	18.53
Crime Rate per 100,000						
Violent Crime	210	202	214	341	276	308
Property Crime	1630	1110	1579	1084	2126	1176

4. Results

Evidence from this study shows that crime trends in sanctuary and non-sanctuary counties were not significantly different in the first decade of the 2000s. However, an analysis of trends over time shows that sanctuary practices, in the form of declined notification and detainer requests, lead to a post-hoc decline in crime rates. Further, a pooled cross-sectional analysis of 2013–2016 data shows that sanctuary counties strengthen the inverse association between proportion foreign-born Latino and property crime, and reverse the positive association between proportion native-born Latino and property crime. On the other hand, sensitivity tests show that moderate sanctuary counties do not have the same protective effect as full sanctuary counties. This was expected because detainees will still be transferred to ICE, thus does not qualify as a protective practice, and people flagged as undocumented will still end up entangled in the detention and deportation system. In what follows, these results are each discussed in more detail.

Table 3 shows descriptive statistics for all counties in 2016. On average, sanctuary counties in 2016 had higher rates of property crime and violent crime than non-sanctuary counties. This is consistent with prior MSA-level findings that in both 2000 and 2010 cities with sanctuary legislation had higher levels of robbery and homicide than non-sanctuary cities (Martínez-Schuldt and Martínez 2019). Sanctuary counties on average have a higher population density and are nearly four times more populated than non-sanctuary counties. Although sanctuary counties have slightly less inequality than other counties as indicated by a higher Gini coefficient, they are more disadvantaged overall, and have higher residential instability. Sanctuary counties on average have more than five times as many Asian residents, a significantly higher proportion of native-born Latino, and twice as many foreign-born Latinos than non-sanctuary counties. The correlation between percent native-born Latino and percent foreign-born Latino is 0.63. There are a few counties in which there are more people who identify as foreign-born Latino than those that identify as Latino, resulting in a negative number of native-born Latinos. In these 15 counties, the number of native-born Latinos is coded as zero. Sanctuary counties have a smaller proportion of Black residents, and a higher percentage of votes for democratic presidential candidates than non-sanctuary counties.

When estimated at a cross section, results from negative binomial regression models show that sanctuary counties are not

Table 4
Negative binomial regression models: Estimating the association between sanctuary policies and crime on pooled data, 2013–2016.

	Property Crime			Violent Crime		
	Baseline	% Foreign Born Latino	% Latino	Baseline	% Foreign Born Latino	% Latino
Main Effects:						
Sanctuary County	0.02	0.15*	0.10	0.01	−0.05	−0.08
	(0.04)	(0.07)	(0.06)	(0.08)	(0.11)	(0.10)
Ln Population	0.22***	0.22***	0.22***	0.25***	0.25***	−0.08
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.10)
Ln Population density	0.01	0.01	0.01	−0.10***	−0.10***	−0.08
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.10)
Gini Index	0.01***	0.01***	0.01***	0.03***	0.03***	−0.08
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.10)
Disadvantage Index	0.22***	0.22***	0.22***	0.39***	0.39***	−0.08
	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.10)
Residential Instability	0.04***	0.04***	0.04***	0.02***	0.02***	−0.08
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.10)
% Black	0.00	0.00	0.00	0.01*	0.01*	−0.08
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.10)
% Asian	−0.02**	−0.02*	−0.02**	0.01	0.01	−0.08
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.10)
% Foreign-Born Latino	−0.02***	−0.02***	−0.02***	−0.02***	−0.02***	−0.08
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.10)
% Native-Born Latino	0.01***	0.01***	0.01***	0.01**	0.01**	−0.08
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.10)
% Democrat 2012	−0.01**	−0.01**	−0.01**	−0.01*	−0.01*	−0.08
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.10)
% Democrat 2016	0.01	0.01	0.01	0.01*	0.01*	−0.08
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.10)
Interaction Effects:						
Sanctuary # Foreign- Born Latino		−0.03*			0.01	
		(0.01)			(0.02)	
Sanctuary # Native- Born Latino			−0.01*			0.01
			(0.00)			(0.01)
Constant	−7.15***	−7.16***	−7.16***	−9.86***	0.00***	0.00***
	(0.21)	(0.21)	(0.21)	(0.32)	(0.00)	(0.00)
Lnalpha	−0.76***	−0.76***	−0.76***	−0.51***	−0.51***	−0.51***
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Observations	12,420	12,420	12,420	12,420	12,420	12,420

Standard errors in parentheses.
*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

significantly associated with property or violent crime. These findings are shown in the baseline models in Table 4, and are consistent with previous findings at the MSA-level in which Lyons et al. (2013) did not find any significant association between sanctuary legislation and homicide or robbery at a cross-section in the year 2000. Interaction terms, however, show that in sanctuary counties, one percent increases in foreign-born Latinos and native-born Latinos are associated with three percent and one percent decreases in property crime, respectively. These findings are illustrated in Fig. 1 and Fig. 2. Interaction terms for violent crime, however, were not significant. Although previous research finds that sanctuary policies strengthen the inverse relationship between immigrant concentration and homicide at the city and tract level, this might not be observable at county level (Lyons et al., 2013; Martínez-Schuldt and Martínez 2019). Theoretically, however, these findings are in alignment: immigrant concentration has for the most part been associated with lower crime (Ousey and Kubrin 2018), and this paper joins Lyons et al. (2013) and Martínez-Schuldt and Martínez (2019) in demonstrating that immigrant-friendly policies and practices further strengthen the link between immigrant concentration and public safety.

Consistent with prior research, the county population is significantly associated with higher property and violent crime. However, population density is only associated with higher violent crime. These mixed results mirror mixed findings in existing population density research (Battin and Crowl 2017; Christens and Speer 2005; Regoeczi 2002). As expected, the Gini coefficient of inequality, the disadvantage index, and residential instability are associated with higher rates of crime (Brush 2007; Krivo et al. 2009; Stucky et al. 2012). Each 1% increase the Black population is associated with 1% increase in violent crime, but not property crime. Each 1% increase in the Asian population is associated with a 2% decrease in property crime, but not violent crime. Each 1% increase native-born Latinos is associated with a 1% increase in both property crime and violent crime. Finally, each 1% increase in the foreign-born Latinos is associated with a 2% decrease in violent crime, which supports the immigrant revitalization perspective. The immigrant revitalization perspective posits that immigrant concentration bolsters social cohesion and neighborhood vitality in a way that has positive results for public safety (Ousey and Kubrin 2018). Political liberalism is associated with lower violent crime in 2012, but higher violent crime in 2016. These shifting relationships between percent democratic votes and crime are not the focus of this study, but point to interesting questions about how and for whom political climate and partisanship shape public safety. Although an emerging body of research is examining how partisanship is associated with opinion about crime, law enforcement, and immigration, the relationship between politics and crime remains understudied (for an exception see Koopmans and Olzak 2004), and is gaining in relevance because of how the Trump administration galvanized white power organizations, and the increase in hate crimes towards Asian Americans and Pacific Islanders in the wake of the COVID-19 pandemic, a public health crisis which has been racialized by political statements (NPR 2021a; 2021b).

Evidence from more rigorous analysis of trends over time shows that the adoption of sanctuary practices is associated with within-county decreases in both violent and property crime. These results are shown in Table 5. Post-hoc decreases in violent crime are consistent with neighborhood level and city level studies that find that sanctuary policies are followed by decreases in robbery (Martínez-Schuldt and Martínez 2019). The unconditional fixed effects negative binomial regression included in the table shows coefficients of a similar size and in the same direction.

Finally, there is no evidence that between 2000 and 2012 there were significant differences in crime between counties that would eventually adopt sanctuary practices vs those that would not. However, between 2013 and 2016 violent crime decreased more in sanctuary counties on average than in non-sanctuary counties. These findings are shown in Table 6. The comparison of these two tables

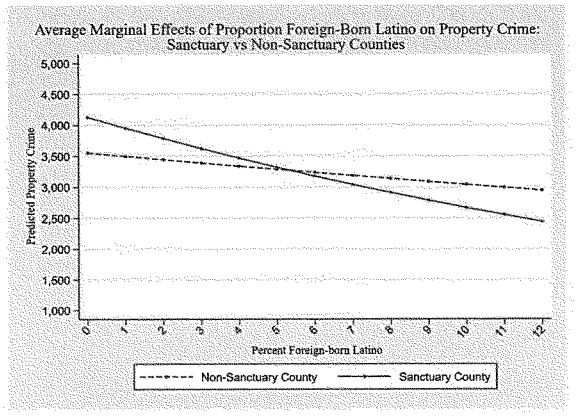

Fig. 1. Average marginal effects of proportion foreign-born Latino on property crime.

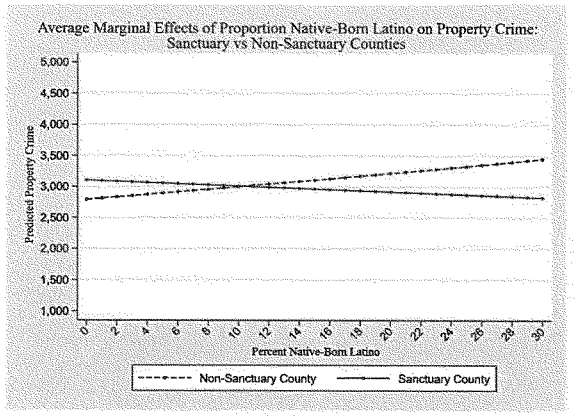

Fig. 2. Average marginal effects of proportion native-born Latino on property crime.

Table 5
Fixed effects negative binomial regression models: Estimating the effect of sanctuary policies on crime within counties, 2013–2016.

	Conditional All Counties		Unconditional Only Largest Counties	
	Property	Violent	Property	Violent
Sanctuary County	−0.06***	−0.09***	− 0.04†	−0.06*
	(0.02)	(0.02)	(0.02)	(0.02)
Dummy for Year	YES	YES	YES	YES
Dummy for County	NO	NO	YES	YES
Constant	−3.04***	−2.41***	9.97	−5.47
	(0.24)	(0.29)	(28.60)	(35.08)
Lnalpha			−4.35***	−4.08***
			(0.03)	(0.04)
Observations	12,232	12,136	1924	1924

Standard errors in parentheses.
†$p < .1$, *p < 0.05, **p < 0.01, ***p < 0.001.

suggests that crime reduction on average occurred after sanctuary practices were implemented, rather than being a reflection of declining crime rates in counties that eventually adopted sanctuary practices.

Table 7 panel A shows the same model dis-aggregated by type of crime: like Table 6, Table 7 shows that between 2000 and 2012 there were no significant differences changes in crime in eventual sanctuary counties vs non-sanctuary counties. Panel B shows that

Table 6
Estimating change in crime rates per 100,000 residents.

	2000–2012		2013–2016	
	Violent	Property	Violent	Property
Sanctuary County	11.8	64	−16.8*	−34.83
	(11.52)	(53.82)	(7.19)	(26.78)
Change in county-level covariates	YES	YES	YES	YES
Constant	−38.11**	55.71	−4.57*	−223.48***
	(12.19)	(56.96)	(1.86)	(6.94)
Observations	6210	6210	12,420	12,420

Standard errors in parentheses.
*p < 0.05, **p < 0.01, ***p < 0.001.

between 2013 and 2016 robbery and burglary decreased more in sanctuary counties than in non-sanctuary counties. Tables 6 and 7 together show that the decrease in violent crime was driven by decreases in robbery, and that although property crime did not significantly change when analyzed in aggregate, burglary decreased more in sanctuary counties than non-sanctuary counties.

5. Discussion

Scholars and practitioners working in the field of immigration have been increasingly concerned with the linking of criminal and immigration-related databases through restrictive immigration initiatives. While these linkages purport to improve public safety by identifying and removing deportable criminals, there is little evidence that public safety has increased as a result. In fact, evidence from this study shows the contrary, that expansive immigration policies and practices, not restrictive ones, contribute to safer communities. Cross-sectional analysis show that the benefit of sanctuary practices is most evident in counties with higher proportions of Latinos, which makes sense considering that Latinx communities, both documented and undocumented, are most vulnerable to changes in immigration policies and practices, and have the most to gain from more immigrant-friendly ones. Subsequent analysis of trends over time shows that sanctuary practices lead to a post-hoc decline in crime rates, findings that were driven by declines in robbery and burglary.

These findings make four theoretical contributions at the intersection of immigrant political integration, spillover effects of sanctuary policies, and criminology. First, *sanctuary practices reflect and reinforce immigrant political and bureaucratic incorporation.* Law enforcement and court systems, along with elected officials, tend to have the least inclusive orientation towards Hispanic newcomers, even in areas where other bureaucrats such as school and hospital administrators are making adjustments to accommodate foreign-born residents (Marrow 2009). What this means is that in areas with sanctuary practices, immigrants are more likely to find accommodating environments across other institutions as well. In a separate line of work, there is consistent evidence that there is a negative, albeit small, association between immigrant concentration and crime (Ousey and Kubrin 2018). My findings are consistent with Lyons et al. (2013) and Martínez-Schuldt and Martínez (2019) who find that sanctuary practices reinforce the negative association between immigrant concentration and crime.

Although I do not have the data to test the specific mechanism, I theorize that sanctuary practices reflect and reinforce increased access to local institutions, more positive relationships with police, and increased representation in elected office, which are community characteristics that protective against crime (Lyons et al., 2013; Vélez 2006). When local law enforcement provides the same rights to undocumented immigrants as they would other residents, immigrants are more incorporated into the social fabric, have increased access to local institutions as well as more formal and more regulated opportunities for employment and education. This is consistent with prior research that shows that in more immigrant-friendly contexts, local residents are more free to invest in their communities in other ways, such as building community organizations, or advocating for more bike lanes and more greenspaces, all of which increase community wellbeing (Garcia 2019; Sharkey 2019).

Another way to understand the protective effect of sanctuary policies is through Du Bois's criminological theories of social disharmony and racial injustice (Werth 2022). Du Bois's theory of social disharmony and racial injustice posits that it is racial inequality and racially exclusive social structures that are the cause of crime and violence. Thus, crime-reducing interventions informed by this theory should focus on eradicating systems of oppression and economic injustice (Werth 2022), which in this case would be eradicating punitive immigration enforcement policies and practices, efforts that can go hand-in-hand with more community-oriented solutions to crime such as establishing community and recreational centers in target communities, interventions that took place in Chicago and that were informed by theories of social disorganization (Schlossman and Sedlak 1983; Shaw and McKay 1942; Werth 2022).

Sanctuary policies and practices both symbolically attenuate the image of the criminalized immigrant, while also erode racialized legal systems which target Latinos and other non-white immigrants for detention and removal. By doing so, sanctuary practices encourage a social structure that is more expansive and inclusive, particularly along the lines of race/ethnicity and documentation status.

Second, *sanctuary practices spill-over to impact non-immigrant Latinos and Latinx communities in the United States.* There is mounting evidence that restrictive immigration policies spillover to impact native-born Latinos, who are not legally subject to immigration laws. These studies demonstrate that in punitive policy contexts, even native-born Latinos are reluctant to contact the police in the case of a crime, express heightened fear of deportation, and perform worse on some indicators of educational achievement (Asad 2020; Bellows 2018; Menjívar et al. 2018). My findings provide evidence that the spillover mechanism occurs for expansive and immigrant-friendly practices as well. I find that sanctuary practices *reverse* the positive association between percent native-born Latino and crime. This finding builds on previous work that sanctuary policies reinforce the negative association between immigration and crime (Lyons et al., 2013; Martínez-Schuldt and Martínez 2019), and sheds light on the overlooked possibility that the benefits of immigrant-friendly practices expand beyond immigrant communities to positively impact native-born communities as well.

This is important because while predominantly Latino areas in the United States tend to have lower rates of crime than predominantly Black areas, they tend to have higher rates of crime than White areas (Feldmeyer 2010). Researchers theorize that Latino areas have less crime than comparable Black areas due to their structural positions: overall lower levels of disadvantage, higher levels of immigrant concentration, better relationships with local bureaucrats, and proximity to White neighborhoods (Vélez 2006). However, Latino areas also have higher rates of crime than predominantly White areas (Feldmeyer 2010). What I find in this paper is that sanctuary practices, are one way to mitigate the positive association between Latino concentration and crime. Immigrant friendly sanctuary practices can bolster some of the very mechanisms that make Latino areas moderately safe, such as, relationships with local institutions and increased participation in the formal labor market.

Table 7
Panel A. Estimating change in crime rates per 100,000 residents, 2000–2012.

	Model 5a	Model 5b	Model 5c	Model 5d	Model 5e	Model 5f
	Robbery	Aggravated Assault	Homicide	Burglary	Motor Vehicle Theft	Larceny
Sanctuary County	2.76	7.25	−0.02	21.70	1.04	26.18
	(2.13)	(10.30)	(0.39)	(16.17)	(6.12)	(38.78)
Change in county-level covariates	YES	YES	YES	YES	YES	YES
Constant	−11.03***	−29.54**	−2.06***	52.24**	−70.04***	35.71
	(2.22)	(10.74)	(0.40)	(16.85)	(6.38)	(40.41)
Observations	6210	6210	6210	6210	6210	6210
Panel B. Estimating Change in Crime Rates per 100,000 Residents, 2013–2016						
Sanctuary County	−7.36***	−9.66	0.25	−48.27***	−0.26	13.61
	(1.01)	(6.97)	(0.38)	(10.16)	(3.75)	(18.57)
Change in county-level covariates	YES	YES	YES	YES	YES	YES
Constant	−0.63*	−3.54*	0.28**	−97.00***	14.03***	−142.52***
	(0.26)	(1.80)	(0.10)	(2.63)	(0.97)	(4.80)
Observations	12,420	12,420	12,420	12,420	12,420	12,420

Standard errors in parentheses.
*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

Third, *sanctuary practices increase social organization.* Findings in this paper show that after the proliferation of sanctuary practices around 2014, crime decreased more in sanctuary counties than non-sanctuary counties, net of other predictors of crime. These differences were driven by decreases in robbery and burglary. Robbery and burglary are unique because they are forms of "parasitical consumption" in the sense that the perpetrators often survive or subsist from what they obtain in these endeavors (Rosenfeld and Fornango 2007). This is distinct from aggravated assault or homicide in which there is likely no material benefit. The anomie tradition in sociology posits that this type of parasitic consumption increases in times of social disorganization, when the culturally agreed upon values of economic success do not align with legitimate ways of achieving those goals (Merton 1938; Messner and Rosenfeld 2013). In criminological literature this is known as "strain theory". Further, incentives for burglary and robbery are higher when there are more opportunities to re-sell stolen goods in an illegitimate market (Venkatesh and Alladi, 2008). It is important to note that immigrants are likely to be *victims* of these types of crimes, because perpetrators believe they have no recourse to the law (Barranco and Shihadeh 2015). By encouraging immigrant incorporation, sanctuary practices bring more people into mainstream networks to seek employment, housing, health care, and other services, thus diminishing the need for illegitimate markets, increasing broad access to protection of law enforcement, therefore also decreasing opportunities to sell stolen goods.

Fourth, *because sanctuary practices encourage crime reporting, the protective effect of sanctuary practices on crime in this and previous research is a conservative estimate.* Previous research has found that punitive immigration policies, and awareness of such policies, can lead to negative attitudes about police and decreased crime-reporting intentions (Menjívar et al. 2018; Menjívar and Bejarano, 2004; Messing et al., 2015; Muchow and Amuedo-Dorantes 2020). More recent research shows that immigrant-friendly sanctuary policies encourage crime reporting among Latinx communities (Amuedo-Dorantes and Arenas-Arroyo 2021; Amuedo-Dorantes and Deza 2019; Martínez-Schuldt and Martínez 2021). Further, the Priority Enforcement Program (PEP) replaced Secure Communities between 2015 and 2017 was intended to target only the people convicted of serious crimes, rather than immigration violations. This shift from targeting all undocumented people to only the ones convicted of serious crimes increased crimes increased crime reporting to police by 4% among Latinos in Dallas, Texas (Jácome 2021). These findings are important because the crime variables used in this study and in previous studies investigating the sanctuary policies-crime link are from the FBI Uniform Crime Reports (UCR). While the UCR are one of the most common ways researchers study crime in the United States, they keep track only of crimes that have come to the attention of the police. We know from victimization surveys that about half of crimes are not reported to the police (Morgan and Thompson 2021). The goal of this study was to assess whether and under what circumstances sanctuary practices might be related to property crime and violent crime. If sanctuary practices also impact crime *reporting*, this threatens the validity of the measurement. What I find is that sanctuary practices are associated with less crime, specifically in areas with higher concentration of native-born and foreign-born Latinos. Is it possible that these findings are driven by changes in crime reporting? If sanctuary practices have an effect on the reporting of crime, it will be a positive effect, and will be particularly evident in areas with higher native-born and foreign-born Latinos. What this means is that the negative association between sanctuary policies and crime is likely a conservative estimate.

These findings demonstrate that despite an unpredictable national immigration enforcement context, local institutional practices can have meaningful benefits for public safety in ways that benefit both immigrant and non-immigrant communities. By decriminalizing immigration, and disentangling immigration enforcement from the criminal justice system, US residents will be able to take care of themselves, their neighbors, and their communities. Specifically, public safety will be bolstered in three principal ways: first, by building trust with the very communities that the police rely on to report crime and cooperate on police investigations. Second, by encouraging residents to contact the police in case of victimization or fear of victimization, rather than retaliate and exacerbate cycles of violence (Kirk et al., 2012; Menjívar and Bejarano, 2004; Theodore and Habans 2016). And third, by extending police protection to all residents, regardless of immigration status, sanctuary practices decrease the vulnerability of people who are foreign born, perceived to be foreign born, and are linguistically isolated (Shihadeh and Barranco 2010).

6. Conclusion

In conclusion, although it was the Trump administration that most forcefully vilified sanctuary policies for protecting criminals and increasing crime, former president Obama also famously stated that immigration enforcement should focus on deporting immigrant "felons and not families" (León 2015). This false dichotomy between the "bad" immigrant felon and the "good" immigrant family reproduces a problematic narrative that sees immigrants as a threatening presence that need to be identified, controlled, and expelled from the country, rather than people whose rights need to be protected (Gupta and Altman 2021). The Biden administration continues to promote this false dichotomy (Gupta and Altman 2021). By excluding entire groups of people based on citizenship or perceived citizenship, a perception that is shaped by race, class, and gender stereotypes (Flores and Ariela Schachter., 2018), the United States risks creating a precarious class that does not have full access to services and resources necessary for successful participation in modern society.

The risk of social and economic exclusion and marginalization has been well documented, and has been conceptualized as segmented assimilation theory (Portes et al. 2005; Waters and Jiménez 2005). Depending on group characteristics, paired with the openness of the receiving context, the biggest risk for immigrants is to assimilate "down", and integrate into a marginalized, racialized, and criminalized population at the bottom rungs of society (Portes et al., 2005; Ziller et al. 2019). By creating policies of racialized surveillance and exclusion from social, economic, and political institutions, federal and sub federal governments create and accelerate processes of isolation and marginalization, which increases the risk of downward assimilation. In this light, it is likely that local immigration policies have an uneven impact on community safety along race, class, gender, ethnicity, and nativity lines.

Aggregate measures of crime used in this paper do not allow for this type of analysis, but future research should consider at least three avenues of inquiry. First, how local policy impacts victim/offender characteristics. Second, whether and how city and state level power structures enable or constrain immigrant social and political participation, and the extent to which this is related to public safety. Third, how systems that marginalize, exclude, and exploit immigrants are maintained and perpetuated through political projects. Who is benefiting from these policies and discourse? What are they gaining?

To conclude, my research joins an emerging body of work that highlights the importance of immigrant-friendly sanctuary practices for public safety. Policy makers in immigrant receiving countries should consider the extensive risk that comes with systematic exclusion and marginalization of certain groups of people by simultaneously increasing vulnerability, rewarding local law enforcement for racially biased policing practices, and curtailing the ability of law enforcement agents to respond to calls for service.

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The immigration–crime relationship: Evidence across US metropolitan areas

Lesley Williams Reid [a,*], Harald E. Weiss [b], Robert M. Adelman [a], Charles Jaret [a]

[a] *Department of Sociology, Georgia State University, Atlanta, GA 30317, USA*
[b] *Department of Sociology, Ohio State University, Columbus, OH 43210, USA*

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Abstract

Despite popular commentary claiming a link between immigration and crime, empirical research exploring this relationship is sparse. Especially missing from the literature on immigration and crime is a consideration of how immigration affects rates of crime at the macro-level. Although individual-level studies of immigrant criminality and victimization tend to demonstrate that immigrants typically engage in less crime than their native-born counterparts, the effect of immigration on aggregate criminal offending is less clear. In this research, we attempt to address this weakness in the literature by examining the effects of aspects of immigration on crime rates in metropolitan areas. We combine 2000 US Census data and 2000 Uniform Crime Report data to explore how the foreign-born population influences criminal offending across a sample of metropolitan areas. After controlling for a host of demographic and economic characteristics, we find that immigration does not increase crime rates, and some aspects of immigration lessen crime in metropolitan areas.
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* Corresponding author. Fax: +1 404 651 1752.
E-mail address: lesleyreid@gsu.edu (L.W. Reid).

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1. Introduction

Over the past two centuries, stereotypical images of the criminal immigrant have appeared in the popular press, political discourse, and public opinion; but the pervasiveness of these images has fluctuated (Hagan and Palloni, 1999; Massey and Denton, 1993). During the heavy migration of Eastern Europeans in the late 19th and early 20th centuries there was great concern about the impact of immigrants on social problems, especially crime, in American cities. These concerns fueled restrictive legislation that aimed to slow, or even to stop, certain immigrants from entering the United States (Hagan and Palloni, 1999). With a strong US economy and declining numbers of immigrants, rampant public expression of such fears waned after World War II (Hagan and Palloni, 1999).

In recent years, however, public opinion has once again turned negative toward immigrants. Since the passage of the 1965 Hart-Cellar Immigration Reform Act, which allowed immigrants from non-European countries easier entry, Asian and Latin American immigrants have dominated the migratory streams into the US (Heer, 1996). Like a century before, there are concerns about the impact of these "new" immigrants on urban crime. Popular anti-immigrant commentary has frequently cited the Hart-Cellar Immigration Reform Act as the cause of subsequent increases in rates of crime, arguing that "under current immigration laws and procedures, frighteningly large numbers of newcomers see crime as their avenue to the American dream" (Tanton and Lutton, 1993, pp. 217). Such commentary argues that immigration has fueled all types of crime, from youth gangs (Miller, 1990) to white collar offending (O'Kane, 1992).

Despite this body of commentary on the alleged link between immigration and crime, empirical research exploring this link is sparse (Martinez and Lee, 1999). Noticeably missing from the literature on immigration and crime is a consideration of how immigration affects rates of crime at the macro-level. Because immigrants enter a pre-existing socioeconomic urban structure, macro-level analyses of the relationship between immigration and crime are essential. Research on the relationship between immigration and crime must take into account the ecological impacts of immigrants that may influence rates of criminal offending. Thus, while individual-level studies of immigrant criminality tend to show that immigrants typically engage in *less* crime than their native-born counterparts, the effect of macro-level aspects of immigration on rates of criminal offending is unclear. On the one hand, it is possible that a large immigrant population in a metropolitan area could contribute to structural factors conducive to crime. For example, a large immigrant population could flood the low-wage labor market forcing the native-born population out of the labor market and into chronic unemployment, leading to criminal offending by those displaced workers (Grogger, 1998). On the other hand, a large immigrant population could rejuvenate economically stagnant metropolitan areas, thus contributing to structural factors that lessen crime (Lee et al., 2001). The existing literature on the relationship between immigration and crime at the aggregate level provides insufficient guidance as to which alternative is most accurate.

We attempt to address this weakness by examining how immigration influences crime across metropolitan areas. Before presenting our analyses, we discuss the theoretical rationales under-girding the ideas that (1) immigration could lead to increases in crime rates and (2) the alternative arguments that suggest immigration could lead to decreases in crime rates. We then discuss what the existing empirical research tells us about the relationship between immigration and crime at both the individual and aggregate levels. Finally, after establishing the context for our research, we present our analyses of the relationship between immigration and crime across a representative sample of US metropolitan areas.

2. Theoretical perspectives on immigration and crime

Early 20th century sociological theories of criminal offending typically argued that there was a positive relationship between immigration and crime (Sellin, 1938; Shaw and McKay, 1942), and that this relationship was driven by the social dislocation and economic deprivation that accompanies the immigration process (Shaw and McKay, 1969). Immigration is not a homogenous process, hence its effects may be both historically contingent and context specific. While contemporary criminological theories, which emerged from early sociological theories, tend to concur with the assumption that immigration increases crime (Bankston, 1998; Chilton et al., 1995; Gurr, 1989; Short, 1997), recent theories of immigration argue against the proposition that heightened immigration leads to increased crime rates. Work on immigration in the urban sociology literature suggests that in the contemporary context large immigrant populations may have beneficial effects on cities, leading to fewer social problems, including crime (Lee et al., 2001; Portes and Mooney, 2002). However, this research recognizes that the context of immigration may mitigate its effects. As the "segmented assimilation" research (Portes and Rumbaut, 2001; Portes and Zhou, 1993; Zhou and Bankston, 1998) contends, the growth of immigrant communities can result in either higher or lower levels of criminal behavior, depending on a mix of factors, principally immigrants' "mode of incorporation" into the US, the external obstacles they face (e.g., labor market situation, racial discrimination), and the type of acculturation they experience (i.e., "dissonant" produces more crime; "selective" and "consonant" are linked to little or no crime). We discuss these perspectives below.

3. The crime-conducive aspects of immigration

Although popular assumptions about the relationship between immigration and crime may be unfounded, there are valid sociological theories that suggest immigration may increase crime. These include theories addressing opportunity structure, criminal subcultures, and social disorganization (Bankston, 1998; Martinez, 2002; Martinez and Lee, 2000a).

Theories that focus on the differential opportunity structure open to immigrants versus native-born persons argue that immigrants face disadvantages that shape their

opportunities for economic success. From higher levels of poverty when they first move into a new country (Clark, 1998; DeJong and Madamba, 2001) to labor market discrimination (Waldinger, 1993), immigrants often face economic hindrances in their destination country. These blocked opportunities may lead to greater levels of criminal offending on the part of immigrants, in a strictly Mertonian sense (Lee et al., 2001; Merton, 1938). In other words, when faced with limited opportunities to achieve economic success through legitimate means, immigrants may turn to illegitimate means. Beyond the effects of limited opportunities for economic success on instrumental property crimes, limited opportunities may also drive violent offending by increasing one's propensity to engage in violent, interpersonal crimes out of frustration or a need for retaliation (Agnew, 1992; Blau and Blau, 1982; Messner and Rosenfeld, 2000; Tonry, 1997).

Theories that focus on the opportunity structure available to immigrants may be too restrictive in assuming that immigrants either engage in crime or do not engage in crime. It is more likely that legal and illegal "work" overlap for immigrants, especially recent immigrants. Freeman (1996) refers to this overlap as the foraging model of criminality. When legitimate opportunities for employment are present the individual takes them. However, when legitimate opportunities are unavailable, the individual is likely to engage in illegitimate, criminal opportunities. Often an individual may engage in both legitimate and illegitimate activities at the same time, since neither low-wage labor market employment nor petty crime alone provide sufficient economic resources (Freeman, 1996; Grogger, 1998).

One way that immigrants may adapt to the limited opportunity structure they face in their destination country is to adopt a criminal immigrant subculture, often organized around ethnic gangs (Bankston, 1998; Short, 1997). In response to limited legitimate labor market opportunities, criminal immigrant subcultures provide increased opportunities for immigrants to engage in crime. This crime typically takes the form of organized involvement in property crimes, such as theft, robbery, and extortion (Bankston, 1998). Organized property crime subsequently leads to increased violent crime as gangs develop counterculture status markers and systematically protect their territory from competing gangs (Bankston, 1998; Cohen, 1955; Sellin, 1938).

Finally, social disorganization theory, especially the earliest variant of the theory (e.g., Shaw and McKay, 1942), suggests that large numbers of immigrants, with diverse cultural backgrounds and limited economic resources, could weaken community institutions (Bankston, 1998). The loss of community institutions (e.g., churches, small businesses) consequently leads to increased criminal offending for both immigrants and native-born residents in such a neighborhood (Sampson, 1991; Sampson and Groves, 1989; Vessey and Messner, 1999). More recent variants of social disorganization theory suggest that immigration may contribute to the crime problem by changing the demographic structure of communities, further degenerating social institutions (Mears, 2001). Immigrants, especially recent immigrants, tend to be disproportionately young and male; both factors that are closely associated with increased criminal offending (Waters, 1999).

All of these perspectives assume, at least implicitly, that immigration increases crime because immigrants commit more crime than native-born persons. It is

possible that immigrants may be no more criminal than the native-born, but that immigration may lead to higher rates of crime all the same. One mechanism by which immigration may increase crime, despite little difference in levels of immigrant and native-born offending, is by creating a pool of potential victims possessing fewer resources with which to protect themselves and their property from crime (Butcher and Piehl, 1998b). Beyond increasing vulnerability of individuals, immigration may increase the vulnerability of entire neighborhoods by weakening community ties thereby lessening collective target hardening, as the classical social disorganization literature would suggest. Of course the effects of this increased vulnerability within the population may not be captured by official crime statistics if immigrants, especially illegal immigrants, eschew the criminal justice system.

A second way that immigration may increase rates of crime, regardless of levels of immigrant offending, is by changing the structure of local labor markets. If immigrants find that employment opportunities are available there is no reason for them to engage in many types of crime. Research on urban labor markets indicates that some immigrants face fewer barriers to employment than some native-born persons. In fact, since a significant proportion of employers prefer immigrants as employees over African Americans, current research suggests that increased immigration may displace other, native-born, minorities (Beck, 1996; Waldinger, 1996, 1997) and lead to increases in the criminality of the displaced groups (Wilson, 1987, 1996). Both of these explanations could clarify why micro-level studies have found little support for the increased criminality of immigrants while the evidence has been less convincing in macro-level studies. Immigration may not be adding to the criminal population. Rather, immigration might reshape urban demographic and economic structures in ways that increase the criminality of native-born persons.

3.1. The crime inhibiting aspects of immigration

Although the bulk of theories in criminology suggest that immigration may increase crime, many theories emerging from the urban sociology literature provide equally valid reasons why immigration may have no effect on crime, and may even lessen it, at least at the aggregate level (Lee et al., 2001). Criminological theories that posit a positive relationship between immigration and crime assume, at least on some level, that immigrants commit crime because they are economically disadvantaged. However, the assumption that immigrants are generally economically disadvantaged may have been more accurate for immigrants at the turn of the 20th century than for more recent arrivals, making theories that rely on this assumption less meaningful in the contemporary context. As Zhou (2001, p. 206) notes, "[t]he image of the poor, uneducated, and unskilled 'huddled masses,' used to depict the turn-of-the-century European immigrants, does not apply to today's newcomers." There is large variability in educational attainment, and thus in economic standing, in the contemporary immigrant population (Zhou, 2001). Immigrants from India for example are far more likely to enter the US with a college degree than, for example, immigrants from Mexico or Ecuador (Zhou, 2001). Immigrant groups who enter the United States with higher educational attainment will find better opportunities and will reside in better

neighborhoods than their low-income counterparts. Since not all immigrants face severe economic disadvantages in the US, it is possible that increased immigration to an area, especially an urban area, may actually lessen crime rates.

Even less-skilled and less-educated immigrants may not face the same economic limitations today that they would have faced in earlier decades. Logan et al. (1994) find that, in addition to ethnic enclaves in some cities, many immigrant groups occupy certain economic niches that allow them to make a living even though they are not competitive with native-owned businesses. Thus, both enclaves and niches become important opportunity structures for immigrants. Immigrants' ethnicity helps them avoid discrimination of native-owned companies by allowing them access to employment in businesses established by earlier immigrants (Zhou, 1992). That said, the very assumption that recent immigrants face discrimination in contemporary labor markets might be erroneous. Employers and their employees who make hiring decisions report that they would rather hire a low-skilled immigrant than a low-skilled native-born worker, especially a low-skilled African American worker (Beck, 1996; Waldinger, 1996, 1997; Wilson, 1996). In urban labor markets, racist assumptions about the productivity and reliability of native-born black workers may operate to the advantage of recent immigrants (Wilson, 1996). This evidence clearly questions the assumption that low-skilled immigrant workers have fewer opportunities than native-born, low-skilled workers. Hence, contemporary research also calls into question the assumption that a lack of economic opportunity results in widespread growth of ethnic youth gangs, or increases in immigrant criminality more generally (Wilson, 1996; Zhou, 1992).

This research suggests that larger immigrant populations in metropolitan areas may invigorate local economies leading to redevelopment of the stagnating economies of the urban core of metropolitan areas. The causal process by which the size of the immigrant population could lessen crime is via job growth, both for immigrants and the native-born; business development in previously economically depressed areas; and the repopulation of the urban core. Kotkin (2000), for example, argues that immigrants' intensive involvement in trade (retail and wholesale), much of it within immigrant enclaves and niches, has positive economic ramifications for non-immigrants working in cargo shipping, material moving, and warehouse occupations. Furthermore, metropolitan areas with large recent immigrant populations are likely to need to expand their workforces in the areas of community and social services, social control, and safety (e.g., police, fire, emergency, health, security, and legal services). A majority of these jobs, especially those with more responsibility and better pay, would likely go to native-born workers. Such increased economic opportunities may result in lower rates of property offending among native-born persons, especially when measured at an aggregate level (see Rosenfeld and Tienda, 1999).

While opportunity structure, cultural differences, and social disorganization approaches may have been useful for understanding immigration in earlier eras, they certainly do not represent a grand theory for immigration-crime research. It is quite possible that the relationship between immigration and crime is historically contingent; that is, the relationship may differ by the nature of immigration at a given time. In that the explanations predicting a positive relationship between immigration and

crime, and the explanations predicting a negative one, developed during different eras, they may explain the relationship during those eras, but not others. It is also possible that the two perspectives may both be useful in explaining the situation of some, but not all, of the contemporary immigration process. The specific characteristics of an immigrant group may shape the immigration–crime relationship, especially when assessing outcomes at an aggregate level; hence neither theoretical perspective may be adequate to explain the effects of a process as heterogeneous as immigration today.

4. Previous empirical research on immigration and crime

Although there are ample theories to draw on to explain the connection, or lack thereof, between immigration and crime at the aggregate level, surprisingly little empirical research draws on this body of theory (for notable exceptions see Hagan and Palloni, 1999; Lee et al., 2001; Martinez, 2002). At the micro-level the question is whether immigrants commit more or less crime than their native-born counterparts. This is a simple question, but answering it is particularly difficult because of the dearth of available data. The vast bulk of official data collected by individual jurisdictions focuses solely on the difference between black and white offending, virtually ignoring differences across other ethnic groups or by immigrant status (Hawkins, 1999). The only official, national data with information on immigrant status are incarceration data, collected on an intermittent basis (McDonald, 1997).

A multitude of problems emerge when using incarceration data to make comparisons between immigrant and native-born offending. Once immigrants are in the criminal justice system, they are more likely than the native-born to face restrictions such as high bail or pretrial detention because of the assumed risk that they will flee to their homelands before trial (Hagan and Palloni, 1999). Pretrial detention, in turn, increases the likelihood that a defendant will be convicted, thereby biasing incarceration data and likely inflating the incarceration rate of immigrants (Hagan and Palloni, 1999). But despite the greater likelihood that immigrants will be incarcerated, once age, gender, and employment status are controlled, immigrants tend to be incarcerated less frequently than the native-born population (Butcher and Piehl, 1998a; Hagan and Palloni, 1999).

Research using the limited data available beyond incarceration rates reveals similarly lower rates of both offending and victimization among immigrants than native-born persons. Across the United States, Toussaint and Hummer (1999) find that recent immigrants have a greater risk of death due to homicide than native-born persons. However, the significance of this relationship disappears once the effects of gender, age, and race are controlled. These national findings are further supported by Martinez and his colleagues' body of work on homicide event characteristics in specific cities (Lee et al., 2000; Martinez, 1997; Martinez and Lee, 2000b). In Miami, which has a large Latino immigrant population, Latinos experience a rate of homicide victimization that is less than half what their population size would predict; conversely native-born blacks in Miami have a homicide rate that is significantly larger

than their population size would predict (Martinez, 1997). In a similar study that compared Latino homicide victimization and offending in Miami and El Paso, Lee et al. (2000) find that the rate of Latino homicide victimization is even lower in El Paso than it is in Miami.

Similarly, disaggregating ethnicity and immigration, Alba et al. (1994) find that Latinos in suburban New Jersey face a risk of criminal victimization that is no different than that of whites. However, Latinos who are recent immigrants to the United States face a higher risk of victimization, indicating that being Latino, even in cities with large immigrant populations, may not be an adequate proxy for immigrant status. With this in mind, Martinez and Lee (2000b) specifically examine the offending of different immigrant groups and find that Haitian, Jamaican, and Mariel Cuban immigrants in Miami have lower levels of criminal offending than native-born persons and show decreasing patterns of violent offending after 1980 as these immigrant groups have become larger, more established, and less populated by young men.

At the micro-level, the bulk of available research contradicts most criminological theories linking immigration and crime. However, current micro-level research is limited in many respects. One of the primary weaknesses is that the focus is almost exclusively on violent offending. There are theoretically driven reasons to expect greater immigration effects on property offending than on violent, personal offending. These reasons draw on the greater economic distress that some immigrants, especially recent immigrants, may face. Economic distress is embedded in the urban structure that immigrants enter, making macro-level analyses of the relationship between immigration and crime critical. Although micro-level research may indicate that immigrants have a lower level of offending than native-born persons, such findings do not take into account the ecological impacts of immigration processes that may influence aggregate rates of criminal offending, regardless of whether the immigrant population is disproportionately criminal (Mears, 2001). However, as urban research indicates, these immigration processes may result in lower levels of crime, instead of higher ones.

Unlike micro-level studies, the results of macro-level studies of the immigration-crime nexus are somewhat mixed. In areas with large immigrant populations, neighborhood-level studies find that the percent of residents who are recent immigrants either has no effect on, or actually lessens the risk of, violent victimization in those neighborhoods (Alaniz et al., 1998; Lee et al., 2001). In a national study, Lauritsen (2001) finds that in suburban areas immigration is positively associated with crime, while in the central city immigration is negatively associated with crime. Analyses that use cities and metropolitan areas as their unit of analysis provide similarly mixed results. In their study of a small sample of metropolitan areas, using pooled data from 1980 to 1990, Butcher and Piehl (1998b) find no relationship between the percent of the population who are immigrants and the crime rate. Likewise, in a study of a larger sample of cities, Martinez (2000) finds that immigration has no effect or a negative effect on most types of Latino homicides. The exception is for felony homicide rates, which are positively related to levels of immigration. Felony homicides occur in the course of committing some other crime. This may provide some clarity

to Hagan and Palloni's (1998) finding that across metropolitan areas, the size of the immigrant population is positively related to rates of property crime.

Quite clearly, previous research on immigration and crime suggests that individual immigrants are no more likely to commit violent crimes than other individuals. However, a number of questions remain unanswered at the macro-level. First, how does immigration influence crime across metropolitan areas? Do high or low levels of immigration in a metropolitan area serve to somehow shape the ecological structure in either crime-conducive or crime-prohibitive ways? Second, do different aspects of the immigration process affect crime differently? In that immigrants come to the United States from different regions and bring with them different levels of social and human capital, the effects of immigration on crime may not be uniform. Finally, does immigration influence rates of violent and property offending differently? And if so, why? Although the literature provides much information on immigration and violent offending, we know less about immigration and property offending. When examining the ways that high or low levels of immigration may shape the structure of urban labor markets, city neighborhoods, and population characteristics, it appears from past research, and a fair share of theoretical work, that immigration may influence property crime rates to a greater extent than violent crime rates. As Hagan and Palloni (1998) point out, petty property crime may serve as a stopgap measure for young, male immigrants seeking subsistence as they negotiate US labor markets. In this research, we intend to contribute a clearer picture of the relationship between aggregate crime, both violent and property, and different aspects of immigration across metropolitan areas.

5. Methods

5.1. Data

We drew a stratified, random sample of 150 Metropolitan Statistical Areas (MSAs) and Primary Metropolitan Statistical Areas (PMSAs) as defined in the 2000 Census to assess the relationship between immigration and crime. We stratified the sample based on region and population size, thus the sample is representative of the regional distribution of US metropolitan areas (MAs). In our sample, all metropolitan areas with a population of one million or more are included, and we chose smaller ones (population 75,000 to 1,000,000) with an equal probability of selection method. The number of metropolitan areas in four categories based on their population in 2000 is: 75,000 to 500,000 ($n=52$); 500,001 to 1,000,000 ($n=38$); 1,000,001 to 2,000,000 ($n=34$); and over 2,000,000 persons ($n=26$). The major metropolitan areas are over-represented in this sample, but since our interest is on immigration and crime, this is appropriate because the largest numbers of immigrants reside in these areas.

A number of researchers have used metropolitan areas in crime research (Messner, 1982; Phillips, 2002; Shihadeh and Ousey, 1996; Williams, 1984). However, some argue that MA-level analyses are not adequate for analyzing crime (Bailey, 1984). We

chose to examine crime in metropolitan areas because of the clear economic linkages between cities and suburbs (Crutchfield et al., 1982). While crime may be more of an urban, than suburban, phenomenon [although that is debated, see Stahura and Huff (1981)], labor markets are not. To examine the characteristics of a labor market by examining only cities or counties would be to ignore broader employment conditions across MAs (Gottdiener and Hutchison, 2000). The social and economic links between central cities and their surrounding counties are also reflected in the increasing suburbanization of the immigrant population (Bankston, 1998). This increasing suburbanization is a further reason to examine crime rates at the metropolitan area level. In a number of metropolitan areas, such as Atlanta, the largest influx of immigration has been to the suburbs of the metropolitan area, not the central city (Bankston, 1998; Hartshorn and Ihlanfeldt, 2000). Analyses at the city or county levels would fail to capture the effects of suburban immigration [see Phillips (2002) for a similar argument for examining racial differences in homicides at the metropolitan area level].[1]

5.2. Variables

5.2.1. Violent and property crime

Since we are interested in comparing the potentially different effects of immigration on violent and property crime, we examine four dependent variables measuring each type of crime. We examined two violent crimes: murder and robbery. For comparison, we examined two property crimes: burglary and theft.[2] As we have outlined earlier, most research on the relationship between immigration and crime has focused on violent crime, particularly homicide. We expand this focus by including four different types of serious crime in our analyses. If, as Hagan and Palloni (1998) suggest, property crime is instrumentally motivated and serves as a method by which recent immigrants remain economically afloat while they establish themselves in US labor markets, then we could expect that a higher proportion of immigrants in an MA would yield more property crime than violent crime. Since most research on this topic has investigated violent crimes, such effects are unknown.

The crime data for this study represent index crimes known to police for the year 2000 (Federal Bureau of Investigation, 2001). Reporting crime data to the FBI were voluntary for law-enforcement agencies, as a result several of the MAs in our sample

[1] In further support of research at the MA-level, those who limit their analyses to cities, counties, or even census tracts, use inflated crime rates because a large number of available victims are not part of the resident population of that same area and hence are not accounted for in the data (Stafford and Gibbs, 1980; Williams, 1984). While the numerator of the crime rate equation, representing the number of crimes committed, counts victimization of residents and commuters, the denominator, representing the number of people at risk of being victimized, excludes all individuals that are not residents of the area under study. The result is an overestimation of crime rates because they are based on a far smaller number of potential victims. Thus, contrary to Bailey's (1984) argument, the use of city, county, and census tract level data create artificial borders that people do not experience. Through the use of MAs, we include regions that are socially and economically connected to the central city in the study.

[2] We exclude aggravated assaults due to concerns with the validity of the measure in the UCR.

had missing values for one or more crime rates in the FBI's Uniform Crime Report.[3] The US Department of Justice, however, imputes annual crime rates for counties (not MSAs) with reported crimes for three months or more in a given year (US Department of Justice, 2002). We used these data to construct crime rates for those MAs in our sample with missing data by summing the offense data for the constituent counties of each MA. We could not, however, replace all of our missing data in this fashion. We did not construct crime rates for MAs that included fractions of a county or for which one or more county was missing data in the US Department of Justice (2002) data.

5.2.2. Immigration

In examining the effects of immigration on crime, we are interested in four different measures. First, we examine the effects of the relative size of the recent foreign-born population as a whole. This variable is measured as the percentage of the MA population that is recent foreign-born, in other words those people who are foreign-born and who immigrated to the United States between 1990 and 2000. These variables come from the 2000 Census (US Bureau of the Census, 2003).

While much of the research linking immigration and crime has only examined overall rates of immigration, we are interested in whether or not the heterogeneous nature of contemporary immigrant streams impacts crime. To capture differences in the immigrant populations in the United States we examine the effects of country of origin and human capital of immigrants. To measure the effects of country of origin, we estimate a second model that includes the percentage of the MA population that was born in an Asian country and the percent of the population born in a Latin American country. To capture, at least in part, differences in human capital across immigrant groups, we estimate a final model that includes a measure of English language proficiency of the immigrant population. This variable is calculated as the percent of the MA population who are recent foreign-born and who report speaking English not well or not at all. All of our measures of immigration were highly skewed. To normalize the distributions and lessen the possible influence of outlying cases, we estimated our models using the natural log of each of these variables. All of the data come from the 2000 Census (US Bureau of the Census, 2003).

5.2.3. Control variables

In addition to our main independent variable, we include a number of control variables that are theoretically related to our dependent variables. Our control variables replicate the baseline model used by Land et al. (1999). Past research indicates that areas with larger populations tend to have higher rates of crime. The larger the area's population, the more opportunities for offending and, at the same time, the less informal social control over potential offenders (Land et al., 1991; Martinez, 2000). To control for this effect we include a scale that captures the population structure of

[3] In order to investigate the reason for the missing data we contacted the Federal Bureau of Investigation. According to this source, the missing data represent cases in which less than 75% of the agencies in the MA reported crime rates for the year 2000.

an MA. This scale is composed of the standardized values of the natural log of the population of the metropolitan area and the natural log of the population density. Beyond the population structure of an MA, research indicates that the level of economic deprivation in an area, both in relative and absolute terms, influences crime rates (Blau and Blau, 1982). Therefore, we include another scale that incorporates the standardized values of the following variables: the natural log of the median family income, the percent of families living below poverty, the percent black, the percent of children under age 18 who are not living with both parents, and the Gini coefficient of income inequality. We include a number of other control variables beyond these two scales. Unemployment has been long linked to aggregated changes in crime (Carlson and Michalowski, 1997; Chiricos, 1987), therefore we control for the unemployment rate in MAs. Further, recent research indicates that the type of employment available in a metropolitan area will have an effect on crime rates.[4] Therefore, we include in our analyses the percent employed in low-skill service sector industries (Crutchfield et al., 1999) and the percent employed in manufacturing industries (Grant and Martinez, 1997; Reid, 2003).[5] Family structure is associated with aggregate crime rates, therefore we include the divorce rate as a control variable (Wells and Rankin, 1991). Likewise, both violent and property crimes are most often committed by young people (Hirschi and Gottfredson, 1983). We control for this effect by including the percent of the population of each MA that is between the ages of 15 and 29. Finally, we control for the effects of region, defined as the location of MAs in the South versus other regions (Wolfgang and Ferracuti, 1967). Our control variables are calculated from data in the 2000 Census (US Bureau of the Census, 2003).

5.3. Analytical technique

Many of the variables in our equations represent mean values for individuals residing within MAs. Analyses based on such averaged data may misspecify the mean square error because observations of larger aggregates typically exhibit less variance around true values than those of smaller aggregates, leading to heteroskedasticity (Johnston and DiNardo, 1997; Messner and Blau, 1987). To remove these heteroskedastic effects we apply Long and Ervin's (2000) HC3 correction to our data. This procedure provides OLS regression coefficients but calculates robust standard errors that are adjusted for both known and unknown sources of heteroskedasticity.

The advantage of HC3 over weighted least squares regression, a more often used correction for heteroskedasticity, is that for the latter the source of the heteroskedasticity must be known and an appropriate functional correction must be available. HC3 corrects heteroskedasticity from both known and unknown sources. Long and Ervin (2000) go so far as to suggest that any type of heteroskedasticity testing, including White's test and Breusch-Pagan, cannot reliably detect all heteroskedasticity and that, for this reason, HC3 should be used as a routine correction for all OLS analyses.

[4] Measures of type of job were not included in Land et al. (1990).

[5] We operationalize low-skill service sector industries as the percentage of the civilian workforce, aged 16 and over, that is employed in retail sales, accommodation, and food services.

Table 1
Correlations, means, and standard deviations

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Homicide rate	—															
2. Robbery rate	.704*	—														
3. Burglary rate	.526*	.523*	—													
4. Larceny rate	.408*	.350*	.774*	—												
5. Population structure	.115	.440*	−.215*	−.224*	—											
6. Deprivation	.526*	.668*	.359*	.275*	.357*	—										
7. % Unemployed	.196*	.274*	.152	.056	−.021	.295*	—									
8. % Low-skill service	.056	−.085	.199*	.199*	−.470*	−.010	.262*	—								
9. % Manufacturing	.019	−.138	−.055	−.084	−.097	−.213*	−.030	−.130	—							
10. % Divorced	.144	.018	.441*	.428*	−.353*	.027	−.101	.363*	−.012	—						
11. % Age 15–29	−.018	−.071	.146	.268*	−.229*	−.074	.056	.180*	−.072	−.124	—					
12. South	.365*	.286*	.528*	.471*	−.093	.363*	−.028	.132	−.132	.207*	.070	—				
13. % Recent foreign-born	−.141	.219*	−.162	−.087	.545*	.202*	.071	−.214*	−.329*	−.242*	.084	−.167*	—			
14. % Asian foreign-born	−.180*	.133	−.318*	−.260*	.523*	.163*	−.005	−.221*	−.333*	−.321*	.149	−.315*	.834*	—		
15. % Hispanic foreign-born	−.000	.270*	−.006	.018	.449*	.227*	.158	−.157*	−.311*	−.134	.015	.003	.904*	.624*	—	
16. % Limited English	.012	.267*	−.056	−.008	.533*	.113	.232*	−.255*	−.277*	−.187*	−.006	−.078	.844*	.601*	.890*	—
Mean/Percent	5.68	153.85	828.59	2836.86	.00	−.00	5.59	11.08	4.21	10.17	21.30	.36	1.00	.26	.41	.28
Standard deviation	3.68	81.98	330.29	872.43	1.74	2.61	1.45	1.82	1.88	1.47	3.41	.48	.84	.80	1.29	1.17

$n = 150$.

* $p < .05$.

Beyond issues of heteroskedasticty, our diagnostic analyses show that there are no multicollinearity problems in our models. As indicated in Table 1, none of the correlations between the independent variables are problematic (none exceed $r=0.624$). The highest variance inflation factor in our models is 2.38, below the suggested limit of 2.50 (Allison, 1998, p. 141).[6] To assess the robustness of our findings to fluctuations in sample composition, we examined outlying and influential data in each of the regression equations. By Fox's (1990) standards, the leverage and Cook's D values were satisfactory, but to err on the side of caution we ran all of the models with and without the influential cases. There were no substantive differences in the results of models with or without these cases. While using the logged versions of our immigration measures lessened the possible influence of extreme cases, we also examined the results of all of the models excluding those cases that were two standard deviations (or more) above the mean on the immigration measure in each model. The results are no different (in any substantive way) from the results including all of the cases.

6. Results

In addition to correlation coefficients, Table 1 displays the basic descriptive characteristics of our variables. Of our four dependent variables, the mean homicide rate is about 6 homicides per 100,000 individuals and the mean robbery rate is slightly less than 154 per 100,000 individuals in the average MA. For property crime, the mean burglary rate is about 829 crimes per 100,000 and the mean theft rate is almost 2837 per 100,000. However, for both violent crime and property crime, there is a broad spread of values around the means. Homicide rates range from less than one per 100,000 in Binghamton, NY, to over 20 in New Orleans, LA. Robbery rates range from 23 in Boise, ID, to 397 in Miami, FL. Likewise, property crimes exhibit considerable variance. The lowest burglary rate was 306 in Scranton, PA, and the highest was 1813 in Topeka, KS. Larceny rates range from 1082 in Wheeling, WV, to 5359 in Topeka, KS. With regard to our primary explanatory variables, most MAs in our sample had modest levels of immigration. In Wheeling, WV, the percent recent foreign-born was 0.24. Wheeling also had the lowest levels of Asian foreign-born (0.19), Hispanic foreign-born (0.05), and limited English speaking recent foreign-born (0.04). At the opposite end of the spectrum, the percent recent foreign-born was 18.46 in Miami, FL. Miami also has the greatest degree of Hispanic foreign-born (30.71) and limited English speakers (20.03). The largest proportion of Asian foreign-born was in San Francisco, CA (15.38).

Tables 2–5 present the results of our regression analyses; Tables 2 and 3 predicting violent crime rates and Tables 4 and 5 predicting property crime rates. Each table presents four regression models. For each dependent variable, we include only our control variables in the initial model. In the second model, we control for the percent

[6] Because of problems with multicollinearity we did not include our measure of English language proficiency variable in the same model with any other measure of immigration.

Table 2
Unstandardized coefficients for the OLS (HC3) regression of homicide rates on measures of immigration and selected control variables

	1	2	3	4
Population structure	0.093 (0.230)	0.442 (0.273)	0.358 (0.279)	0.078 (0.280)
Deprivation	0.606** (0.208)	0.598*** (0.184)	0.650*** (0.183)	0.611** (0.212)
% Unemployed	0.223 (0.239)	0.266 (0.224)	0.163 (0.237)	0.213 (0.246)
% Low-skill service employment	−0.043 (0.199)	−0.059 (0.218)	−0.045 (0.214)	−0.039 (0.207)
% Manufacturing employment	0.262 (0.174)	0.099 (0.175)	0.101 (0.186)	0.267 (0.177)
% Divorced	0.356 (0.203)	0.378 (0.211)	0.306 (0.205)	0.352 (0.210)
% Age 15–29	−0.016 (0.074)	0.035 (0.079)	0.051 (0.086)	−0.017 (0.076)
South	1.551* (0.789)	1.214 (0.720)	0.722 (0.896)	1.546 (0.793)
% Recent foreign-born (ln)	—	−1.195* (0.515)	—	—
% Asian foreign-born (ln)	—	—	−1.322 (0.746)	—
% Hispanic foreign-born (ln)	—	—	0.067 (0.308)	—
% Limited English foreign-born (ln)	—	—	—	0.032 (0.316)
Intercept	−0.156	0.487	0.583	−0.118
R^2	0.346	0.387	0.381	0.346
F	9.34	9.42	7.41	8.28

Notes. Numbers in parentheses are standard errors, $n = 137$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Table 3
Unstandardized coefficients for the OLS (HC3) regression of robbery rates on measures of immigration and selected control variables

	1	2	3	4
Population structure	15.472*** (4.730)	16.024** (5.539)	16.457** (5.816)	13.099** (4.872)
Deprivation	13.818*** (4.172)	13.798** (4.293)	14.568*** (3.949)	14.570*** (4.271)
% Unemployed	10.142 (5.375)	10.216 (5.596)	8.637 (5.288)	8.596 (5.751)
% Low-skill service employment	−2.876 (5.314)	−2.893 (5.530)	−2.674 (5.536)	−2.360 (5.602)
% Manufacturing employment	0.845 (2.516)	0.607 (2.836)	0.012 (3.138)	1.716 (2.511)
% Divorced	8.932 (4.766)	8.956 (4.838)	7.964 (4.608)	8.369 (4.735)
% Age 15–29	0.325 (1.315)	0.424 (1.474)	0.955 (1.650)	0.119 (1.366)
South	21.469 (16.160)	20.922 (15.841)	11.576 (16.615)	20.681 (16.382)
% Recent foreign-born (ln)	—	−1.880 (10.266)	—	—
% Asian foreign-born (ln)	—	—	−14.893 (15.592)	—
% Hispanic foreign-born (ln)	—	—	5.629 (5.994)	—
% Limited English foreign-born (ln)	—	—	—	5.155 (6.166)
Intercept	17.888	18.442	29.155	25.902
Adjusted R^2	0.541	0.542	0.549	0.544
F	21.81	20.04	18.80	19.18

Notes. Numbers in parentheses are standard errors, $n = 140$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Table 4
Unstandardized coefficients for the OLS (HC3) regression of burglary rates on measures of immigration and selected control variables

	1	2	3	4
Population structure	−25.318 (16.010)	−22.623 (19.874)	−17.512 (19.947)	−34.737 (19.520)
Deprivation	29.319* (13.823)	29.225* (14.321)	34.564** (12.780)	32.305* (14.842)
% Unemployed	32.467 (17.823)	32.825 (18.743)	22.051 (18.118)	26.329 (20.259)
% Low-skill service employment	−19.820 (15.944)	−19.902 (16.355)	−18.439 (16.390)	−17.770 (16.534)
% Manufacturing employment	7.306 (13.136)	6.144 (15.261)	1.022 (15.634)	10.764 (14.471)
% Divorced	85.967*** (18.041)	86.086*** (18.332)	79.196*** (18.096)	83.735*** (19.038)
% Age 15–29	12.248 (6.271)	12.733 (6.698)	16.835* (6.533)	11.431 (6.488)
South	249.203*** (63.179)	246.532*** (65.237)	178.833** (63.241)	246.076*** (63.173)
% Recent foreign-born (ln)	—	−9.182 (43.018)	—	—
% Asian foreign-born (ln)	—	—	−106.214 (60.822)	—
% Hispanic foreign-born (ln)	—	—	38.291 (29.999)	—
% Limited English foreign-born (ln)	—	—	—	20.462 (27.023)
Intercept	−393.250	−390.544	−315.111	−361.438
Adjusted R^2	0.485	0.485	0.508	0.488
F	25.78	22.98	21.88	21.62

Notes. Numbers in parentheses are standard errors, $n = 140$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Table 5
Unstandardized coefficients for the OLS (HC3) regression of larceny rates on measures of immigration and selected control variables

	1	2	3	4
Population structure	−49.243 (46.532)	−66.126 (60.714)	−34.861 (59.839)	−105.410 (56.051)
Deprivation	60.708 (36.847)	61.292 (39.604)	76.400* (34.808)	78.514 (40.554)
% Unemployed	31.262 (54.166)	29.018 (58.230)	−1.008 (57.905)	−5.338 (62.182)
% Low-skill service employment	−44.286 (75.336)	−43.768 (76.252)	−39.835 (78.626)	−32.062 (79.134)
% Manufacturing employment	1.270 (31.815)	8.548 (35.911)	−13.053 (38.743)	21.894 (32.356)
% Divorced	231.449*** (55.627)	230.706*** (56.258)	211.224*** (55.674)	218.138*** (57.011)
% Age 15–29	69.864** (23.404)	66.823** (24.778)	81.808** (27.316)	64.994** (23.079)
South	541.356** (169.803)	558.087*** (167.228)	342.382*(172.482)	522.412** (173.822)
% Recent foreign-born (ln)	—	57.518 (118.783)	—	—
% Asian foreign-born (ln)	—	—	−297.627* (150.545)	—
% Hispanic foreign-born (ln)	—	—	125.186 (72.528)	—
% Limited English foreign-born (ln)	—	—	—	122.017 (78.280)
Intercept	−894.521	−911.471	−654.206	−704.823
Adjusted R^2	0.428	0.430	0.457	0.442
F	18.24	15.67	15.98	15.23

Notes. Numbers in parentheses are standard errors, $n = 140$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

of the population who are recent immigrants. The third model replaces the overall level of immigration with the percent of the population who are Asian foreign-born and, separately, Hispanic foreign-born. Our final model includes only the percentage of the population who are recent foreign-born and who report speaking English not well or not at all.

Across our homicide models in Table 2, only deprivation is a consistent predictor of the dependent variable, with greater social and economic deprivation leading to higher rates of homicide. Being located in the South is significant in our initial model, however once we introduce measures of immigration it loses significance indicating that homicide rates are no different in the South than in other regions. In model two, the coefficient for percent recent foreign-born is negative and statistically significant. This finding contradicts the contention that an increased pool of immigrants leads to more crime. It indicates that the greater the relative size of the recent foreign-born population in an MA, the lower that MA's level of homicide. While the percentage of the population of an MA that are foreign-born persons who have recently immigrated to the United States has a negative effect on homicide rates, controlling for differences in demographic characteristics of MAs, none of our other immigration measures are related to homicide. Neither the percent of the population who have emigrated from Asian or Hispanic countries has a significant effect on homicide rates. However, the coefficient for percent Asian foreign-born approaches significances, and is consistent with the effect of percent recent foreign-born. Likewise the relative size of the recent immigrant population who has limited English language skills does not influence homicide rates. Models two, three, and four can all be compared directly to model one. Doing so indicates that the inclusion of measures of immigration significantly increases the explanatory power of models two and three over the baseline.

Table 3 presents the same equations as Table 2, this time predicting rates of robbery. These four models are similar to the models for homicide. In the zero-order correlations presented in Table 1, robbery was the only crime variable to exhibit a significant, albeit modest, positive relationship with immigration. In Table 3, however, no measure of immigration exhibits an effect on robberies. In other words, the robbery rates of MAs with higher levels of recent immigration, Asian immigration, Hispanic immigration, or English-limited immigration are no different from MAs with lower values on these variables. As was the case with homicide, deprivation has a positive effect on rates of robbery. Unlike the results for homicide, MAs with larger populations and population densities exhibit greater rates of robbery. In the case of robbery, the addition of measures of immigration explain significantly more variance in the dependent variable than does the baseline model.

Tables 4 and 5 present the results of our regression analyses predicting property crime rates. As was true of robbery, in the case of burglary immigration does not have an effect on crime (see Table 4). None of the four measures of immigration are statistically significant predictors of burglary. However, in the case of burglary both the addition of region of origin-specific measures and language ability increases the explanatory power of the models over the baseline control variables. While deprivation continues to have a significant and positive effect on rates of burglary, as was the

case for robbery and homicide, both rates of divorce and being located in the South also increase rates of burglary across MAs.

Finally, different measures of immigration have different effects on rates of theft, as is shown in Table 5. Neither overall recent immigration nor language proficiency affect theft, however the relative size of the Asian foreign-born population lessens theft across MAs. This effect holds even taking into account our wide array of control variables. Unlike our results from our three other crime variables, however, deprivation does not have a consistent effect on theft, while being located in the South, divorce rates, and the relative size of the young population are all related to higher rates of theft.

Across our analyses of four different types of crime, there is no evident crime-conducive effect of immigration. The effects of a variety of measures of immigration on homicide, robbery, burglary, and theft are consistent. Even controlling for demographic and economic characteristics associated with higher crime rates, immigration either does not affect crime, or exerts a negative effect. These results are similar to previous findings (Alaniz et al., 1998; Butcher and Piehl, 1998b; Lee et al., 2001). The overall point is that contrary to public discourse, immigration does not inflate crime. In fact, we find that some aspects of immigration may actually lessen crime rates.

7. Discussion

In this research our aim was to address a number of questions that have not been fully answered in the existing literature on immigration and crime. First, we examined how levels of immigration in a metropolitan area serve to shape the ecological structure in either crime-conducive or crime-prohibitive ways. Second, we examined a number of measures of immigration that capture differences in country of origin, and differences in human and social capital that recent immigrants bring with them. Finally, we explored how immigration influences rates of violent and property offending differently. Thus, our intention in this research was to contribute a clearer picture of the relationship between aggregate crime, both violent and property, and aspects of immigration across metropolitan areas.

Our findings support neither the conventional conceptualizations nor the criminological theories that predict increased immigration will lead to increased rates of crime. A number of criminological theories suggest that MAs with higher levels of immigration will have higher rates of crime. Theories that consider the opportunity structure of immigrants, immigrant subcultures, and social disorganization all posit that more immigrants lead to more crime. However, our findings show that at the aggregate level these theories are, at least partially, inaccurate. For neither the case of violent crime nor property crime, in our analyses, does recent immigration inflate crime across metropolitan areas. In fact, recent immigration and Asian immigration actually exhibit a crime-reducing effect on homicides and thefts, respectively.

It is possible, as some urban scholars suggest, that immigration may actually revitalize urban areas. The economic revitalization that may be produced by

entrenched immigrant enclaves, especially in central cities, could lessen criminality accounting for our findings, especially for property crime. We recognize that occupational and industry-level research has found that immigrants sometimes displace, or leapfrog, native-born minority groups in specific jobs (Adelman and Tolnay, 2003; Waldinger, 1996, 1997). However, if an influx of immigrants creates a population to be served both by the public and private sector, a large immigrant population may actually produce better jobs for native-born persons. Hence native-born minority group members may be promoted into higher-paying jobs as immigration increases because immigrants do not only "take" jobs, they also "make" jobs that the native-born obtain (Muller, 1993; Rosenfeld and Tienda, 1999). In this way, immigration may increase the economic opportunities for native-born persons and lessen the rate of offending among those persons across MAs. Therefore, the modest crime-lessening effects observed in our analyses may be the result of not simply lower individual levels of immigrant offending, but also of native-born offending.

Throughout this paper, we have worked from the premise that immigration may affect crime not simply through immigrant offending, but by changing broader economic conditions that have an influence on the offending rates of the native-born. It is possible, however, that these broad economic conditions may be influencing immigration rates themselves. The question that this research cannot answer is whether or not recent immigrants self-select into metropolitan areas with lower rates of crime. While those entering the United States may not have information about crime on which to base their decisions on where to relocate, they likely have information on other factors, such as economic deprivation, that are related to crime. Alternatively, decisions regarding where to relocate are heavily influenced by the location of family and friends who are already living in the United States. Thus, the relationship between rates of immigration and crime may be reciprocal. It is not within the scope of this paper to tease out such reciprocal effects, but future research that takes a longitudinal, versus cross-sectional, approach should explore these possibilities.

Despite this limitation, our findings add an important dimension to the sparse research examining the macro-level effects of immigration on rates of crime. It appears that anti-immigrant sentiments that view immigrants as crime prone are not only inaccurate at the micro-level, they are also inaccurate at the macro-level. In finding that immigration has no effect on patterns of violent crime at the community-level, Lee et al. (2001) conclude that if such findings were replicated, the way the immigrant-crime story is told would be substantially altered. Not only do our findings mirror their results, at the metropolitan area level, we add yet another "unexpected plot twist" (Lee et al., 2001, p. 575). The story is not simply a contradiction of popular rhetoric that holds immigration as contributing to violent crime. Instead, increased immigration may actually be beneficial in terms of lessening some types of crime. In fact, that some aspects of immigration lessen both homicide and theft across MAs suggests that municipalities seeking to reduce crime rates may actually benefit by encouraging the development of economically vibrant immigrant communities.

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ARTICLE

Providing Sanctuary or Fostering Crime? A Review of the Research on "Sanctuary Cities" and Crime

Daniel E. Martínez[1] | Ricardo D. Martínez-Schuldt[2] | Guillermo Cantor[3]

[1] University of Arizona

[2] University of North Carolina, Chapel Hill

[3] American Immigration Council

Correspondence
Daniel E. Martínez, University of Arizona, United States.
Email: daniel.martinez@arizona.edu

Abstract

Recent high-profile, isolated incidents of violent crime committed by deportable noncitizens have led to increased public attention paid to so-called "sanctuary" cities, with some policymakers calling for the eradication of policies limiting local officials' role in the enforcement of immigration law. This current public and political debate provides an opportunity to critically examine the existing literature on immigrant "sanctuaries." We begin by offering a broad definition and description of "sanctuary" policies. We follow by discussing how and why such policies have evolved since the early 1980s. Considering the public safety concerns often articulated in contemporary political discourse, we then offer possible sociological explanations regarding how these policies might either be positively or negatively associated with crime. We subsequently highlight findings from existing empirical research that examines the relationship between the adoption or presence of "sanctuary" policies and crime. The few empirical studies that exist illustrate a "null" or negative relationship between these policies and crime. We conclude by offering possible directions for future research.

1 | INTRODUCTION

The 1986 Immigration Reform and Control Act (IRCA) remains the last major inclusionary comprehensive immigration reform passed in the United States. Although IRCA regularized the immigration statuses of approximately three million people by 1992, it also served as a catalyst for expanded border and interior immigration enforcement (Massey, Durand, & Malone, 2003). In the late 1990s, seasonal migration declined due to increased border enforcement, which in turn led to a dramatic increase in the unauthorized population residing in the United States as fewer migrants returned to their countries of origin. By 2014 the unauthorized population stood around 11.1 million people, 52% of which migrated from Mexico (Passel & Cohn, 2016). Unfortunately for this group and their mixed-status family members, the U.S. Congress has failed to pass comprehensive immigration reform in over 30 years.

Congress' inability to pass immigration reform coincides with major transformations in the way immigration laws are enforced. Whereas the federal government and its relevant agencies—currently, U.S. Immigration and Customs Enforcement (ICE) and U.S. Customs and Border Protection (CBP)—have traditionally been accountable for enforcing

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immigration laws, recent policies and practices, made possible by changes to the Immigration and Nationality Act in 1996, shifted a share of the responsibility to state and local officials. Since 1996, broad enforcement initiatives requiring increased cooperation between the federal government and state and local law enforcement resulted in an expansion of the government's capacity to place individuals in removal proceedings. These enforcement initiatives include the Secure Communities program and the Criminal Alien Program. Additionally, the 287(g) program allowed ICE to enter agreements with local and state enforcement agencies authorizing designated officers to perform immigration enforcement activities. Moreover, pressure mounted in the era following the 9/11 terrorist attacks to further involve state and local law enforcement officials in the policing of immigration laws (Meissner, Kerwin, Chrishti, & Bergeron, 2013). Scholars, legal experts, and nongovernmental organizations have criticized these efforts and raised concerns regarding the legality of the programs, public safety, humanitarian considerations, issues of ethno-racial profiling, civil rights violations[1], and fiscal constraints at the local level (Dreby, 2012; Gardner & Kohli, 2009; Homeland Security Advisory Council, 2011; Lasch, 2013; Provine, Varsanyi, Lewis, & Decker, 2016; Wells, 2004; Wishnie, 2004).

Citing notable concerns, many communities across the country have adopted ordinances that explicitly limit cooperation with the federal government in enforcing immigration laws. These ordinances, colloquially known as "sanctuary" policies, are partly the result of a tension between the federal government's responsibility to enforce immigration laws and the state and local government's obligation to protect the well-being of all residentially present individuals (Wells, 2004). Although local communities adopt "sanctuary" policies to limit cooperation with the federal government, the implementation of these policies does not imply that noncitizens are protected from federal immigration enforcement action, leading some to suggest that the term "sanctuary" is a misnomer (Tramonte, 2011). In fact, many "sanctuary" jurisdictions allow for local assistance and cooperation in the enforcement of federal immigration laws when individuals have been convicted of violent or serious felony offenses[2].

However, "sanctuary" jurisdictions have faced criticism, particularly as related to "law and order" concerns. Public and political attention paid to "sanctuary" policies increased substantially in July 2015 after the tragic death of 32-year-old Kathryn Steinle. Steinle was shot and killed in San Francisco, a well-known "sanctuary" city. The alleged shooter was 45-year-old Juan Francisco López-Sánchez, an unauthorized immigrant from Mexico who had seven prior felony convictions and five deportations from the United States, mostly for nonviolent drug offenses (Littlefield, 2015).

Steinle's murder sparked a divisive political debate regarding the enforcement of immigration laws and the legitimacy of "sanctuary" policies. Following the tragic death of Steinle, several legislative proposals were introduced at the federal level that would increase penalties for unauthorized reentry and limit federal grants available to "sanctuary" cities. These initiatives have largely failed to materialize due to lack of political support. However, two recent versions of these bills successfully passed through the House of Representatives in June of 2017 and are currently waiting to be heard in the Senate (Marcos, 2017). In a similar vein, President Trump signed an executive order in January of 2017 that aims to strip federal funding from "sanctuary" jurisdictions (Executive Order 13768, 2017). Thus, the current public and political discourse surrounding unauthorized immigration in general—and deportable noncitizens more specifically—provides an opportunity to critically examine the existing literature on "sanctuary" policies.

In what follows, we provide a broad definition of so-called "sanctuary" policies and discuss how and why such policies have evolved since the early 1980s. Considering the public safety concerns often articulated in contemporary political discourse, we then offer possible sociological explanations of how these policies might either be positively or negatively associated with crime. We subsequently highlight findings from existing research that examines the association between the adoption or presence of "sanctuary" policies and crime. Our review of the literature identifies notable variation in the definition of "sanctuary" ordinances. In some cases, "sanctuary" policies are strictly defined as local efforts aimed at limiting cooperation with federal enforcement. In other cases, such policies are described as preconditions for immigrant inclusion at the local level. In terms of the relationship between "sanctuary" policies and crime, we find no evidence in the extant literature supporting the assertion that the implementation of such policies fosters crime. We conclude by offering possible directions for future research.

2 | A BRIEF HISTORY OF "SANCTUARY" POLICIES

In the early 1980s, hundreds-of-thousands of Central American immigrants fled violent conflict and political persecution in their countries of origin and sought refuge in the United States. The U.S. federal government often rejected Central American asylum applications during this period. In response, religious organizations began offering "sanctuary" in their places of worship to protect immigrants from deportation and provided other forms of assistance including food, clothing, housing, and social or legal services (Bau, 1994; Ridgley, 2008; Villazor, 2008). This led to a so-called "Sanctuary Movement" throughout the United States in the 1980s (Bilke, 2009; Garcia, 2009; Gregorin, 2011; Villazor, 2010). Following the lead of religious congregations and private individuals, localities enacted municipal ordinances stipulating that city officials would not cooperate with Immigration and Naturalization Service—the agency that administered federal immigration laws and regulations until 2003—in the arrest or deportation of noncitizens in the wake of IRCA, which expanded immigration enforcement into the interior United States (Bau, 1994; Ridgley, 2008).

"Sanctuary" policies continued to expand at the local level with the implementation of federal reforms, including the 1996 Illegal Immigration Reform and Responsibility Act (IIRIRA), the 1996 Antiterrorism and Effective Death Penalty Act, and more recently, the "War on Terror" (Bilke, 2009; Ridgley, 2008). The '96 laws and the War on Terror led to the devolution of immigration enforcement from the federal to the state and local levels (Coleman, 2007; Stumpf, 2006). These pieces of legislation expanded the list of deportable offenses, expanded the category of "aggravated felony," which is a felony class that only applies to noncitizens, beyond murder and trafficking of drugs or firearms[3], and limited "the legal review of immigration and criminal charges brought against undocumented migrants" as well as other immigrants (Coleman, 2007, p. 56).

Additional localities adopted "sanctuary" ordinances during the 2000s in response to federal initiatives such as the 287(g) program, Secure Communities, and the Criminal Alien Program. Whereas 287(g) deputizes local law enforcement officials to enforce immigration law (Meissner et al., 2013), Secure Communities checks the fingerprints of individuals who encounter local law enforcement agents against federal electronic databases (Kubrin, 2014; Waslin, 2011). Similarly, the Criminal Alien Program aims to identify allegedly deportable noncitizens incarcerated in correctional facilities. The Criminal Alien Program typically involves an interview or screening (i.e., an "encounter") of an individual while they are incarcerated to determine whether the individual is potentially removable. The encounter may result in ICE's placing a hold or detainer on the individual (American Immigration Council, 2013).

Because of 287(g), Secure Communities, and the Criminal Alien Program, some localities with existing "sanctuary" policies responded by further limiting their cooperation with federal authorities on matters of immigration enforcement. According to the Immigrant Legal Resource Center, four states, 364 counties, and 39 cities have policies that to some degree limit their cooperation with requests from ICE to hold noncitizens in detention.[4] In addition, the National Immigration Law Center (NILC) notes that 65 cities or local law enforcement agencies have adopted "sanctuary" ordinances (more broadly defined) in some form since 1979, with the majority enacted between 2000 and 2008[5]. In sum, although "sanctuary" policies were initially developed to protect Central American refugees in the 1980s, they have since expanded to limit the role of local law enforcement agencies in the enforcement of federal immigration laws (Ridgley, 2008).

3 | CONTEMPORARY "SANCTUARIES"

How do contemporary "sanctuaries" differ from those established during the earlier Sanctuary Movement in the 1980s? To address this question, Villazor (2008) draws important distinctions between "private" and "public" sanctuaries. Private sanctuaries include religious congregations or nongovernmental organizations that offer food, shelter, or other forms of aid to unauthorized immigrants, while public sanctuaries consist of localities that "limit government employees, particularly local police officers, from inquiring or disseminating information about the immigration status

of immigrants whom they encounter" (Villazor, 2008, p. 148). Given the current political debate surrounding the alleged association between "sanctuaries" and public safety, we use the term "sanctuary" to identify what Villazor refers to as "public sanctuaries."

There is no official federal designation for what constitutes a "sanctuary" (Armenta, 2017; Garcia, 2009). For instance, President Trump's January 2017 executive order entitled "Enhancing Public Safety in the Interior of the United States" describes "sanctuary" jurisdictions as locales that refuse to comply with federal statute 8 U.S.C. 1373, which restricts jurisdictions from adopting policies that prohibit government entities or personnel from exchanging information related to individuals' immigration statuses with ICE or CBP (Executive Order 13768, 2017). On the other hand, the U.S. Department of Justice (DOJ) describes the term as a "jurisdiction that may have state laws, local ordinances, or departmental policies limiting the role of local law enforcement agencies and officers in the enforcement of immigration laws" (2007, footnote 44). As such, the DOJ's broader definition is perhaps a more accurate conceptualization of the ways in which "sanctuary" jurisdictions operate.

To account for variation across geography, some scholars use broader terms such as "sanctuary policy" or "ordinance" instead of "sanctuary city" (Price, 2014; Ridgley, 2008; Walker & Leitner, 2011). Despite this nuance, "sanctuary policies" generally fall into one of three categories: "(1) 'don't ask' policies that limit inquiries related to nationality or immigration status; (2) 'don't enforce' policies that limit immigration-related arrests or detentions; and (3) 'don't tell' policies that limit what information enforcement offices may share with federal officials" (Kittrie, 2006, as cited by Sullivan, 2009, p. 574). However, it is not uncommon for localities to use a combination of these approaches. Because "sanctuary" policies vary across geography (Kittrie, 2006; Pearson, 2015; Sullivan, 2009) as well as in terms of the protections they provide to immigrants, we use the term "limited cooperation policy" in lieu of "sanctuary city" or "sanctuary policy" throughout the remainder of this article.

4 | POTENTIAL LINKS BETWEEN LIMITED COOPERATION POLICIES AND CRIME

In the proceeding sections we identify the possible relationships between limited cooperation policies and crime. For the sake of parsimony, we present the dominant perspectives of both opponents and supporters of limited cooperation policies. Nevertheless, we must emphasize that the implementation of limited cooperation policies at the local level is not merely about whether a particular locality is "pro-immigrant" or "anti-immigrant." This would be an oversimplification, as some jurisdictions do not honor immigration detainers due to concerns over potential civil rights violations as well as financial constraints at the local level (Kittrie, 2006). In other words, a local jurisdiction may not comply with immigration detainers simply because it is not in their best legal or financial interest. However, our aim here is to capture some of the core concerns stemming from opposing sides of the debate.

4.1 | Arguments against limited cooperation policies

Limited cooperation policies have become a heavily contested political and legal issue in recent years. Drawing on a sociological perspective, we explore the possible consequences of limited cooperation policies as well as the logics that link these policies with public safety outcomes. As mentioned, some contemporary opponents often view limited cooperation policies as a public safety issue. In fact, President Trump's executive order, "Enhancing Public Safety in the Interior of the United States," claims that "Sanctuary jurisdictions across the United States willfully violate Federal law[6] [...]. These jurisdictions have caused immeasurable harm to the American people and to the very fabric of our republic" (Executive Order 13768, 2017). However, the executive order does not specify exactly how, if at all, "sanctuary" jurisdictions threaten public safety. Below we discuss a few ways in which limited cooperation policies could potentially be linked to increased crime.

Explanations that hypothesize a positive correlation between limited cooperation policies and crime point toward three notable types of transformation within immigrant communities: behavioral changes, compositional

changes, or changes in the relative population size (see Treyger, Chalfin, & Loeffler, 2014 for similar discussions in the context of the Secure Communities program). Hypothetically, the adoption of limited cooperation policies could bring about *behavioral* changes in the local immigrant population by increasing the likelihood that individuals will engage in crime. For example, noncitizens may perceive there to be a lower risk of deportation in an area where officials refuse to cooperate with the federal government's immigration enforcement agencies. If the risk of deportation or removal from the country serves as a deterrent for immigrant-related crime, then noncitizens may be more inclined to engage in crime in the absence of that threat. Although intuitive, the "behavioral change" hypothesis suffers from two notable weaknesses. First, some have suggested that noncitizens may not fully understand the nuances of limited cooperation policies, especially in contexts where the federal and local governments possess opposing policy orientations towards immigrants (e.g., if a local government is more receptive or inclusive of immigrants compared to more punitive federal enforcement actions) (Kittrie, 2006). Thus, noncitizens are unlikely to substantially alter their behavior across jurisdictions. Second, and more important, even if noncitizens have perfect understandings of limited cooperation policies, local governments often make exceptions for violent and serious felony offenses. Therefore, the most serious (e.g., publicly harmful) offenses should have similar risks of punishment (e.g., removal) across all jurisdictions.

A second possible explanation for the positive association between limited cooperation policies and crime is that such policies bring about *compositional* shifts in local immigrant populations. That is, the existence of limited cooperation policies could *attract* noncitizens that are most likely to engage in crime on day-to-day basis. If the proportion of criminally-inclined noncitizens increased in a specific area, then we would expect crime rates to also increase net of other factors. Obviously, this line of reasoning, like that of the "behavioral change" perspective, assumes that noncitizens across the United States are aware of specific jurisdictions with limited cooperation policies but are not sufficiently informed to understand that such policies do not provide true "sanctuary." In addition, these lines of argument also generally associate immigrants with criminality. Nevertheless, a large body of research relying on both self-report surveys and official statistics reveals that immigrant adults and youths, relative to the general U.S. population, have lower levels of criminality and are consequently less likely to engage in criminal or delinquent behavior (Powell, Perreira, & Harris, 2010; Rumbaut & Ewing, 2007; Ewing, Martinez, & Rumbaut, 2015; Bersani, Loughran, & Piquero, 2014; Vaughn, Salas-Wright, Delisi, & Maynard, 2014).

A third potential connection between limited cooperation policies, immigration, and crime stems from the social disorganization framework. According to this framework, dense community social and institutional network structures inhibit crime and delinquency by fostering informal social control mechanisms such as the supervision of adolescents (Sampson, 2011; Shaw & McKay, 1969). However, structural factors like poverty or residential instability prevent, disrupt, and erode social network ties and weaken local institutions. When social and institutional networks are fractured, communities become characterized by social disorganization, or the inability of the community to maintain order (Sampson, 2011). As such, community crime is expected to increase under the condition of social disorganization (Shaw & McKay, 1969). Thus, even if immigrants have lower levels of criminality—as suggested by much of the extant literature—this framework would suggest that a rapid increase in the immigrant population may burden local institutions, deplete their resources, and limit new and existing community members' ability to effectively communicate and form or maintain social relationships. In other words, rapid social change—stemming from immigration—may destabilize communities and negatively impact informal and formal social control mechanisms resulting in increased crime (Shaw & McKay, 1969; Stowell, 2007).

A substantial body of research tests hypotheses derived from the social disorganization framework that link immigration to crime at aggregate levels. For example, Martinez, Stowell, and Cancino (2008) examined patterns in homicide in neighborhoods, defined as census-tracts, in the cities of San Diego and San Antonio. The authors found that the concentration of recent immigrants is negatively associated with homicide in San Diego neighborhoods overall but found a null relationship in Latino neighborhoods specifically. Similarly, Martinez and colleagues found

no evidence of a relationship between immigrants and crime in San Antonio across Latino communities and in communities overall. These results fail to support the expectations of social disorganization as it relates to immigration and crime.

Some scholars contend that research fails to find evidence in support of the social disorganization framework because researchers predominantly examine the immigration-crime link in traditional immigrant designations like San Diego, San Antonio, or other large metropolitan gateways (Shihadeh & Barranco, 2010). This position argues that traditional destinations possess institutional and social support structures that can readily accommodate influxes of new immigrants and protect against social disorganization. In contrast, newer immigrant destinations, such as cities and counties in North Carolina, lack necessary institutional support networks and are disproportionately likely to experience weakened social organization and thus an increase in violence (Shihadeh & Winters, 2010). However, much of the evidence supporting the claim that immigration leads to social disorganization in new destinations comes from analyses of cross-sectional data, which may suffer from omitted variable bias (Light, 2017). These studies are also limited in that they may not correctly model the temporally dynamic relationship between immigration and crime that is theoretically expected based on the social disorganization framework (Ousey & Kubrin, 2017).

A recent meta-analysis by Ousey and Kubrin's (2017) sheds further light on the question of whether or not there is a macro-level relationship between immigration and crime as well as the degree to which the relationship unfolds over time or differs between contexts of reception. The authors consider a sample of over 50 studies examining the immigration-crime link, which includes over 500 point estimates of an immigration effect. After controlling for various dimensions of study design, their meta-analysis found an average negative—albeit weak—effect of immigration on crime. Furthermore, while there was no significant average relationship between immigration and crime in studies of new destination contexts and for studies that relied on cross-sectional methods, there was a significant *negative* effect of immigration on crime in traditional destinations and in studies that utilized longitudinal analytic designs. These results, coupled with the evidence at the individual-level, strongly suggest that an influx of immigrants *does not* increase crime in communities across the United States, despite the rhetoric presented by opponents of limited cooperation policies.

4.2 | Arguments in favor of limited cooperation policies

Supporters of limited cooperation policies contend that such policies potentially heighten community safety and protect *all* members from criminal victimization. For example, limited cooperation policies may strengthen community-police relations because noncitizens do not feel at risk for removal when they interact with police, including when they are victims of violent crime. As such, noncitizens and immigrant communities may be more willing to seek assistance from law enforcement officials or cooperate with criminal investigations (Sullivan, 2009). A reduction in crime in the context of greater crime reporting and police cooperation may seem counterintuitive. Crime rates, as reflected in official statistics such as the FBI's Uniform Crime Reports, could increase if more crimes are reported to police. However, greater cooperation with police should facilitate the clearance of crimes by arrest, which has been shown to incapacitate and deter would-be criminals (Levitt, 1998). Limited cooperation policies, according to this logic, may contribute to a *reduction* in crime over time through improved cooperation with police and increased arrests.

Lyons, Vélez, and Santoro (2013) situate limited cooperation policies within a broader context of what they describe as "immigrant political opportunities" and argue that localities with "open political opportunities can generate a 'spiral of trust' that improves communication between officials and immigrants, promotes legislation protecting immigrant interests, and generates system-level trust in government on the part of immigrant groups" (p. 609-610). A "spiral of trust" may also be effective in increasing public and informal social controls that facilitate "collective crime control efforts" (Carr, 2005, as cited by Lyons et al., 2013, p. 610). This latter point is particularly important, as

criminologists have highlighted the importance of social organization and collective efficacy in reducing neighborhood crime (Sampson & Graif, 2009; Sampson, Raudenbush, & Earls, 1997).

5 | EXTANT EMPIRICAL RESEARCH ON "SANCTUARY" CITIES AND CRIME

Despite well-publicized political debates offering various forms of conjecture, only four empirical studies to our knowledge systematically examine the relationship between limited cooperation policies and crime (Lyons et al., 2013; Wong, 2017; Gonzalez, Collingwood, & El-Khatib, 2017; Martinez-Schuldt & Martinez, n.d.). Although the authors of these studies conceptualize and operationalize "sanctuaries" differently, examine patterns in crime across different units of analyses, rely on diverse analytic techniques, and focus on distinctive points in time, none of the studies support the claim that "sanctuaries" are more crime-prone than non-sanctuaries. In what follows, we provide a brief review of these four studies.

Wong (2017) compares the overall violent and property crime rates for demographically matched "sanctuary" and "non-sanctuary" counties across the United States. Wong defines "sanctuary" jurisdictions as counties that do not accept immigration detainer requests from ICE and categorizes counties by relying on data provided by ICE through a Freedom of Information Act request. Wong draws on the American Community Survey (ACS)'s 2015 5-year estimates to match "sanctuary" and "non-sanctuary" counties along demographic characteristics (e.g., population size, percentage of the population that is Latino, percent of the population that is foreign-born, etc.). Lastly, Wong derives violent and property crime rates from the FBI's Uniform Crime Reports (UCR). After matching counties and comparing their crime rates, Wong finds that large central metropolitan counties, micropolitan counties, and rural counties with "sanctuary" policies are *safer* than their "non-sanctuary" counterparts. However, similarly matched "sanctuary" and "non-sanctuary" large fringe metropolitan counties, medium metropolitan counties, and small metropolitan counties have crime rates that are not statistically different. Unfortunately, Wong's cross-sectional approach only confidently reveals the existing differences (or similarities) between counties as opposed to longer-term consequences of limited cooperation approaches.

Gonzalez et al. (2017) offer an improvement over Wong's (2017) approach by employing a similar causal-inference matching strategy on U.S. cities in conjunction with multivariate regression (to account for imperfect matching). Specifically, the authors compare overall violent crime, overall property crime, and rape rates in U.S. cities before and after the implementation of "sanctuary" policies as well as between 2000 and 2014, more generally. Unlike Wong (2017), Gonzalez et al. (2017) rely on a more encompassing definition of "sanctuary" cities, and include any "city or police department that has passed a resolution or ordinance expressly forbidding city or law enforcement officials from inquiring into immigration status and/or cooperation with Immigration and Customs Enforcement (ICE)" (p. 2). Overall, Gonzalez et al. (2017) find that "sanctuary" cities do not experience any uniform shift in crime rates in the year following the implementation of a "sanctuary" policy for all three of their outcome measures. Furthermore, Gonzalez and colleagues find no statistically significant differences in crime rates between 2000 and 2014 for their matched cities.

Like Wong (2017), Gonzalez et al.'s (2017) analysis is not without limitations. For example, the authors compare overall violent and property crime rates at the expense of more refined and/or reliable measures (e.g., homicide). It is possible that the effects (positive or negative) of limited cooperation policies are specific to certain types of crime. On a more technical note, the authors employ a regression analysis (post-matching) to compare crime *rates*, opposed to incidents, which may also suffer from bias (Osgood, 2000).

Lyons et al. (2013) draw on data from the National Neighborhood Crime Study (NNCS) and use multi-level statistical techniques to examine the relationships between neighborhood factors, city-level characteristics, and neighborhood violent crime. Specifically, the authors examine the number of homicides and robberies that occur in census-tracts (a proxy for neighborhoods) in 2000. Although not the central focus of the analysis, Lyons et al. (2013) include a city-level measure for "sanctuary" city, which they created by drawing on data provided by NILC.

Lyons and colleagues find that immigrant concentration is associated with a reduction in neighborhood violence, a finding that has been routinely demonstrated in social scientific research and often described as the "immigrant revitalization perspective" (Martinez & Lee, 2000; Ousey & Kubrin, 2017). However, the authors find that the inverse relationship between neighborhood immigrant concentration and crime is stronger in "sanctuary" cities (municipalities with at least one law or formal resolution limiting local enforcement of immigration laws) compared to "non-sanctuary" cities. Lyons and colleagues suggest that the existence of "sanctuary" policies represent one dimension of the city-level concept of "immigrant political opportunities," which they define as "the political receptivity or vulnerability of cities to meeting the needs and demands of immigrant communities" (2013, p. 605). The authors suggest that immigrant political opportunities strengthen the inverse immigrant-crime relationship because there is greater communication and trust between immigrant communities and city institutions (Sandoval, 2009), an interpretation that closely mirrors the justifications proposed by supporters of "sanctuary" policies. We must note that Lyons et al.'s (2013) cross-sectional analysis suffers from limitations similar those discussed regarding Wong's (2017).

Lastly, Martínez-Schuldt and Martínez (n.d.) combine data from the FBI's Uniform Crime Reports, the U.S. Census, and the American Community Survey to examine the relationship between the implementation of limited cooperation policies and incidents of violent crime across 107 U.S. cities between 1990 and 2010. Martínez-Schuldt and Martínez conceptualize and operationalize limited cooperation policies in the same manner as Lyons et al. (2013) and Gonzalez et al. (2017). However, unlike previous approaches, they simultaneously examine variation in refined measures of violence (e.g., homicide and robbery) and utilize longitudinal multivariate regression techniques. Martínez-Schuldt and Martínez's results indicate that cities experience an initial decline in incidents of robbery following the passage of "sanctuary" ordinances, but no changes in homicide. Consistent with Lyons et al.'s (2013) expectations regarding "immigrant political opportunities," the authors also find that "sanctuary" polices strengthen the inverse relationship between immigrant concentration measures and both homicide and robbery.

The analyses presented by Martínez-Schuldt and Martínez too are not without limitations. For example, the authors rely on official measures of crime as opposed to measures that are less susceptible to reporting bias such as those from victimization surveys. Moreover, similar to previous research on limited cooperation policies and crime, Martínez-Schuldt and Martínez focus on general measures of violence instead of ethnoracial-specific measures. As such, they neglect the fact that ethnoracial minority groups are not equally affected by the structural conditions that foster crime and victimization (Peterson & Krivo, 2005). Future researchers may want to consider how, if at all, ethnoracial groups are differentially impacted by the passage of limited cooperation policies.

Ultimately, there is minimal empirical research on the "sanctuary" policies-crime relationship and the four existing studies yield mixed results. However, none of these studies suggest that limited cooperation policies foster crime. Instead, findings support the maintenance and expansion of limited cooperation policies, at least insofar as they foster positive immigrant community-police relations and other immigrant political opportunities. Moreover, as Lyons et al. (2013) suggest "By marginalizing newcomers, creating political cynicism, and instilling mistrust of the police and local authorities, hostile regimes may set in motion the very [criminogenic] processes they fear" (Lyons et al., 2013, p. 624).

6 | DISCUSSION

Findings from the robust literature on the immigration-crime link (Lee & Martinez, 2002; Lee, Martinez, & Rosenfeld, 2001; Martinez, Stowell, & Lee, 2010; Ousey & Kubrin, 2017; Rumbaut & Ewing, 2007; Sampson et al., 1997) as well as those from recent studies examining the relationships between limited cooperation policies and crime are clear (Lyons et al., 2013; Wong, 2017; Gonzalez et al., 2017; Martínez-Schuldt & Martínez, n.d.). Neither immigration nor limited cooperation policies are directly positively associated with higher rates of crime. Despite these consistent and reliable findings, many policymakers continue to question unauthorized migration and limited cooperation policies on the basis of public safety concerns.

This leads us to question the underlying nature of the public discourse surrounding limited cooperation policies, immigration, and crime. Overall, we contend that this discourse has less to do with crime and safety concerns and more with what Chavez (2008) describes as the "Latino Threat" narrative as well as public conceptualizations of what is means to be "real American." That is, the "rule of law" trope often deployed by opponents of limited cooperation policies serves as a color-blind, race-neutral guise for xenophobia and anti-immigrant sentiment, which are ultimately products of a larger racialized social system (Armenta, 2016; Armenta, 2017; Bonilla-Silva, 1997). This indirect and covert manifestation of racism becomes further entrenched and taken-for-granted precisely because certain segments of the population frame unauthorized migration as an assault on the "rule of law" or a "law and order" issue (Rumbaut & Ewing, 2007). We believe this helps explain why certain segments of the polity continue to recycle empirically inaccurate arguments that limited cooperation policies and noncitizens, more broadly, represent public safety concerns.

7 | CONCLUSION

In January of 2017, President Donald Trump signed an executive order entitled "Enhancing Public Safety in the Interior of the United States" (Executive Order 13768, 2017). The executive order targets so-called "sanctuary" jurisdictions by withholding federal grants, except for those funding law enforcement initiatives, until they fully cooperate with the federal government in the enforcement of immigration laws. Recent pieces of legislation proposed at the federal level have followed suite. Political pressure placed on the federal government to crackdown on limited cooperation policies is predicated upon the notion that localities with such ordinances threaten public safety by harboring "deportable" noncitizens. Nevertheless, this assertion is largely based on high-profile, isolated incidents of violent crime committed by deportable noncitizens rather than on empirical evidence. The current public and political discourse on "sanctuaries" inspired us to review the extant literature and pose three interrelated questions: 1) what is a "sanctuary" jurisdiction? 2) How and why have "sanctuary" policies evolved across time? And 3) what, if any, is the link between "sanctuary" policies and crime? In addressing these questions, our brief review of the existing academic literature on local "sanctuary" (i.e., limited cooperation) policies identifies several important findings.

First, although limited cooperation policies largely grew out of the 1980s Sanctuary Movement, these policies have evolved considerably as immigration enforcement has devolved from the federal to the local level. As such, contemporary "sanctuary" jurisdictions are broadly defined as places where state or local governments enact policies that limit local officials' cooperation with federal authorities in the enforcement of immigration laws. These policies broadly consist of "don't ask," "don't tell," and "don't enforce" measures (Kittrie, 2006).

Second, we find that relatively little empirical research examines the impact that local limited cooperation policies have on crime. This is concerning given the recent reliance on anecdotal evidence of violence committed by deportable noncitizens to call for the dismantling of limited cooperation policies through the withholding of federal funds. The studies we are aware of, conducted by Lyons et al. (2013), Gonzalez and colleagues (2017), Wong (2017), and Martínez-Schuldt and Martínez (n.d.) have yielded an inverse or null relationship between limited cooperation policies and crime. For the most part, it appears that jurisdictions with limited cooperation policies are either safer from crime or no different than jurisdictions without such policies (Wong, 2017; Gonzalez et al., 2017). Furthermore, limited cooperation policies may indirectly reduce crime by magnifying the crime-reducing effects of immigration (Martínez-Schuldt & Martínez, n.d.; Lyons et al., 2013; Ousey & Kubrin, 2017).

Our review of the literature suggests that existing evidence supports the expansion of limited cooperation policies. Criminological research has long established the importance of social organization and collective efficacy in reducing neighborhood crime (Sampson et al., 1997; Sampson & Graif, 2009). Ultimately, returning to the work of Lyons et al. (2013), it appears that limited cooperation policies expand immigrants' political opportunities and solidify community-official communication, which builds trust between communities and the officials that they represent and

serve. The result is that this "spiral of trust" strengthens formal and informal social control through community organization, thus reducing crime.

Finally, there exist several avenues for future research on so-called "sanctuaries" and crime. First, there is a clear need for additional longitudinal macro-level research that, 1) relies on more precise measures of crime (e.g., counts of specific types of crime), 2) examines intra-ethnoracial crime, 3) compares different units of analysis, 4) uses different approaches to operationalize "sanctuary" policies, and 5) considers the moderating effects of these policies and other key independent variables on crime. Second, there appears to be a notable dearth in individual-level research that examines the relationships between limited cooperation policies and their effects on, 1) criminal offending, including minor crime, 2) criminal victimization across types of jurisdictions, and 3) criminal victimization reporting. Continued research in these areas will significantly advance the current understanding of the limited cooperation policies-crime nexus and undoubtedly contribute to ongoing policy debates.

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ENDNOTES

[1] See *Miranda-Olivares v. Clackamas County* and *Galarza v. Szalcyk*.

[2] For example, see San Francisco's Due Process for All and Sanctuary 96-16 ordinance.

[3] Anti-Drug Abuse Act of 1988, Pub. L. No. 100-690, 102 Stat. 4181 (November 18, 1988).

[4] For a full list of city and county ordinances and administrative policies as well as state laws that limit compliance of local law enforcement with federal authorities, see Immigrant Legal Resource Center, "Detainer Policies," last updated on August 20, 2015. https://www.ilrc.org/detainer-policies

[5] See http://www.ailadownloads.org/advo/NILC-LocalLawsResolutionsAndPoliciesLimitingImmEnforcement.pdf.

[6] The constitutionality and legality of limited cooperation policies as well as immigration detainers are currently being debated in legal circles and in different circuit courts around the country, therefore it is imprecise to conclude that sanctuary jurisdictions across the United States willfully violate federal law.

ORCID

Daniel E. Martínez http://orcid.org/0000-0001-6773-4269

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Daniel E. Martínez is an Assistant Professor of Sociology at the University of Arizona. He is a co-principal investigator of the Migrant Border Crossing Study, which is a binational research project focusing on unauthorized migrants' border crossing, apprehension, and repatriation experiences. His research and teaching interests include criminology, juvenile delinquency, race and ethnicity, and unauthorized immigration. Martínez also does extensive research on migrant deaths along the US-Mexico border.

Ricardo Martínez-Schuldt is a doctoral candidate in the Department of Sociology at the University of North Carolina at Chapel Hill. His research focuses how sending-states, through consulate offices, facilitate the integration of migrants, as well as how local contexts impact the needs of immigrant populations and enable or constrain consular efficacy. In addition, he conducts research on neighborhood and city-level correlates of crime, with an emphasis on immigration-crime nexus.

Guillermo Cantor is the Deputy Director of Research at the American Immigration Council, where he leads the Council's research efforts and manages the research team. He has authored numerous publications on immigration policy and immigrant integration and regularly appears in English and Spanish-language media. He also currently teaches sociology of migration at Georgetown University. Cantor holds a PhD in Sociology from the University of Maryland, College Park.

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Policy Analysis

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Illegal Immigrant Incarceration Rates, 2010–2018

Demographics and Policy Implications

By Michelangelo Landgrave and Alex Nowrasteh

EXECUTIVE SUMMARY

President Trump has prioritized the arrest and deportation of illegal immigrants based largely on his argument that they are a significant and disproportionate source of crime in the United States. But is his argument supported by the facts? Illegal immigration and the crimes illegal immigrants commit are notoriously difficult to measure. This policy analysis is the latest paper in a series that attempts to answer that question by estimating illegal immigrant incarceration rates in the United States by using the American Community Survey Public Use Microdata Sample from the U.S. Census. This analysis goes beyond previous studies in the series as it updates our residual estimation method based on new research authored by University of California, Riverside, doctoral candidate Christian Gunadi and published in *Oxford Economic Papers*. Furthermore, we apply the updated methods to estimate the illegal immigrant incarceration rates in earlier years. Gunadi's new methods slightly increased the illegal immigrant crime rate relative to that of native-born Americans, but the illegal immigrant crime rate is still much lower than for that of native-born Americans. The states and federal government should collect better incarceration, conviction, and arrest data by immigration status so that we can more accurately understand how immigrants affect crime in the United States. Our estimates show that immigrants are less likely to be incarcerated than native-born Americans relative to their share of the population. Separately, legal immigrants and illegal immigrants each are less likely to be incarcerated than native-born Americans.

Michelangelo Landgrave is a political science doctoral candidate at the University of California, Riverside. Alex Nowrasteh is the director of immigration studies at the Cato Institute's Center for Global Liberty and Prosperity.

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> "This analysis goes beyond previous studies in the series as it updates our residual estimation method based on new research."

INTRODUCTION

President Trump has prioritized the arrest and deportation of illegal immigrants based largely on the perception that they are a significant and disproportionate source of crime in the United States.[1] According to a recent poll by the Public Religion Research Institute, 38 percent of Americans believe that immigrants increase crime in local communities; there's little doubt that many of the responders are specifically thinking of illegal immigrants when they answer affirmatively.[2] Is this perception accurate? This policy analysis is the latest paper in a series that attempts to answer that question by estimating illegal immigrant incarceration rates in the United States by using the American Community Survey (ACS) Public Use Microdata Sample (PUMS) from the U.S. Census. This analysis goes beyond previous studies in the series as it updates our residual estimation method based on new research authored by University of California, Riverside, doctoral candidate Christian Gunadi and published in *Oxford Economic Papers*.[3] We also apply the updated methods to estimate the illegal immigrant incarceration rates in earlier years. Gunadi's new methods slightly increased the illegal immigrant crime rate relative to that of native-born Americans, but the illegal immigrant crime rate is still much lower than it is for native-born Americans. The data show that all immigrants—legal and illegal—are less likely to be incarcerated than native-born Americans relative to their shares of the population. By themselves, illegal immigrants are less likely to be incarcerated than native-born Americans.

BACKGROUND

We published the first nationwide estimates of the incarcerated illegal immigrant population in 2017, followed quickly by updates in 2018 and 2019.[4] The 2017 brief analyzed incarceration rates for 2014, the 2018 brief analyzed incarceration rates for 2016, and the 2019 brief analyzed incarceration rates for 2017. The public interest in those briefs was so large that we updated the estimates using the most recent 2018 inmate data from the U.S. Census's ACS. Estimates of the total criminal immigrant population vary widely in other sources and according to different measures, but the illegal immigrant incarceration rate is an important indicator of their criminality.[5]

Previous empirical studies of immigrant criminality generally find that immigrants do not increase local crime rates, are less likely to cause crime than their native-born peers, and are less likely to be incarcerated than native-born Americans.[6] Illegal immigrant incarceration rates are not well studied; however, Cato Institute research based on data from the Texas Department of Public Safety found that, as a percentage of their respective populations, illegal immigrants had a criminal conviction rate about half that of native-born Americans in Texas in 2015 and 2017.[7] Recent peer-reviewed empirical studies have found no link between violent crime and illegal immigration and a negative relationship between the number of illegal immigrants and most types of nonviolent crime.[8] Our estimate of a low illegal immigrant incarceration rate is consistent with other research that finds that increasing immigration enforcement and deporting more illegal immigrants does not reduce the crime rate, which would occur if illegal immigrants were more crime prone than natives.[9]

METHODOLOGY

This policy analysis uses ACS data to estimate the incarceration rate and other demographic characteristics for immigrants aged 18–54 in 2018. ACS inmate data are reliable as they are ordinarily collected by or under the supervision of correctional institution administrators; however, the quality of the data for the population that includes the incarcerated was not always as reliable. The response rate for the group quarters population—a subpopulation who live in facilities that are owned and managed by others, which includes prisoners incarcerated in correctional facilities—was low in the 2000 census.[10] Recognizing the problem with data collection from the group quarters

population, the Census Bureau substantially resolved it in the 2010 census and the ACS, making several tweaks over the years that have continually improved the size and quality of the group quarters sample.[11]

The ACS counts the incarcerated population by their nativity and naturalization status, but local and state governments rarely record whether prisoners are illegal immigrants.[12] As a result, we have to use common statistical methods to identify incarcerated illegal immigrant prisoners by excluding prisoners with characteristics that illegal immigrants are unlikely to have.[13] In other words, we can identify likely illegal immigrants by looking at prisoners with individual characteristics highly correlated with being an illegal immigrant.

We changed our methods for identifying illegal immigrants in this analysis. In earlier versions of this policy analysis, we identified an illegal immigrant as one who falls under these criteria: the immigrant must have entered the country after 1982 (the cutoff date for the 1986 Reagan amnesty); cannot have been in the military; cannot be receiving Social Security or Railroad Retirement Income; cannot have been covered by Veteran Affairs or Indian Health Services; is not a citizen of the United States; is not living in a household where somebody receives food stamps (unless the immigrant's child, who may be eligible for food stamps if a U.S. citizen, is living with the immigrant); is not from the Democratic Republic of the Congo or Syria; was aged 59 years or younger upon arrival; and is not of Puerto Rican or Cuban origin if classified as Hispanic. Further, we omitted lawfully present migrants on Temporary Protected Status (TPS) by identifying them based on their birthplace, year of migration, and citizenship status. We imputed likely TPS recipient status to individuals from El Salvador (year of arrival 2001 or earlier); Honduras (1998 or earlier); Nicaragua (1998 or earlier); Haiti (2011 or earlier); Nepal (2015 or earlier); Somalia (2012 or earlier); Sudan (2013 or earlier); South Sudan (2016 or earlier); Syria (2016 or earlier); and Yemen (2017 or earlier).[14] Despite our efforts to count TPS holders as legal immigrants in those earlier briefs, some other legal immigrants whose answers were consistent with those given by illegal immigrants were counted as illegal immigrants despite their legal immigration statuses.

In this new analysis, we adjusted our identification method based on excellent recommendations by Christian Gunadi, which were published in a paper in *Oxford Economic Papers*.[15] Whereas earlier we imputed *illegal* immigrant status, Gunadi imputed *legal* immigrant status and identified those left over as illegal immigrants. His methods avoid the overestimation problem that we had in our earlier briefs and produce a more accurate result. Following Gunadi, we identified an immigrant as lawfully present if he or she met any of the following criteria: the immigrant arrived after 1980; is a U.S. citizen; received welfare benefits such as Social Security, Supplemental Security Income, Medicaid, Medicare, or military insurance; served in the Armed Forces; works for the government; resided in public housing or received rental subsidies or was the spouse of someone who resided in public housing or received rental subsidies; or was born in Cuba and has a spouse who is a legal immigrant or U.S. citizen. Gunadi also counted those who work in an occupation that requires some form of licensing as lawfully present, but the proliferation of licensing for illegal immigrants on the state level makes this step less useful, so we dropped it.[16] Otherwise, we used Gunadi's methods to identify legal immigrants and identified those who remained as likely to be illegal immigrants.

A limitation of the ACS data is that not all inmates in group quarters are in correctional facilities. Although most inmates in the public-use microdata version of the ACS are in correctional facilities, the data also include those in mental health and elderly care institutions and in institutions for people with disabilities.[17] These inclusions add ambiguity to our findings about the illegal immigrant population but not to our findings about the immigrant population as a whole, because the ACS releases macrodemographic snapshots of inmates in correctional facilities, which allows us to check our work.[18]

The above-mentioned ambiguity in illegal

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"Our estimate of a low illegal immigrant incarceration rate is consistent with other research."

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> “Controlling for the size of the population is essential for comparing relative incarceration rates between the native-born, illegal immigrant, and legal immigrant subpopulations.”

immigrant incarceration rates prompted us to narrow the age range to those who are aged 18–54. This range excludes most inmates in mental health and retirement facilities. Few prisoners are under age 18, many in mental health facilities are juveniles, and many of those over age 54 are in elderly care institutions. Additionally, few illegal immigrants are elderly, whereas those in elderly care institutions are typically over age 54.[19] As a result, narrowing the age range does not exclude many individuals from our analysis. We are more confident that our methods do not cut out many prisoners because winnowing the age range reduces their numbers in the 18–54 age range to about 4.5 percent above that of the ACS snapshot.[20] Natives in our results are those born as American citizens, and the group includes both those born in the United States and those born abroad to American parents.

Controlling for the size of the population is essential for comparing relative incarceration rates between the native-born, illegal immigrant, and legal immigrant subpopulations. Thus, we report the incarceration rate as the number of incarcerations per 100,000 members of that particular subpopulation, just as most government agencies do.[21]

INCARCERATIONS

An estimated 1,933,039 native-born Americans, 83,698 illegal immigrants, and 71,472 legal immigrants were incarcerated in 2018. The incarceration rate for native-born Americans was 1,477 per 100,000; 877 per 100,000 for illegal immigrants; and 380 per 100,000 for legal immigrants in 2018 (Figure 1). Illegal immigrants are 41 percent less likely to be incarcerated than native-born Americans. Legal immigrants are 74 percent less likely to be incarcerated than natives. If native-born Americans were incarcerated at the same rate as illegal immigrants, about 785,000 fewer natives would be incarcerated. Conversely, if natives were incarcerated at the same rate as legal immigrants, about 1.4 million fewer natives would be in adult correctional facilities.

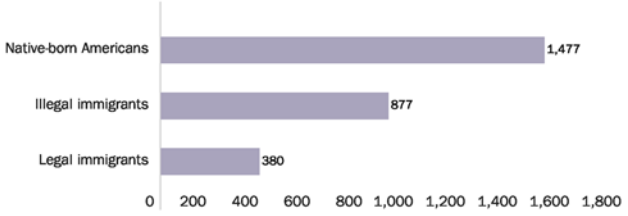
The ACS data include illegal immigrants incarcerated for immigration offenses and those in Immigration and Customs Enforcement's (ICE) detention facilities.[22] They are not incarcerated for violent or property crimes but only for immigration violations. If we were to remove the 42,188 people in ICE detention facilities on any given day, that would lower the illegal immigrant incarceration rate to 435 per 100,000—just 15 percent above the incarceration rate for legal immigrants.[23]

Robustness Checks for Counting the Illegal Immigrant Population

Because our chosen ACS variables could have affected the number of illegal immigrants we identified in the data, we altered some of the variables to see whether the results significantly changed. First, we included illegal immigrants who lived in households

Figure 1

Incarceration rates by immigration status, ages 18–54



Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

with users of means-tested welfare benefits. Illegal immigrants do not have access to those benefits, but U.S. citizens and some lawful permanent residents in their households do. This adjustment increased the illegal immigrant incarceration rate to 1,002 per 100,000, decreased the legal immigrant incarceration rate to 255 per 100,000, and did not affect the native incarceration rate.

Our second robustness check excluded all immigrants who entered the United States after 2009. Immigrants on lawful permanent residency can apply for citizenship after five years, guaranteeing that most of the lawful permanent residents who are able to naturalize have done so, which decreases the pool of potential illegal immigrants in our sample. This robustness check shrinks the size of the nonincarcerated illegal immigrant subpopulation relative to those incarcerated and, thus, slightly raises the rate of illegal immigrant incarceration to about 1,061 per 100,000. These variable changes did not alter our results enough to undermine confidence in the findings.

ILLEGAL IMMIGRANT INCARCERATION RATES OVER TIME, 2010–2018

This policy analysis uses a slightly different method than earlier studies to identify illegal immigrants. As a result, we decided to apply our new methods to previous years to give a sense of how the illegal immigrant incarceration rate has changed over time (Figure 2). In every year, the illegal immigrant incarceration rate is between 26 percent and 41 percent below that of native-born Americans. In every year, the legal immigrant incarceration rate is between 66 percent and 75 percent below that of native-born Americans. Furthermore, the incarceration rate has declined for every group. From 2010 to 2018, the native-born incarceration rate fell by 7 percent, the legal immigrant incarceration rate fell by 22 percent, and the illegal immigrant incarceration rate fell by 15 percent.[24]

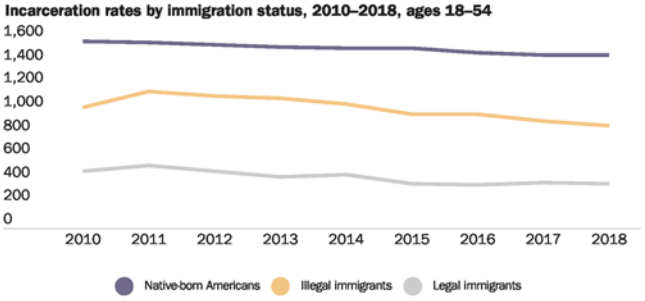
Subtracting the number of illegal immigrants in ICE detention so as to focus on more serious criminals who are likelier to pose an actual danger to public safety shows that the illegal immigrant incarceration rate is closer to that of legal immigrants in those years. From 2010 through 2018, illegal immigrants in ICE detention facilities accounted for 49–89 percent of the higher illegal immigrant incarceration rate *relative* to legal immigrants. In other words, the main difference between the legal and illegal immigrant incarceration rates is that illegal immigrants are more likely to be incarcerated for violating immigration law.

Compared to the earlier versions, our current analysis revises the illegal immigrant

> “The main difference between the legal and illegal immigrant incarceration rates is that illegal immigrants are more likely to be incarcerated for violating immigration law.”

Figure 2

Incarceration rates by immigration status, 2010–2018, ages 18–54



Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

> "The incarceration rate for all illegal immigrants is lower than the incarceration rate for native-born white Americans."

incarceration rate upward by 24 percent, 21 percent, and 20 percent for the years 2014, 2016, and 2017, respectively (Figure 3).[25] These new revised illegal immigrant incarceration rates are still well below those for native-born Americans. Figure 3 shows that the illegal immigrant incarceration rate in 2014, 2016, and 2017 was below that of native-born Americans by 31 percent, 36 percent, and 38 percent, respectively. That is a smaller difference than the 44 percent, 47 percent, and 49 percent that we reported in the previous respective years.

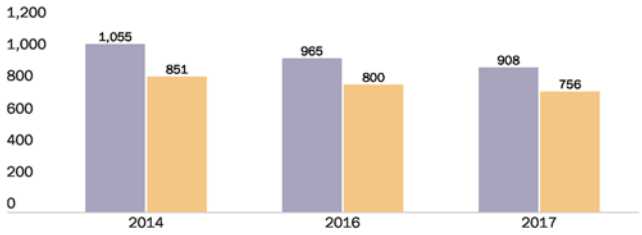
Demographic and Social Characteristics

Incarceration rates vary widely by race and ethnicity in the United States, even within each immigrant category (Table 1). By race and ethnicity, legal and illegal immigrants have a lower incarceration rate than native-born Americans of the same race or ethnicity. The incarceration rate for all illegal immigrants is lower than the incarceration rate for native-born white Americans.

Immigrants from certain parts of the world are more likely to be incarcerated than others (Table 2). Of all legal immigrants, those from other countries have the highest incarceration rate. The "other" category is composed of those individuals whose country of birth is unknown or those individuals who were born at sea. Again, illegal immigrants from other countries have the highest incarceration rate of any group, followed by those from Latin America, who are most likely to be incarcerated for immigration offenses and held in ICE detention facilities than immigrants from any other

Figure 3

Illegal immigrant incarceration rates by estimation method, ages 18–54



Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

Table 1

Incarceration rates by race, ethnicity, and immigration status, ages 18–54

	Native-born Americans	Legal immigrants	Illegal immigrants	All
White	917	179	477	887
Black	3,872	476	1,037	3,473
Asian	429	153	161	234
Hispanic (any race)	1,701	567	1,241	1,335
Other	2,206	594	1,104	2,046
All	1,477	380	877	1,311

Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

region of the world. Across all broad groups, those born in other countries have the highest incarceration rates followed by those born in the United States. About 73 percent of all immigrants in the United States come from the top 20 countries of origin for the foreign-born population.[26] Of those, illegal immigrants from Honduras and legal immigrants from Cuba have the highest incarceration rates (Table 3). The higher incarceration rates for illegal immigrants from Mexico, El Salvador, Guatemala, and Honduras are likely exacerbated by the significant number of them who are detained for immigration offenses.

The distribution of prisoners by their immigration status and region of origin shows that 6.16 percent of all prisoners are from Latin America, whereas 92 percent were born in the United States (Table 4).

About 88.1 percent of all prisoners are men, whereas only 11.9 percent are women (Table 5). Legal and illegal immigrant women make up a smaller proportion of their respective prisoner populations than native-born women, while men make up a higher proportion. The sex distribution of legal immigrant prisoners is far closer to that of native-born Americans than to that of illegal immigrants.

Prisoners in every subpopulation are less educated than their total subpopulation (Table 6). About 63.7 percent of all native-born adults, including those not incarcerated, have some college education or above, whereas 18.6 percent of native-born prisoners have the same level of education. A total of 23.6 percent of legal immigrant prisoners and 14.4 percent of illegal immigrant prisoners have some college education or above, percentages that are lower than the percentages of their subpopulations with the same level of education (53.9 percent and 42.8 percent, respectively).[27] Those in every immigration category who are highly educated tend to avoid incarceration.

Native-born Americans and illegal immigrants have higher incarceration rates when they are young (Table 7). The peak incarceration rate for native-born Americans and illegal immigrants is between ages 30 and 34. The legal immigrant incarceration rate peaks between ages 25 and 29.

The incarceration rates for legal and illegal immigrants generally increase with the

> “The higher incarceration rates for illegal immigrants from Mexico, El Salvador, Guatemala, and Honduras are likely exacerbated by the significant number of them who are detained for immigration offenses.”

Table 2

Incarceration rates by region of birth, ages 18–54

Region of birth	Native-born Americans	Legal immigrants	Illegal immigrants	All
United States	1,489	N/A	N/A	1,489
Other North America	478	403	692	482
Latin America	791	548	1,261	820
Europe	632	242	304	339
East Asia	595	186	255	229
Indian Subcontinent	667	105	93	109
Middle East	1,026	77	489	245
Other Asia	-	459	-	311
Africa	214	343	629	398
Oceania	1,700	239	458	544
Other	-	2,501	1,980	1,705
All	1,478	380	877	1,312

Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation; N/A = not applicable.

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> “Related to the amount of time immigrants have spent in the United States, illegal and legal immigrants who immigrate at a younger age are more likely to be incarcerated.”

Table 3
Incarceration rates by country of birth, ages 18–54

Country	Legal	Illegal
Mexico	589	1,301
China	144	148
India	51	94
Philippines	169	144
El Salvador	686	879
Vietnam	139	1,217
Cuba	857	N/A
Dominican Republic	518	1,555
Korea	168	72
Guatemala	674	1,176
Canada	355	708
Colombia	212	610
Jamaica	799	2,819
United Kingdom	294	232
Haiti	282	918
Honduras	786	2,363
Germany	444	559
Peru	47	355
Ecuador	348	550
Poland	265	432

Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation; N/A = not applicable.

amount of time they have spent in the United States, the major exception being the higher legal immigrant incarceration rate in the 0–4 years category that then falls once legal immigrants have been here for 5–9 years (Table 8). A possible reason for this phenomenon is that legal immigrants who are criminally inclined rapidly run afoul of the law, serve short prison sentences, and are removed from the United States quickly enough that the incarceration rate for the 5–9 years of residency category declines greatly.

Related to the amount of time immigrants have spent in the United States, illegal and legal immigrants who immigrate at a younger age are more likely to be incarcerated (Table 9). Illegal immigrants who arrive between ages 0 and 17 are about twice as likely to be incarcerated than those who arrive after age 17, suggesting that illegal immigrants who were old enough to choose to come here illegally are more law-abiding than those who were brought here as minors.

The pattern is even more pronounced for legal immigrants. Those who immigrated between the ages of 0 and 17 were more than twice as likely to be incarcerated than legal immigrants who came at later ages. This again suggests that those old enough to choose to come to the United States legally are more law-abiding.

At least two nonmutually exclusive theories can explain why those who entered in their youth have higher incarceration rates. First, spending part of one's childhood in the United States assimilates many immigrants to our high-crime culture. A second theory is that those who decide to come here have some systematically different characteristics that make them less likely to commit crimes, whereas those who are too young to make the decision to immigrate do not.

POLICY IMPLICATIONS

The president has prioritized the arrest and deportation of illegal immigrants because much of the public believes that they are a significant and disproportionate source of crime in the United States.[28] For immigration in general, Gallup asked Americans whether immigration has worsened the crime problem in the United States. Some 42 percent of respondents said that immigration worsened the crime situation, 7 percent said that immigration alleviated the crime situation, and 50 percent said it had no effect.[29] According to Transatlantic Trends survey data, slightly less than half of Americans believe that immigrants increase crime in the United States.[30] Of those who believe that immigrants increase crime, about 60 percent believe that illegal immigrants are primarily responsible.[31]

Although substantial percentages of the American public believe that immigration increases crime and that illegal immigrants

Table 4
Percentage of all prisoners by region of birth, ages 18–54

Region of birth	Native-born Americans (%)	Legal immigrants (%)	Illegal immigrants (%)	All (%)
United States	99.37	N/A	N/A	92.02
Other North America	0.02	1.64	0.92	0.11
Latin America	0.20	71.75	87.77	6.16
Europe	0.22	6.33	2.16	0.51
East Asia	0.11	9.53	3.68	0.57
Indian Subcontinent	0.02	2.52	1.33	0.15
Middle East	0.03	0.53	0.88	0.08
Other Asia	0.00	0.29	0.00	0.01
Africa	0.01	6.16	2.77	0.33
Oceania	0.03	0.31	0.34	0.05
Other	0.00	0.12	0.05	0.01

Source: Authors' analysis of the American Community Survey data.
Note: N/A = not applicable.

Table 5
Percentage of prisoners by sex and nativity, ages 18–54

Sex	Native-born Americans (%)	Legal immigrants (%)	Illegal immigrants (%)	All (%)
Female	12.3	11.7	4.5	11.9
Male	87.7	88.3	95.5	88.1

Source: Authors' analysis of the American Community Survey data.

Table 6
Percentage of prisoners by education and nativity, ages 18–54

Education	Native-born Americans (%)	Legal immigrants (%)	Illegal immigrants (%)	All (%)
Less than high school	28.9	39.2	50.3	30.1
High school graduate	52.6	37.1	35.3	51.4
Some college	15.7	17.1	10.4	15.5
College graduate	2.3	4.8	3.3	2.4
Postgraduate	0.6	1.7	0.7	0.6

Source: Authors' analysis of the American Community Survey data.

disproportionately contribute to the problem, the evidence is that they *decrease* incarceration rates in the United States. Immigrants accomplish this because they are less crime prone than native-born Americans. The addition of a less crime-prone subpopulation to the United States mechanically reduces the overall incarceration rate in the country. The facts uncovered in this policy analysis should point the government toward other immigration policies that would actually reduce crime.

For instance, federal officials should

"The addition of a less crime-prone subpopulation to the United States mechanically reduces the overall incarceration rate in the country."

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> “Illegal immigrants have a lower incarceration rate than native-born Americans, so scarce law enforcement resources should not be spent on identifying and deporting a subpopulation with such a low crime rate.”

Table 7

Incarceration rates by age and immigration status, ages 18–54

Age (years)	Native-born Americans	Legal immigrants	Illegal immigrants	All
18–24	1,057	357	883	1,012
25–29	1,721	583	963	1,588
30–34	1,917	508	1,010	1,687
35–39	1,805	421	1,001	1,554
40–44	1,585	329	790	1,323
45–49	1,262	295	704	1,070
50–54	1,202	282	601	1,033

Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

Table 8

Incarceration rates for immigrants by their time in the United States and immigration status, ages 18–54

Time in the United States (years)	Legal immigrants	Illegal immigrants
0–4	360	691
5–9	155	676
10–14	282	861
15–19	365	1,047
20–24	509	1,279
25–29	399	977
30–34	490	1,959
35–39	683	2,296
40+	517	0

Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

Table 9

Incarceration rates for immigrants by their age of arrival in the United States and immigration status, ages 0–54

Age (years)	Legal immigrants	Illegal immigrants
0–17	592	1,491
18–24	388	1,034
25–29	209	559
30–34	157	638
35–39	209	611
40–44	158	445
45–49	367	535
50–54	138	1,088

Source: Authors' analysis of the American Community Survey data.
Note: Rates are per 100,000 residents in each subpopulation.

abandon efforts to convince so-called sanctuary cities to cooperate with federal immigration officials because such cooperation will not lower violent and property crime rates nationwide. Illegal immigrants have a lower incarceration rate than native-born Americans, so scarce law enforcement resources should not be spent on identifying and deporting a subpopulation with such a low crime rate. If the purpose of law enforcement is to deter crime and to punish criminals, their resources would be inefficiently allocated if targeted at illegal immigrants.

Second, the federal government already has effective programs to identify illegal immigrant criminals who have been arrested, convicted, or incarcerated. The federal government should continue those policies and make the removal of illegal immigrant criminals a priority but should not widen their reach to include illegal immigrants who have not committed criminal offenses or have not otherwise put Americans at risk.[32]

Third, the government should collect better data on illegal and legal immigrant criminality. Incarceration rates are just one measurement of criminality to fully understand relative crime rates in the United States. Unfortunately, the paucity of data means that we must estimate the number of illegal immigrants who are incarcerated, which adds some uncertainty to our final numbers. Every state should collect and make available data on the immigration statuses of those convicted and arrested for crimes, just like Texas does, as well as those who are incarcerated.[33] To be clear, this proposal would only require documenting the immigration status of people who are arrested for crimes, convicted of crimes, or incarcerated for crimes. There is no excuse for paucity of data on this important public policy issue.

CONCLUSION

Legal and illegal immigrants were less likely to be incarcerated than native-born Americans in 2018, just like in 2014, 2016, and 2017.[34] Under the Gunadi methods that we employ here, the illegal immigrant incarceration rate rises somewhat relative to the incarceration rate for native-born Americas, but the gap is still huge for the period of 2010–2018.

Those incarcerated do not represent the total number of immigrants who can be deported under current law or the complete number of convicted immigrant criminals who are in the United States but merely those who are incarcerated. The younger the immigrants are upon their arrival in the United States and the longer that they are here, the more likely they are to be incarcerated as adults. This analysis provides numbers and demographic characteristics to better inform the public policy debate over immigration and crime. Lastly, we recommend that governments at all levels in the United States focus on collecting better data so that we can more precisely understand how illegal immigrants and legal immigrants contribute to crime in the United States.

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Illegal Immigration and Crime in Texas

By Alex Nowrasteh, Andrew C. Forrester, and Michelangelo Landgrave

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Illegal Immigrant Crime in Texas*

Alex Nowrasteh
Cato Institute

Andrew C. Forrester
Cato Institute

Michelangelo Landgrave
UC Riverside

This Version: October 13, 2020

*Alex Nowrasteh is the director of immigration studies at the Cato Institute. Andrew C. Forrester is a research associate at the Cato Institute's Center for Global Liberty and Prosperity. Michelangelo Landgrave is a political science doctoral candidate at the University of California, Riverside

1 Introduction

Donald J. Trump launched his candidacy for the Republican presidential nomination in June 2015 by comments on illegal immigrants and the crime they commit in the United States. "When Mexico sends its people, they're not sending their best. They're not sending you. They're not sending you," he said. "They're sending people that have lots of problems and they're bringing those problems with us. They're bringing drugs, they're bringing crime, they're rapists, and some, I assume, are good people."[1] A few weeks after Trump's announcement, 32-year-old Kate Steinle was shot and killed by an illegal immigrant José Inez García Zárate in San Francisco, California. Although Zárate was later acquitted of all murder and manslaughter charges due to mistakes made by the prosecutor, his shooting of Steinle seemed to support Trump's worry about illegal immigrants causing a crime spree and helped win him the election in 2016.

As tragic as the shooting and death of Kate Steinle was, it was one of the 13,455 murders that year in the United States and it does not tell us how many of those victims were murdered by illegal immigrants.[2] The most important measure that matters when judging the crime rates of illegal immigrants is how likely they are to be criminals compared to other sub-populations. If illegal immigrants are more likely to be criminals then their presence in the United States would raise crime rates, supporting Trump's assertions. But if illegal immigrants are less likely to commit crime then they would lower the nationwide crime rate.

Politically, this debate spills over to evaluating whether domestic immigration enforcement policies reduce crime. Illegal immigrant crime is also central to the debate over sanctuary jurisdictions that refuse to turn over many illegal immigrants to Immigration and Customs Enforcement, the effects of a border wall, and whether Border Patrol requires more resources to counter crime along the border. Answering whether illegal immigrants are particularly crime prone is essential to addressing these concerns and setting efficient anti-crime policies.

As important as this question is, there is little data available about illegal immigrant criminality to answer this question. Most state governments do not record the immigration

1. Adam B. Lerner, The 10 Best Lines from Donald Trump's Announcement Speech, June 2015, https://www.politico.com/story/2015/06/donald-trump-2016-announcement-10-best-lines-119066.

2. Federal Bureau of Investigation, Expanded Homicide Data Table 1: Murder Victims by Race, Ethnicity, and Sex, 2015, Uniform Crime Reporting (UCR) Program. Accessed April 9, 2020, https://ucr.fbi.gov/crime-in-the-u.s/2015/crime-in-the-u.s.-2015/tables/expanded_homicide_data_table_1_murder_victims_by_race_ethnicity_and_sex_2015.xls.

statuses of those who are convicted of crimes and federal census data on the incarcerated population do not identify illegal immigrants. However, the little evidence that does exist shows that illegal immigrants have a crime rate far below that of native-born Americans, but higher than legal immigrants. This chapter will present the two most important pieces of evidence. The first compelling set of evidence is the relative conviction and arrest rates in the state of Texas, which keeps arrest and conviction data for the number of illegal immigrants by crime. The second is the estimates of the nationwide incarceration rates by immigration status, which are consistent with the Texas conviction rates.

2 Illegal Immigration and Crime in Texas[3]

Texas is the only state that records criminal convictions and arrests by immigration status.[4] The Texas Department of Public Safety (DPS) has these data because its law enforcement agencies cooperate with federal immigration enforcement authorities at the Department of Homeland Security (DHS) by checking the biometric information of all arrestees in the state and tracking them through to their convictions.[5] Unlike other states, the Texas DPS keeps the results of these DHS checks and we acquired them through a Public Information Act request.[6]

The Texas DPS data quality is excellent and, if it errs, it is likely to overcount the convictions and arrests of illegal immigrants because it counts more total arrests than the other publicly available DPS source.[7] It's particularly fortuitous that Texas keeps these data because it borders Mexico, it has the second-largest illegal immigrant population of any state, it is a politically conservative state governed by Republicans, in 2017 it did not have jurisdictions that limited its cooperation with federal immigration enforcement, and it has a law-and-order reputation for severely and strictly enforcing its criminal laws.[8]

3. Much of the research and the methods from this section comes from these briefs: Alex Nowrasteh, Criminal Immigrants in Texas in 2017: Illegal Immigrant Conviction Rates and Arrest Rates for Homicide, Sex Crimes, Larceny, and Other Crimes, Cato Institute Immigration Research and Policy Brief, no. 13, August 27, 2019, August 2019, https://www.cato.org/sites/cato.org/files/pubs/pdf/irpb13_edit.pdf; Alex Nowrasteh, Criminal Immigrants in Texas: Illegal Immigrant Conviction and Arrest Rates for Homicide, Sex Crimes, Larceny, and Other Crimes, Cato Institute Immigration Research and Policy Brief, no. 4, February 26, 2018, February 2018, https://www.cato.org/sites/cato.org/files/pubs/pdf/irpb-4-updated.pdf.

4. Nowrasteh, *Criminal Immigrants in Texas in 2017: Illegal Immigrant Conviction Rates and Arrest Rates for Homicide, Sex Crimes, Larceny, and Other Crimes*.

5. Ibid.

6. "Public Information Act," Texas Department of Public Safety; "Texas Criminal Illegal Alien Data," Texas Department of Public Safety. These data sets are available by email request to Alex Nowrasteh at anowrasteh@cato.org

7. Nowrasteh, *Criminal Immigrants in Texas in 2017: Illegal Immigrant Conviction Rates and Arrest Rates for Homicide, Sex Crimes, Larceny, and Other Crimes*.

8. Ibid.

Controlling for the size of the population is essential to comparing relative conviction and arrest rates between sub-populations in the state of Texas. This means that the total number of native-born Americans, legal immigrants, and illegal immigrants living in Texas is just as important as the number of convictions in calculating their respective crime rates. However, the government doesn't record immigration status in the American Community Survey (ACS), so social scientists estimate the number of illegal and legal immigrants using the residual method. We adopt a residual method proposed by Christian Gunadi in a paper published in *Oxford Economic Papers*.[9]

Gunadi imputed legal immigrant status and identified those left over as illegal immigrants, which is different from other residual methods that identify illegal immigrants first and then count the left-over people as legal immigrants. Using Gunadi's methods, a person is counted as a legal immigrants if he or she met any of the following criteria as recorded in the 2018 ACS: the immigrant arrived after 1980; is a U.S. citizen; received welfare benefits such as Social Security, Supplemental Security Income, Medicaid, Medicare, or military insurance; served in the Armed Forces; works for the government; resided in public housing or received rental subsidies or was the spouse of someone who resided in public housing or received rental subsidies; or was born in Cuba and has a spouse who is a legal immigrant or U.S. citizen. Gunadi also considers occupational licenses, but the method used here drops that filter because so many states issue licenses to illegal immigrants.[10] The number of legal immigrants estimated from this method includes those residing in Texas on temporary non-immigrant work visas and those who have naturalized and earned American citizenship.

There were 28,701,845 people living in Texas in 2018. According to the results of the Gunadi residual method (minus the questions on occupational licensure), there were 23,767,658 native-born American, 3,077,766 legal immigrants, and 1,856,421 illegal immigrants living in Texas. They represented 82.8 percent of the population, 10.7 percent, and 6.5 percent, respectively. Likewise, there were 337,996 criminal convictions of native-born Americans in Texas that year, 16,470 convictions of legal immigrants, and 14,526 convictions of illegal immigrants.

Figure 1 shows that the illegal immigrant criminal conviction rate was 782 per 100,000 illegal immigrants, 535 per 100,000 legal immigrants, and 1,422 per 100,000 native-born Ameri-

9. Christian Gunadi, On the Association Between Undocumented Immigration and Crime in the United States, *Oxford Economic Papers*, September 2019,

10. Michelangelo Landgrave and Alex Nowrasteh, Illegal Immigrant Incarceration Rates, 2010–2018: Demographics and Policy Implications, Policy Analysis No. 890, Cato Institute, Washington, DC, April 21, April 2020.

cans in Texas in 2018. The illegal immigrant criminal conviction rate was 45 percent below that of native-born Americans in Texas. The legal immigrant criminal conviction rate was 62 percent below that of native-born Americans. Figure 1 shows the rates for the number of convictions and not the number of individuals convicted. The conviction rates for individuals are slightly lower for every group than the rate of the number of convictions, but the ratio is roughly the same with illegal immigrants at 743, legal immigrants at 495, and native-born Americans at 1,304 per 100,000 of their respective subpopulations.

[INSERT FIGURE 1 ABOUT HERE]

That pattern also holds for other more serious crimes like homicide, the most serious criminal offense. In Texas, there were 867 convictions for homicide in 2018. Of those, native-born American were convicted 780 times, illegal immigrants 46 times, and legal immigrants 41 times. Figure 2 shows that the illegal immigrant homicide conviction rate was 2.5 per 100,000 illegal immigrants, 1.3 per 100,000 legal immigrants, and 3.3 per 100,000 native-born Americans in Texas in 2018. Thus, illegal immigrants had a homicide conviction rate 25 percent below that of native-born Americans in Texas. The legal immigrant homicide conviction rate was 59 percent below that of native-born Americans. Once again, native-born Americans were the most likely to be convicted followed by illegal immigrants with legal immigrants as the least-likely to be convicted of homicide.

[INSERT FIGURE 2 ABOUT HERE]

The illegal immigrant conviction rate for all violent crime is about 38 percent below that of native-born Americans and the legal immigrant conviction rate is about 65 percent below (Figure 3). Illegal and legal immigrants are substantially less likely to be convicted of property crimes than native-born Americans. Illegal immigrants have a conviction rate 74 percent below that of native-born Americans for property crimes while it is 71 percent below for legal immigrants (Figure 4). The low illegal immigrant property crime rate can probably be explained by their high rate of employment, especially among men, relative to both legal immigrant and native-born Americans men.[11] In other words, the opportunity cost for property crimes is higher for illegal immigrant men because they are more likely to be gainfully employed.

11. George J. Borjas, The Labor Supply of Undocumented Immigrants, *Labour Economics* 46 (June 2017): 1–13.

[INSERT FIGURE 3 ABOUT HERE]

[INSERT FIGURE 4 ABOUT HERE]

The illegal immigrant sex crime conviction rate, which includes sexual assault and other sexual offenses, is the closest to native-born Americans. Figure 5 shows that the illegal immigrant conviction rate for sex crimes is 24.4 per 100,000 illegal immigrants, which is only 3 percent below the sex crime conviction rate for native-born Americans at 25.1 per 100,000 natives. Legal immigrants have the lowest sex crime conviction rate at 14.7 per 100,000 legal immigrants, more than 58 percent below that of native-born Americans.

[INSERT FIGURE 5 ABOUT HERE]

The results for arrests are similar. Texas police made 907,767 criminal arrests in 2018. Legal immigrants accounted for 5.3 percent of all those arrested and illegal immigrants accounted for 4.4 percent, well below their respective 10.7 and 6.5 percent shares of the population. For all arrests in Texas, there were about 2,142 arrests of illegal immigrants for every 100,000 illegal immigrants in the state compared to 1,570 for legal immigrants and 3,449 for native-born Americans. By comparison, the illegal immigrant arrest rate was 38 percent below that of native-born Americans and the legal immigrant arrest rate was 55 percent below (Figure 6).

For every conviction of an illegal immigrant, there were 2.7 arrests compared to 2.9 arrests for every conviction of a legal immigrant and 2.4 arrests for every conviction of a native-born American. Looking at the number of individuals arrested relative to the number of individuals convicted reveals a similar pattern of 2.7 illegal immigrants arrested for every conviction, 2.8 legal immigrants arrested for every conviction, and 2.3 native-born Americans arrested for every conviction (Figure 7). Thus, illegal and legal immigrants who are arrested are less likely to be convicted than native-born Americans.

[INSERT FIGURE 6 ABOUT HERE]

[INSERT FIGURE 7 ABOUT HERE]

There are several potential explanations for this. First, police could have a bias against legal and illegal immigrants, but the rest of the justice system does not and is less likely not

to pursue further legal action that would lead to a conviction. Second, immigrants could have better legal representation. Third, many illegal immigrants arrested by the police are deported before they can be convicted. However, that doesn't account for the even higher ratio for legal immigrants who must be convicted in order to be removed. Fourth, immigrants could be more likely to be arrested but also more likely to skip bail and not be convicted. The last possibility is the most serious. After all, about 65 percent of illegal immigrants in Texas are from Mexico so it would be relatively cheap for those released on bail to escape.

However, all Texas police jurisdictions in 2018 cooperate with federal immigration authorities and they are supposed to turn over arrested or convicted illegal immigrants to federal law enforcement for deportation.[12] In 2018, Texas police arrested 36,970 illegal immigrants and they turned over 34,949 illegal immigrants to Immigration and Customs Enforcement (ICE) for eventual deportation.[13] The small difference between those arrested and those turned over to ICE, the latter which also includes illegal immigrants being released from state prison, does not allow much leakage. There simply cannot be many illegal immigrants who are arrested who then escape or who are deported without being convicted.

Arrest rates for specific crimes are closely related to the conviction rates. The illegal immigrant homicide arrest rate is 28.8 percent below that of native-born Americans. The legal immigrant homicide arrest rate is 61.2 percent below that of native-born Americans (Figure 8). The illegal immigrant property and violent crime arrest rates are uniformly below those of native-born Americans (Figure 9). The illegal immigrant violent crime arrest rate is slightly higher than for legal immigrants, but the illegal immigrant property crime arrest rate is lower than it is from legal immigrants. The sex crime arrest rate for illegal immigrants is 24 percent below that of native-born Americans and 47 percent below that of legal immigrants (Figure 10).

[INSERT FIGURE 8 ABOUT HERE]

[INSERT FIGURE 9 ABOUT HERE]

[INSERT FIGURE 10 ABOUT HERE]

12. Bryan Griffith and Jessica M. Vaughan, Map 1: Sanctuary Cities, Countries, and States, Center for Immigration Studies. Updated March 23, 2020, https://cis.org/Map-Sanctuary-Cities-Counties-and-States.

13. TRAC Immigration, Latest Data: Immigration and Customs Enforcement Detainers, Syracuse University. Accessed April 9, 2020, https://trac.syr.edu/phptools/immigration/detain/.

The criminal conviction and arrest rates above are not historical aberrations and the year 2018 was not cherry-picked to show positive results. For every year for which full data are available and there are enough ACS data to use Gunadi's residual method to estimate the number of illegal immigrants residing in Texas, they have a lower criminal conviction rate (Figure 11). From 2012 through the end of 2018, illegal immigrants in Texas had an average criminal conviction rate 42 percent below that of native-born Americans. Legal immigrants had a criminal conviction rate 63 percent below that of native-born Americans during the same period. Arrest rates are also lower for illegal immigrants during the entire period, but lowest of all for legal immigrants (Figure 12). On average from 2012 through 2018, illegal immigrants in Texas were 40 percent less likely to be arrested than native-born Americans and legal immigrants were 55 percent less likely.

[INSERT FIGURE 11 ABOUT HERE]

[INSERT FIGURE 12 ABOUT HERE]

By country of origin, Mexicans comprise the largest group of illegal immigrants in Texas in 2018 at 65 percent. According to Texas DPS data on the nationality of illegal immigrants by country of origin, Mexicans are 65 percent of those convicted of crimes in 2018 — exactly proportional to their share of the population. Unfortunately, the Gunadi method is not robust enough to identify countries of origin for other illegal immigrant populations in Texas as they are too small.

3 Texas Crime Elasticities by County

Illegal immigrant criminal conviction and arrest rates are low throughout the state of Texas, but there could be substantial local variation whereby illegal immigrant crime is higher in counties where many illegal immigrants reside. Thus, this section examines how a change in the illegal immigrant population affects crime on the county level in Texas for the years 2012-2018. The relationship between changes in the illegal immigrant population and crime is known as an elasticity.

The elasticity between two variables estimates how one variable, the illegal immigrant population here, affects another variable like the number of illegal immigrant convictions. For

good measure, we also sought to compare the relationship between the illegal immigrant population on the county level and overall index crimes (murder, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft) as collected by the Texas DPS.[14] We also included a control for the number of law enforcement officers per capita. The results of these regressions are merely correlative and not causal, but it's reasonable to assume a relationship between the number of crimes committed by illegal immigrants, crimes overall, and the illegal immigrant population for the purposes of these regressions.[15]

We find no statistically significant relationship between an increase in a county's illegal immigrant population and the number of illegal immigrant convictions and the overall number of convictions with one exception (Figure 13). For each regression in Table 13, we find statistically insignificant negative point estimates showing that a 10 percent increase in Texas counties' population share of illegal immigrants is correlated with decreases of 0.07 percent for all convictions and 0.05 percent for convictions of illegal immigrants. In each model, the only significant predictor of conviction rates is county population, which suggests that the total population of a county is a more important predictor of criminal convictions than the share of illegal immigrant. Surprise, more people means more crime.

[INSERT FIGURE 13 HERE]

Figure 13 also breaks down the conviction rates into three subcomponents: murder, all violent crime, and all property crime. The only statistically significant relationship, which is the exception mentioned above, is a negative association between total violent crime convictions and the illegal immigrant share with a point estimate of -0.104 that is significant at the 5 percent level. This exception suggests that a 10 percent increase in the illegal immigrants share of the population is associated with a 1 percent decline in violent crime convictions in our sample of Texas counties.

14. Texas Department of Public Safety answer to Nowrasteh's public information act request.

15. To empirically test the correlation between illegal immigrant conviction rates and the illegal immigrant population, we run the simple regression:

$$\ln(Y_{it}) = \mu + \alpha_i + \lambda_t + \gamma \ln(Illegal_{it}) + \eta \ln(Population_{it}) + \beta \ln(PoliceRate_{it-1}) + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the number of convictions in county i in year t. $Illegal_{it}$ represents the estimated illegal population and $Population_{it}$ is the total county population from the ACS. α_i and λ_t denote county and year fixed effects, respectively. In line with the economics and criminology literature we also control for the number of police officers per 100 population. Our coefficient of interest γ represents the estimated elasticity of the conviction rate with respect to the county population share of illegal immigrants. Since we are limited to 38 identified counties in the raw ACS microdata, we compute wild cluster bootstrap test statistics as described in Cameron, Gelbach, and Miller (2008).

One of our concerns with the regressions in Table 13 is that they are based on the illegal immigrant share of the population in just 38 identified Texas counties.[16] We followed the same Gunadi methods mentioned earlier in this chapter, including the exception for licensing, to estimate the number of illegal immigrants — but the method only works when there is a large population in the counties. Texas has 254 counties, so the 38 for which we can apply Gunadi's methods does give us some pause, but those counties also include most of Texas' population. Because of the small number of counties, we compute wild cluster bootstrap test statistics instead of normal standard errors.[17]

Taken together, these results suggest that higher illegal immigrant population shares have little to no relationship with illegal immigrant criminal conviction rates or overall index crime rates. The only exception is that illegal immigrant shares of the population correlate negatively with violent crime convictions in Texas counties from 2012-2018. While these regressions are purely descriptive in nature, they are consistent with the other findings of illegal immigrant criminality in Texas.

Even on the margin, for small changes in the illegal immigrant population in a local area, there is no statistically significant effect of the illegal immigrant population on the rate of criminal convictions, either overall or for illegal immigrants specifically.

4 Nationwide Illegal Immigrant Conviction Rates

Texas is the only state that records convictions and arrests by immigration status, but most Americans live in other states. It's tempting to take the results from Texas and apply them to the rest of the country to come to a nationwide conclusion, but one should be cautious in doing that directly. However, there is evidence that the relationship between illegal immigration and crime in Texas holds nationwide and that evidence comes from applying Gunadi's method to another dataset in the American Community Survey (ACS).

The ACS surveys those incarcerated in correctional facilities to such an extent that it is possible to apply Gunadi's residual method to estimating how many nationwide prisoners are illegal immigrants. The nationwide number includes those in state and federal prisons,

16. Steven Ruggles et al., U.S. Census Data for Social, Economic, and Health Research, IPUMS USA. Accessed April 10, 2020.
17. A. Colin Cameron, Jonah B. Gelbach, and Douglass L. Miller, Bootstrap-Based Improvements for Inference with Clustered Errors, *Review of Economics and Statistics* 90, no. 3 (August 2008): 414–27.

institutions that house very different types of criminals. State prisons house over 87 percent of all prisoners in the United States in 2016 and they are incarcerated mostly for violent and property offenses.[18] Federal prisoners, on the other hand, account for less than 13 percent of all prisoners and are not representative as federal crimes include immigration offenses and other non-state crimes.

ACS inmate responses to questions are reliable as they are ordinarily collected by or under the supervision of correctional institution administrators, so there's no reason to suspect systematic lying on the part of prisoners.[19] Prison staff oversee the interviews and prisoner information from prison records themselves are the basis for much of the information in the ACS, so the data are trustworthy.

Before applying Gunadi's method to the population incarcerated in adult correctional facilities, the first step is to identify those prisoners in the ACS. This requires isolating the prisoners from the rest of the so-called group quarters population in the ACS as they are all lumped together. Group quarters include all group living arrangements such as retirement homes, orphanages, student dorms, mental health institutions, and prisons. Narrowing down that population by age through identifying only those in the 18-54 age range is the most important step as people outside of that range are more likely to be in retirement homes or mental health institutions, respectively. Estimating the population incarcerated in correctional facilities adds ambiguity, but the ACS also releases macro demographic snapshots of inmates in correctional facilities which return total numbers that are very close to those produced by Gunadi's residual method applied to the 18-54 aged population in adult correctional facilities.[20] Although not perfect, applying Gunadi's method to the 18-54 age groups in the ACS group quarters population is reasonable given the data limitations.

Under this method, there are an estimated 1,933,039 native-born Americans, 83,698 illegal immigrants, and 71,472 legal immigrants incarcerated in the United States in 2018. The incarceration rate for native-born Americans was 1,477 per 100,000; 877 per 100,000 for illegal immigrants; and 380 per 100,000 for legal immigrants in 2018 (Figure 14). Illegal immigrants are 41 percent less likely to be incarcerated than native-born Americans. Legal immigrants

18. E. Ann Carson and Joseph Mulako-Wangota, Corrections Statistical Analysis Tool (CSAT) – Prisoners, Bureau of Justice Statistics. Accessed April 10, 2020, https://www.bjs.gov/index.cfm?ty=nps.

19. Landgrave and Nowrasteh, *Illegal Immigrant Incarceration Rates, 2010–2018: Demographics and Policy Implications*.

20. American Community Survey, Characteristics of the Group Quarters Population by Group Quarters Type (3 Types), 2018, https://data.census.gov/cedsci/table?q=S2602&tid=ACSST1Y2018.S2602&vintage=2018&hidePreview=true.

are 74 percent less likely to be incarcerated than natives. As Figure 14 shows, the relative incarceration rates for 2018 are very similar since 2012.

[INSERT FIGURE 14 ABOUT HERE]

The nationwide incarceration rate in Figure 14 is a different measure of criminality than the conviction rate in Figure 11, but they should be correlated over time if two things are true. The illegal immigrant criminal conviction rate in Texas is very similar to the estimated illegal immigrant incarceration rate nationwide. Although data are only available for 2012-2018, the correlation coefficient is 0.99, which suggests that the nationwide incarceration estimates for illegal immigrants in Figure 14 are very close to their actual incarceration rates.

5 Conclusion

Whether one focuses on criminal convictions, arrests, or the number of individuals convicted or arrested, the results are the same: illegal immigrants have a lower crime rate than native-born Americans in Texas. Legal immigrants have the lowest rates of all, except for some measures of property crime where illegal immigrants are even less crime prone. Native-born Americans living in Texas have the highest criminal conviction and arrest rates in all of the figures above. Even on the margin, there is no statistically significant effect of the illegal immigrant population on the rate of criminal convictions, either overall or for illegal immigrants specifically.

Crime, at least in the state of Texas, is a domestically produced problem and not an imported one. Texas is one of the states where we would expect higher illegal immigrant crime rates if they were an especially crime prone subpopulation. Texas' proximity to Mexico, the reputation of its criminal justice system, and the state-level politics all militate toward increasing the illegal immigrant crime rate relative to legal immigrants and native-born Americans.

The low illegal immigrant conviction and arrest rates in Texas suggest that illegal immigrants have a lower rate in other states and across the entire country. Nationwide estimates of the illegal immigrant incarceration rate are very closely correlated to the Texas illegal immigrant criminal conviction rate.

The government is entrusted with an enormous amount of power to protect the lives, liberties, and property of people living in the United States. American residents and citizens are less free when policy makers have poor information about security threats because they react inefficiently. As a result, some threats loom larger than they should and others don't loom large enough. Better and more accurate information can help policymakers judge threats accurately and respond appropriately. Thus, accurate information can free policy makers from making inefficient choices and liberate taxpayers, citizens, and residents from shouldering the burden of inappropriate government policy. Illegal immigration is a serious public policy problem in the United States, but it is not a problem because it increases the crime rate.

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Figures

Figure 1. Texas Conviction Rate

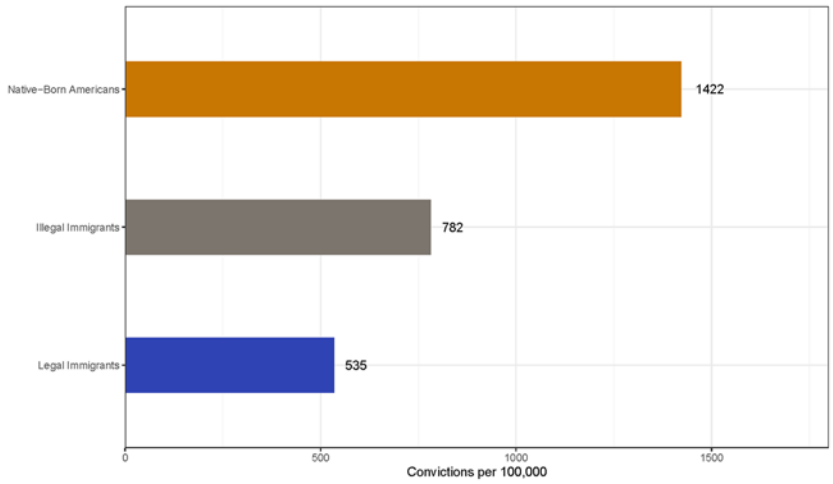

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Figure 2. Texas Homicide Conviction Rate

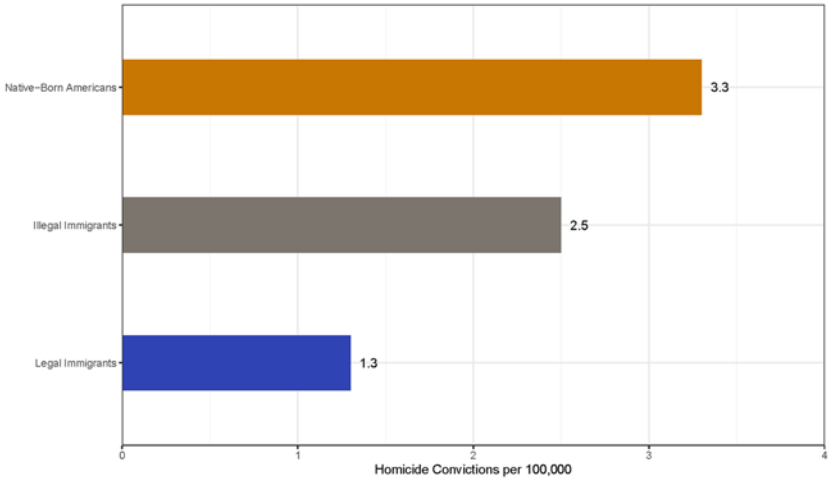

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Figure 3. Texas Violent Crime Conviction Rate

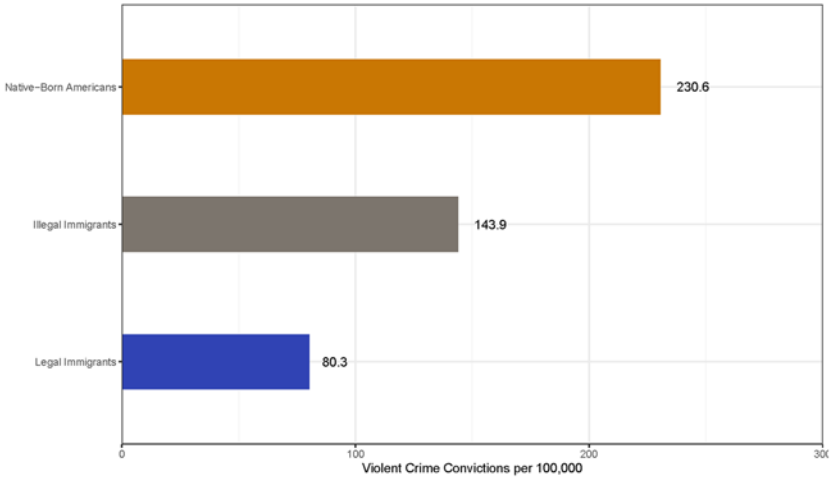

Figure 4. Texas Property Crime Conviction Rate

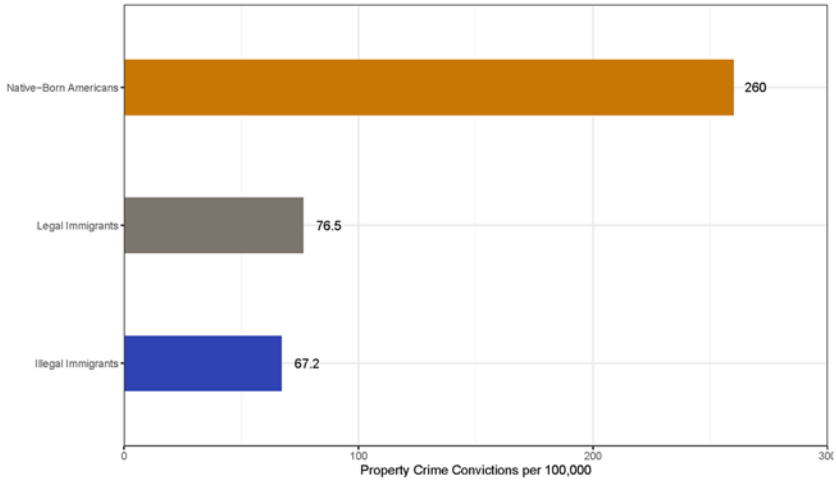

Figure 5. Texas Sex Crime Conviction Rate

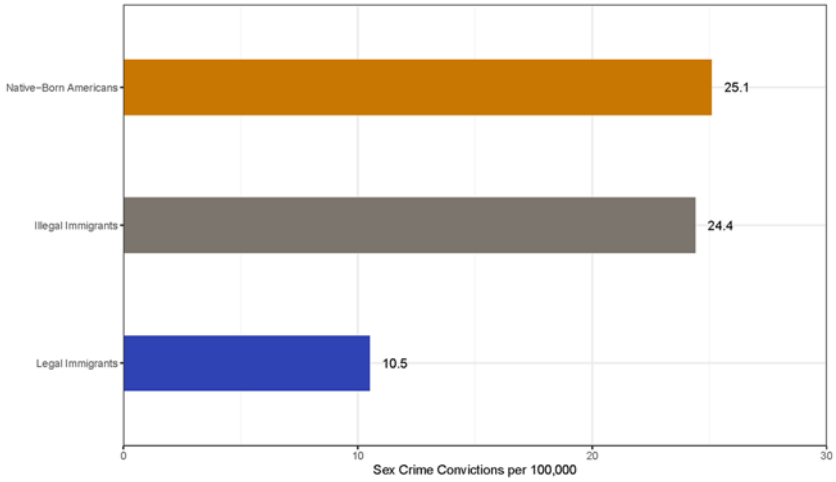

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Figure 6. Texas Arrest Rate

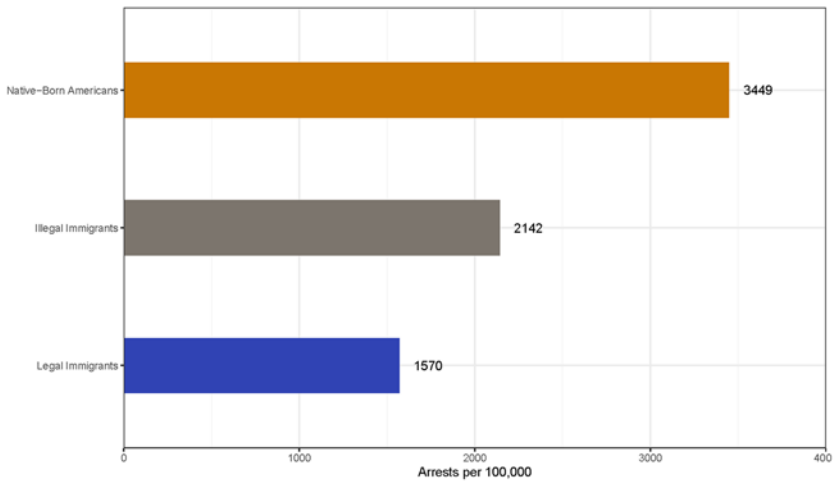

Figure 7. Texas Individuals Convicted and Arrested by Immigration Status

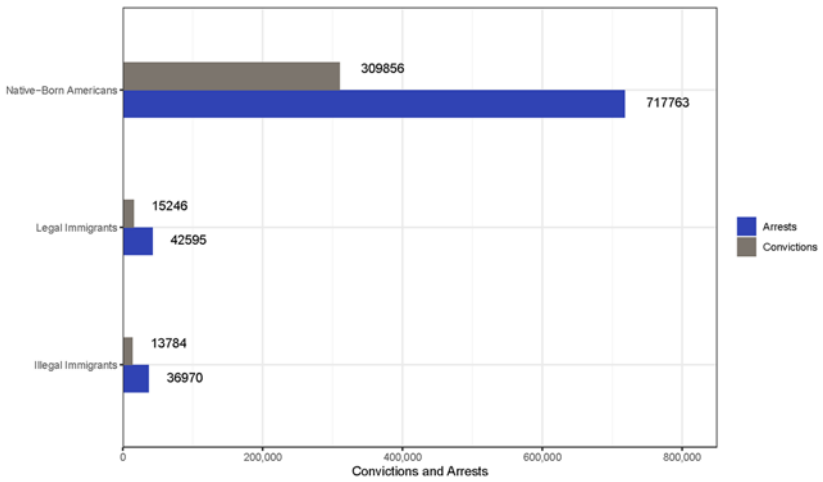

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Figure 8. Texas Homicide Arrest Rate

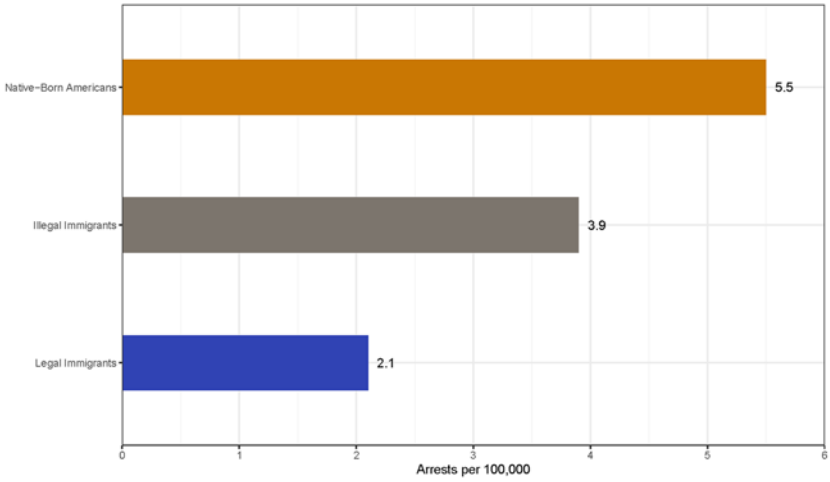

Figure 9. Texas Violent and Property Crime Arrest Rate

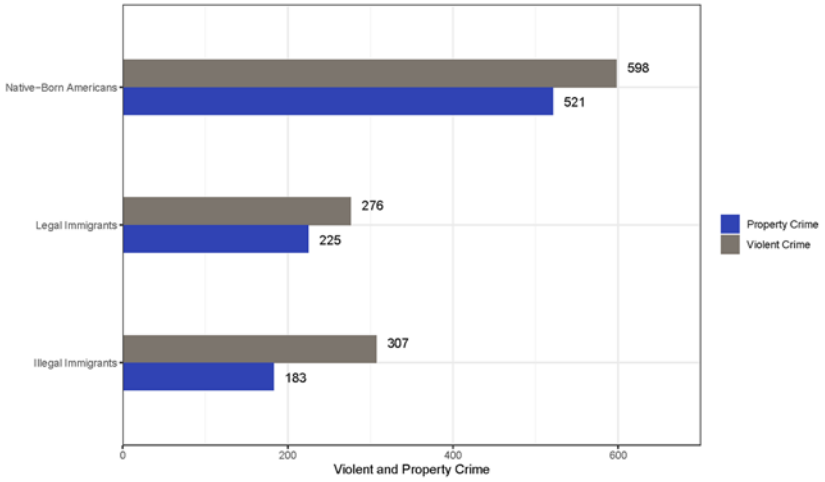

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Figure 10. Texas Sex Crime Arrest Rate

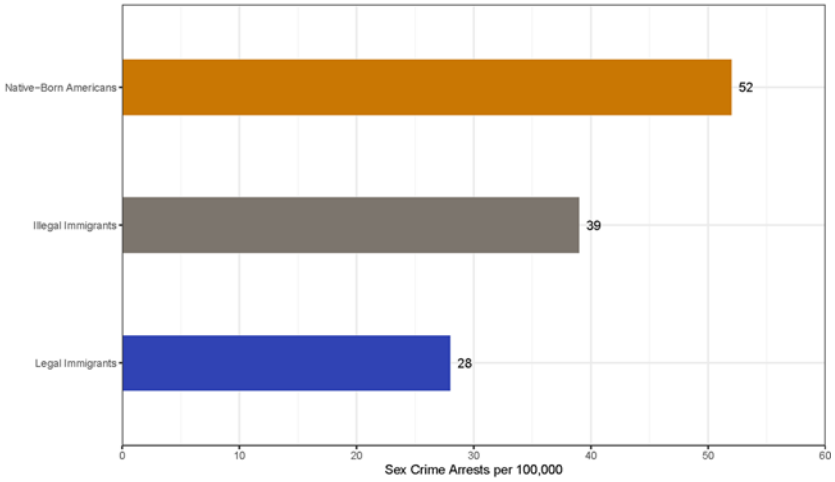


Figure 11. Texas Conviction Rate Over Time

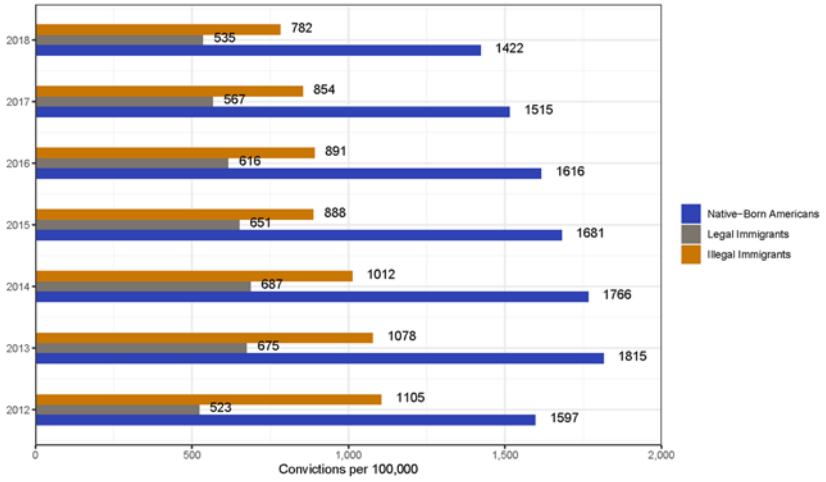


Figure 12. Texas Arrest Rate Over Time

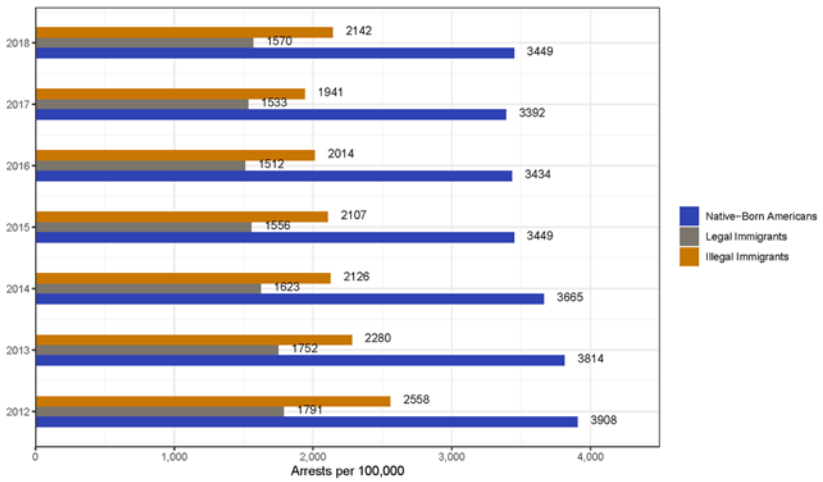
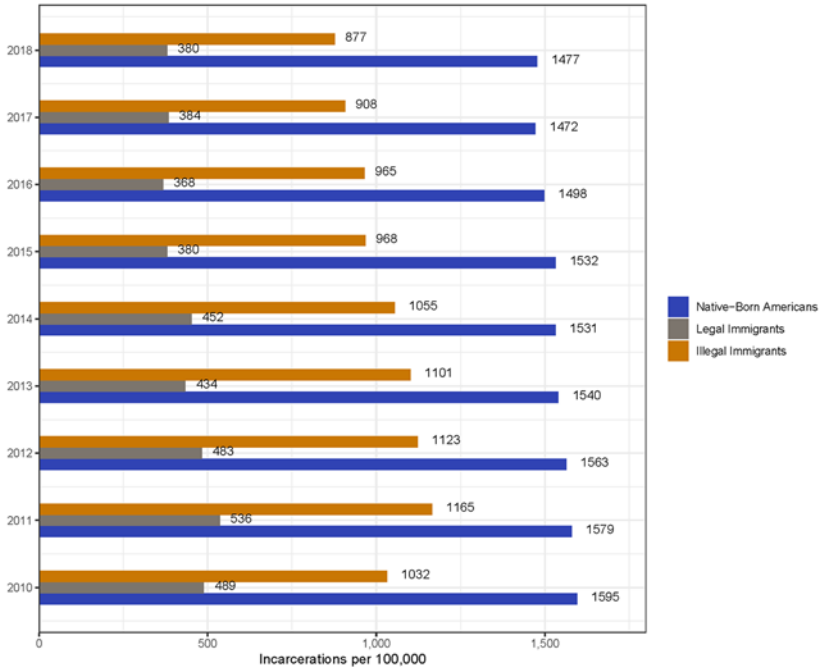


Figure 13. Illegal Immigrant Populations and Convictions

Table presents the estimated elasticity of each crime rate listed in column headers and the illegal immigrant population share. The dependent variable is the natural log of each count plus one to avoid zeroes. Wild cluster bootstrap p-values are shown in parenthesis and their associated 95 percent confidence intervals in brackets. Significance levels are coded * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	All Crime		Murder		*Violent Crime*		Property Crime	
	Total	Illegal	Total	Illegal	Total	Illegal	Total	Illegal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Illegal	-0.06805	-0.049	0.043	-0.005	-0.104*	-0.071	-0.052	-0.117
	(0.082)	(0.764)	(0.486)	(0.846)	(0.018)	(0.632)	(0.238)	(0.454)
	[-0.147, 0.011]	[-0.515, 0.416]	[-0.075, 0.161]	[-0.068, 0.058]	[-0.192, 0.016]	[-0.436, 0.293]	[-0.143, 0.039]	[-0.484, 0.251]
Log Population	1.749***	4.219*	-0.864	-0.482	2.443**	3.647*	1.432***	3.522*
	(0.002)	(0.018)	(0.600)	(0.466)	(0.006)	(0.018)	(0.000)	(0.018)
	[0.842, 2.655]	[0.546, 7.893]	[-4.110, 2.381]	[-1.800, 0.835]	[0.770, 4.117]	[0.594, 6.701]	[0.604, 2.260]	[0.495, 6.550]
Log Cops per 100 (t-1)	-0.034	0.04	0.114	0.044	-0.108	0.023	-0.003	0.050
	(0.196)	(0.792)	(0.260)	(0.166)	(0.294)	(0.848)	(0.866)	(0.662)
	[-0.086, 0.020]	[-0.473, 0.553]	[-0.080, 0.308]	[-0.013, 0.100]	[-0.198, -0.018]	[-0.371, 0.417]	[-0.038, 0.032]	[-0.392, 0.492]
Observations	228	228	228	228	228	228	228	228
R2	0.153	0.042	0.004	0.005	0.218	0.049	0.085	0.057

Figure 14. Nationwide Incarceration Rate Over Time



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Illegal Immigrant Murderers in Texas, 2013–2022

Illegal Immigrant and Legal Immigrant Conviction and Arrest Rates for Homicide and Other Crimes

BY ALEX NOWRASTEH

EXECUTIVE SUMMARY

Crime committed by illegal immigrants is an important and contentious public policy issue, but it is notoriously difficult to measure and compare their criminal conviction rates with those of other groups such as legal immigrants and native-born Americans. This policy analysis is the latest paper that attempts to resolve those data disputes by relying on detailed crime data from Texas. Over the 10-year period from 2013 to 2022, the homicide conviction rate in Texas for illegal immigrants was 2.2 per 100,000, compared to 3.0 per 100,000 for native-born Americans. The homicide conviction rate for legal immigrants in Texas was 1.2 per 100,000. Illegal immigrants were 26 percent less likely than native-born Americans to be convicted of homicide, and legal immigrants were 61 percent less likely. Criminal conviction data for crimes other than homicide are included, but readers should interpret them with caution because the quality of the data is suspect. The conviction and arrest rates of illegal and legal immigrants, separately and together, were lower than those of native-born Americans for homicide and all crimes in Texas during the 2013–2022 period.

ALEX NOWRASTEH is the vice president for economic and social policy studies at the Cato Institute. He is the coauthor with Benjamin Powell of the book *Wretched Refuse? The Political Economy of Immigration and Institutions* (Cambridge University Press, 2020).

INTRODUCTION

Crime committed by illegal immigrants is an important public policy issue.[1] The prevalence and types of crimes committed by illegal immigrants should guide the allocation of immigration enforcement resources—at the federal, state, and local levels—with the goal of removing those who are convicted of violent or property offenses.[2] However, estimating illegal immigrant criminality is difficult due to various data constraints that have only recently been relaxed in Texas. This policy analysis is an update, improvement, and expansion of earlier Cato research that measured criminal conviction and arrest rates by immigration status in Texas.[3] The data below are for all criminal convictions over a longer period, with a focus on homicide because it is the most serious crime and the one where the number of illegal immigrant offenders is least likely to be undercounted. Specifically, this policy analysis adjusts its methodology to account for criticisms of the quality of Texas crime data.

"Most research finds that all immigrants in the United States are less likely to commit crime or be incarcerated than native-born Americans."

Most research finds that all immigrants in the United States are less likely to commit crime or be incarcerated than native-born Americans and that they don't increase crime in local areas, with the exception of Miami immediately after the Mariel Boatlift of 1980.[4] There is less research on illegal immigrant criminality in the United States, but it generally shows that illegal immigrants have lower incarceration rates nationwide relative to native-born Americans, lower conviction and arrest rates in Texas in earlier years, and the same rates of re-arrest.[5] New research inspired by the Cato Institute's earlier findings on illegal immigrant crime in Texas but based on more granular crime data found that illegal immigrants have a *lower* criminal conviction rate than native-born Americans and legal immigrants in that state.[6] Recent peer-reviewed empirical studies on illegal immigrant criminality have found no link between violent crime and the size of the illegal immigration population. They also found a negative relationship between the number of illegal immigrants and most types of nonviolent crime at the local level.[7]

Researchers at the Center for Immigration Studies (CIS) interpreted Texas crime data differently, concluding that illegal immigrants have higher criminal conviction rates than native-born Americans.[8] CIS claims that for two main reasons, much of Cato's research undercounts the number of illegal immigrants convicted of crimes. First, CIS claims that Cato's research did not include illegal immigrants who were later identified in prison after they were incarcerated, and thus included only illegal immigrants arrested and convicted, as recorded by the Texas Department of Public Safety (DPS). Second, CIS claims that the Texas Department of Criminal Justice (TDCJ), which oversees Texas state prisons and jails, requires time and resources to investigate the immigration backgrounds of all incarcerated prisoners. Because of those time and resource constraints, TDCJ largely focuses on investigating the immigration statuses of the most serious criminal offenders, so researchers should focus on data from earlier years for serious crimes. This policy analysis addresses CIS's concerns in three ways: by using more specific data from Texas DPS that does not overcount or undercount incarcerated illegal immigrant criminals; by focusing on homicide; and by analyzing data from earlier years.

METHODOLOGY

This policy analysis relies on data from the Texas DPS obtained through a Public Information Act request.[9] The Texas DPS data separately show the number of convictions and arrests of individual legal immigrants, illegal immigrants, and native-born Americans in Texas for the 10 years of 2013–2022. DPS does not explicitly identify the number of native-born American individuals arrested or convicted of crimes, but it has a third category: "unknown or other." The only other category of people left is native-born Americans, so the "unknown or other" category is identified herein as native-born Americans.

Texas is the only state that records criminal convictions and arrests by immigration status. Texas has this information because its law enforcement agencies cooperate with federal immigration enforcement authorities at the

Department of Homeland Security (DHS), which checks the biometric information of arrestees in the state and tracks them through to their convictions.[10] The Texas DPS keeps the results of these DHS checks, which they label as PEP, named after an earlier system that helped local law enforcement agencies identify illegal immigrant criminals.[11] After criminal convictions, the TDCJ continues to investigate the immigration statuses of offenders incarcerated for some of the most serious offenses. This results in the identification and reclassification of legal and illegal immigrants who were previously categorized as other or unknown. The DPS then retains the results of both the DPS and TDCJ immigration checks.

Guaranteeing that all convicted individuals are included without double-counting is a major challenge in analyzing Texas crime data. The DPS gave data to the CIS that double-counted some illegal immigrants convicted of crimes.[12] The reasons why this double-counting occurred are complex, but Cato scholars resolved the issue through more detailed data requests on the number of legal and illegal immigrants identified only by TDCJ checks, identified only by PEP checks, and identified only by both TDCJ and PEP checks.[13] The clearly defined variables in Cato's data requests removed the possibility of double-counting.

"The homicide conviction rates are confidently presented here because the Texas Department of Criminal Justice most intensely investigates the immigration statuses of individuals convicted of that crime."

The homicide conviction rates are confidently presented here because the TDCJ most intensely investigates the immigration statuses of individuals convicted of that crime. The criminal conviction rates for all crimes are also shown here, but with less confidence, because the TDCJ does not spend nearly as many resources investigating the immigration statuses of lesser criminals. This policy analysis presents criminal conviction rates for all crimes to provide a comparison that some readers may find useful, although they should interpret the rates cautiously. An additional reason to focus on homicide is that it is not an underreported crime, or at least not as underreported as other, less serious crimes.[14] Thus, homicide data are the most complete for any crime and provide the best opportunity to understand criminal behavior overall and by subpopulation.

Controlling for the size of the population is essential for comparing relative conviction and arrest rates between groups, so this paper copies the methods of government agencies that generally report the conviction and incarceration rates per 100,000 members of each subpopulation.[15]

Calculating the crime rates for illegal immigrants, legal immigrants, and native-born Americans in Texas requires estimating the size of each subpopulation. This presents a particular challenge for illegal immigrants because the American Community Survey (ACS) and other population surveys do not specifically ask whether respondents are illegal immigrants. Even if they did ask, it would be unwise to trust the results. Thus, this policy analysis follows widely used statistical methods to estimate the size of Texas's legal and illegal immigrant populations over the 2013–2022 period.[16] That method is a modified residual method developed by economist Christian Gunadi that makes larger adjustments for the estimated undercount of the immigrant population and relaxes earlier assumptions about employment and Medicaid access due to legal changes since Gunadi first published his methods.[17] Gunadi imputed legal immigrant status and identified those remaining foreign-born residents of Texas as illegal immigrants, which is different from other residual statistical methods that identify illegal immigrants first and then count the remaining foreign-born residents as legal immigrants.[18] Our estimation method counts people as legal immigrants if they meet any of the following criteria as recorded in the ACS: arrived in the US before 1982; is a US citizen; received welfare benefits such as Social Security, Supplemental Security Income, Medicaid (with some adjustment based on states extending Medicaid access to illegal immigrants), Medicare, or military insurance; served in the armed forces; resided in public housing or received rental subsidies or was the spouse of someone who resided in public housing or received rental subsidies; had occupational licenses; was born in Cuba and immigrated prior to 2017; and/or

had a spouse who was a legal immigrant or US citizen.[19] The number of legal immigrants estimated using this method includes those residing in Texas on temporary nonimmigrant work visas and those who have naturalized and earned American citizenship.

Our modified residual method estimates that in Texas in 2022, there were approximately 2.1 million illegal immigrants, who comprised about 7.1 percent of the state's resident population. In the same year, there were about 3.3 million legal immigrants, who represented 10.8 percent of the population, and 24.9 million native-born Americans, who represented 82 percent of the population. Our estimates of the illegal immigrant population are sensitive to the assumptions in our model, but they are very close to DHS estimates too. Specifically, we estimated that there were 2,018,723 illegal immigrants in Texas in 2022, and DHS estimated that there were 2,060,000—a difference of 2 percent and not enough to bias our homicide or crime rate calculations relative to others.[20]

The number of criminals identified in the data here is not the total number of criminals residing in Texas over the 2013–2022 period but merely the number of individuals arrested and convicted of homicide and other crimes by their year of arrest. Some small number of individuals may have been arrested in Texas for crimes committed in other states, but those convicted in Texas were convicted for Texas state crimes. The DPS data analyzed in this paper are for all individuals arrested and convicted in the 2013–2022 period, regardless of the year in which the crime was committed.

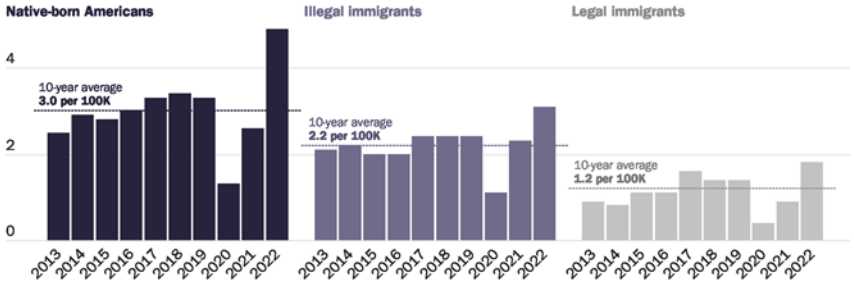
Texas is an ideal state to study immigrant criminality for multiple reasons: It borders Mexico; it has the second-largest illegal immigrant population of any state; it is a politically conservative state governed by Republicans; it did not have jurisdictions in most recent years that limited its cooperation with federal immigration enforcement; it has a reputation for strictly enforcing its criminal laws; and 58 percent of all Border Patrol and Office of Field Operations encounters of illegal immigrants along the Southwest border from October 2020 to January 2024 occurred in Texas.[21]

TEXAS HOMICIDE CONVICTION AND ARREST RATES

During the 10-year span from 2013 to 2022, the homicide conviction rate in Texas was 2.2 per 100,000 illegal immigrants, 1.2 per 100,000 legal immigrants, and 3.0 per 100,000 native-born Americans (Figure 1). Illegal immigrants were 26.2 percent less likely than native-born Americans to be convicted of homicide. Legal immigrants were 61.4 percent less likely than native-born Americans to be convicted of homicide. Over those 10 years, 472 illegal immigrants, 336 legal immigrants, and 7,109 native-born Americans were convicted of homicide in Texas. The years 2020 and 2021 show a substantial decline in the number of homicide convictions.

In 2022, the homicide conviction rate was 3.1 per 100,000

Figure 1
Homicide conviction rates by immigration status in Texas per 100,000 residents in each subpopulation, 2013–2022



Sour es: Author's analysis of data from the Texas Department of Publi Safety and the Ameri an Community Survey.

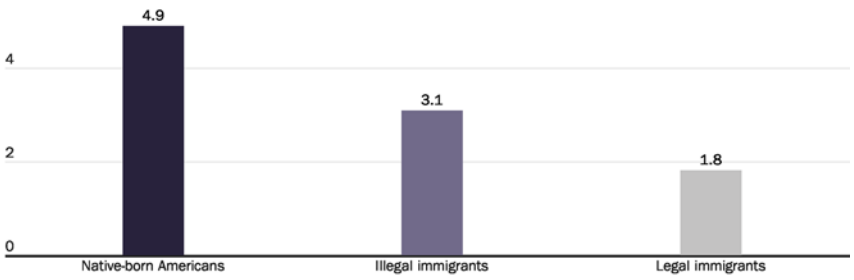
for illegal immigrants, 1.8 per 100,000 for legal immigrants, and 4.9 per 100,000 for native-born Americans (Figure 2). A total of 1,336 people were convicted of homicide in Texas in 2022. Of those, 1,209 were native-born Americans, 67 were illegal immigrants, and 60 were legal immigrants. In 2022, homicide conviction rates for illegal immigrants and legal immigrants were 35.6 percent and 62.3 percent, respectively, below those of native-born Americans. Illegal immigrants made up about 7.1 percent of the Texas population in 2022 but accounted for only 5 percent of all people convicted of homicide. Legal immigrants made up 10.1 percent of the Texas population but accounted for only 4.5 percent of people convicted of homicide. Native-born Americans made up 82.5 percent of the Texas population but accounted for 90.5 percent of people convicted of homicide. Illegal and legal immigrants were statistically underrepresented in homicide convictions, and native-born Americans were statistically overrepresented.

There were 2,277 total homicides in Texas in 2022, according to provisional cause-of-death data collected by the Centers for Disease Control and Prevention's Wide-ranging ONline Data for Epidemiologic Research (CDC WONDER).[22] The FBI's Uniform Crime Reporting (UCR) Program recorded 2,020 homicides in Texas in 2022 from a subsample of law enforcement agencies that report local homicides to the FBI.[23] Different methods of reporting homicides by CDC WONDER and the UCR explain the difference between the lower number of homicides reported by the FBI and the higher number reported by the CDC. The FBI UCR gathers its statistics through a cooperative statistical collaboration of law enforcement agencies across the country. Most law enforcement agencies report data to the UCR, including the largest agencies that serve most Americans, but not all law enforcement agencies do, and their methods, quality, and consistency of recording crime data vary. CDC WONDER gathers its data from the National Vital Statistics System, which starts with state and local reporting of deaths by types that local agencies report upward using nationally standardized codes and methods to indicate the numbers and causes of deaths. In Texas in 2022, there were 1,336 convictions for homicide, which account for about 59 percent of all homicides recorded by the CDC WONDER data in that year. However, the number of convictions in that year includes some for homicides committed in earlier years because the date of arrest or the date of sentencing is the year recorded in the Texas crime data, while the total number of homicides reported by CDC WONDER is the number of homicides that were committed in 2022.

Figure 3 shows the homicide arrest rates for each subpopulation in the 2013–2022 period. In total, 729 illegal immigrants, 765 legal immigrants, and 14,710 native-born Americans were arrested for homicide in Texas. The homicide arrest rates over the entire period are 3.4 for every 100,000 illegal immigrants, 2.6 for legal immigrants, and 6.2 for native-born Americans. Illegal immigrants were 44.8 percent less likely than native-born Americans to be arrested for homicide. Legal immigrants were 57.6 percent

Figure 2

Homicide conviction rates by immigration status in Texas per 100,000 residents in each subpopulation, 2022



Sources: Author's analysis of data from the Texas Department of Public Safety and the American Community Survey.

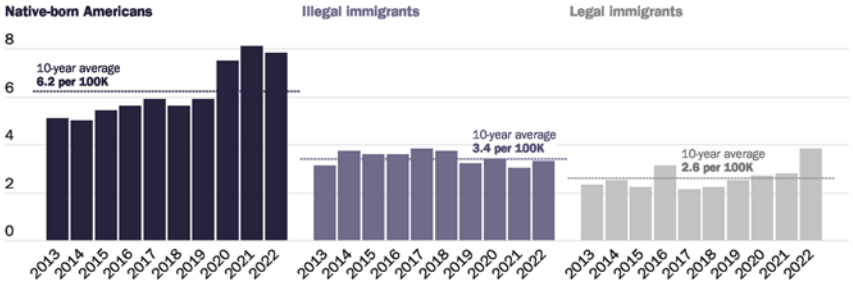
less likely than native-born Americans to be convicted of homicide. The homicide-conviction-to-arrest ratio for illegal immigrants is 0.65 during the 10-year period analyzed here, meaning that about 65 percent of homicide arrests of illegal immigrants led to a conviction. The conviction-to-arrest ratios for legal immigrants and native-born Americans are 0.44 and 0.48, respectively, meaning that fewer arrests of legal immigrants and native-born Americans led to homicide convictions.

There are several potential explanations for the different conviction-to-arrest ratios. Law enforcement or prosecutors may be more focused on solving homicides where illegal immigrants are suspected; illegal immigrants may have less competent criminal defense on average; witnesses may be more likely to cooperate when the accused criminal is an illegal immigrant; or other factors separately or in combination could explain the difference. Regardless of the reasons, the higher conviction-to-arrest ratio for illegal immigrants could be interpreted in several different ways. For instance, illegal immigrants could be less likely to flee after committing a homicide, which would be odd because, with fewer ties to the United States than legal immigrants and native-born Americans, illegal immigrants could return to their home countries at a lower cost. Another possibility is that many do flee, and those who remain and are arrested are more likely to be convicted, which means that either those less likely to be convicted are more likely to flee or that many of the unsolved homicides in Texas were committed by illegal immigrants. Since state-level homicide clearance rates are not related to the size of the illegal immigrant population, it's unlikely that illegal immigrants are committing a disproportionate share of homicides and escaping convictions.[24] Regardless, those explanations cannot be definitively ruled out without more data.

TEXAS CRIMINAL CONVICTION AND ARREST RATES

The data on all criminal convictions and arrests in Texas are more suspect than the data for homicide because, as stated above, the TDCJ does not expend many resources investigating the immigration statuses of those convicted of relatively minor crimes. Thus, readers should skeptically interpret data on the criminal conviction and arrest rates for all crimes. Regardless, the data on all criminal conviction and arrest rates by immigration status are reported here to provide a point of comparison. During the 10-year period of 2013–2022, 145,254 illegal immigrants, 159,138 legal immigrants, and 3,113,693 native-born Americans were convicted of crimes in Texas. The criminal conviction rate over the entire period is 685 per 100,000 illegal immigrants, 551 per 100,000 legal immigrants, and 1,321 per 100,000 native-born Americans (Figure 4). Illegal immigrants were 48 percent less likely than native-born Americans to be convicted of a crime, and legal immigrants were 58 percent less likely. The results are similar for arrest rates, where

Figure 3
Homicide arrest rates by immigration status in Texas per 100,000 residents in each subpopulation, 2013–2022



Sources: Author's analysis of data from the Texas Department of Public Safety and the American Community Survey.

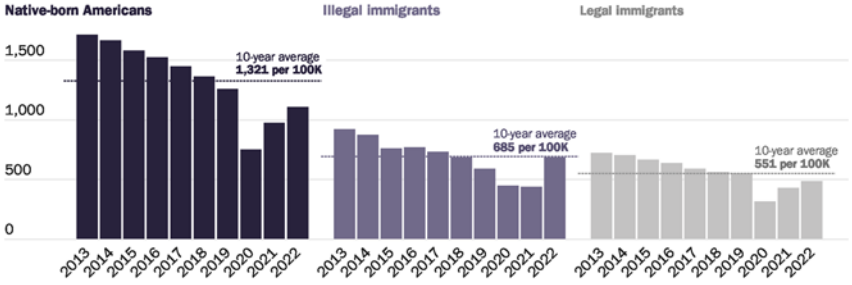
illegal immigrants had an arrest rate 44 percent lower than native-born Americans, and legal immigrants had an arrest rate 53 percent lower (Figure 5).

THE PANDEMIC AND POST-PANDEMIC "CATCH-UP" CONVICTIONS

The COVID-19 pandemic and the government's response to it peaked in 2020 and 2021. One of the myriad government responses to the pandemic was a substantial decline in homicide conviction rates (Figure 1), all criminal conviction rates (Figure 4), and all criminal arrest rates (Figure 5) in 2020 and 2021. From 2013 to 2019, the seven years preceding the pandemic, about 785 people were convicted of homicide each year in Texas. There was a clear upward trend in the number of homicide convictions in Texas during those seven pre-pandemic years, peaking at 902 in 2019. In 2020, there were only 365 homicide convictions in Texas—a 60 percent drop from 2019. The number of homicide convictions rose to 722 in 2021 but was still 20 percent below the 2019 number. The Texas criminal justice system recovered in 2022 and quickly began to make up for lost time by convicting 1,336 people for homicide—the highest number in the sample and just below the combined number of convictions in 2013 and 2014. Many

Figure 4

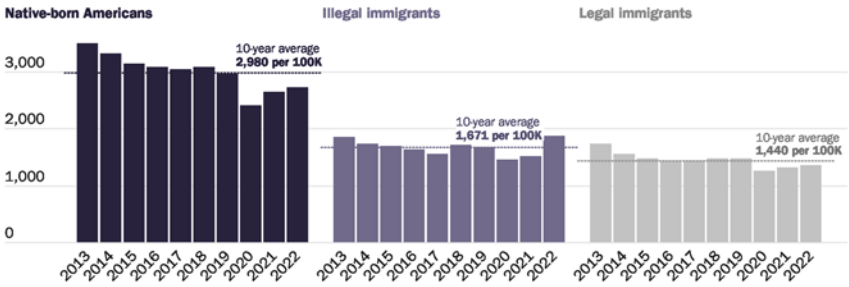
All criminal conviction rates by immigration status in Texas per 100,000 residents in each subpopulation, 2013–2022



Sources: Author's analysis of data from the Texas Department of Public Safety and the American Community Survey.

Figure 5

All criminal arrest rates by immigration status in Texas per 100,000 residents in each subpopulation, 2013–2022



Sources: Author's analysis of data from the Texas Department of Public Safety and the American Community Survey.

of those 1,336 convictions were of individuals who would have otherwise been tried and convicted in 2020 and 2021.

The decline in convictions in 2020 and 2021 is a direct result of the Texas state government's response to the COVID-19 pandemic. In mid-March 2020, Governor Greg Abbott declared a state of disaster in Texas.[25] On March 13, 2020, the Supreme Court of Texas issued the first in a series of emergency orders to address court operations.[26] These orders allowed courts to modify or suspend legal deadlines and procedures, suspend or limit in-person jury proceedings, authorize remote appearances, and take numerous other measures to reduce health risk.[27] Although the intent of the numerous orders was to protect the health of court staff, parties, attorneys, jurors, and the public, another consequence was to slow criminal prosecution and hence convictions in Texas.

A previous section details the homicide conviction rate in 2022 because it is the most recent year for which the data are reliable, but the "catch-up" convictions in that year also make it quite unusual. We should expect that high number of criminal convictions to drop somewhat in 2023 and return to the long-term trend in 2024 when the data for those years are reported. Still, during the slowdown in criminal court proceedings in 2020 and 2021, the ratios of illegal immigrant to native-born American and legal immigrant to native-born American criminal conviction rates remained similar to those of earlier years. Thus, there's no evidence that the slowdown in homicide convictions in 2020 biased the results.

DISCUSSION AND POLICY RECOMMENDATIONS

This policy analysis finds that legal and illegal immigrants were less likely to be arrested and convicted of homicide and other crimes in Texas during the 2013–2022 period. However less likely illegal and legal immigrants are to be convicted of homicide and other crimes, though, some of them *are* criminals. Illegal immigrants and noncitizen legal immigrants who are convicted of violent or property offenses should continue to be punished with incarceration, as native-born Americans are, and then removed from the United States. It is also clear that a broad-based interior immigration enforcement operation will not reduce American crime rates, because illegal immigrants are less crime-prone than native-born Americans (if the results from Texas are representative). Although that is not an argument against removing illegal immigrant criminals, it is an argument against removing illegal immigrants who have not been convicted of violent or property offenses in the hope that doing so would reduce crime rates.[28]

"Illegal immigrants and noncitizen legal immigrants who are convicted of violent or property offenses should continue to be punished with incarceration, as native-born Americans are, and then removed from the United States."

A limitation of this research is that it is confined entirely to Texas, but illegal immigrants live in every state in the union. It's not unreasonable to infer that, based on Texas data, illegal and legal immigrants in most other states typically have a lower homicide and criminal conviction rate than native-born Americans, because Texas is such a great sample, but the public, policymakers, and residents of other states should be certain. There very well could be states where illegal immigrants have a higher homicide or criminal conviction rate than native-born Americans; Americans should know whether that is true and, if so, where. The state of Texas should invite representatives from other states' departments of public safety, criminal justice, and corrections to Austin to show them how to record, maintain, and track the immigration statuses of those arrested, convicted, and incarcerated for crimes. At the same time, or prior to that convening, Texas DPS should invite members of the National Research Council who work on crime and immigration, along with statisticians, social scientists, criminologists, and others with expertise in crime data, to closely examine how Texas DPS records and organizes its data to see whether its methods can be improved to ensure clarity, maximize accuracy, and minimize errors.[29] If an invitation from the Texas state government is not forthcoming, other states should take the initiative and ask Texas for guidance.

CONCLUSION

Texas is the only state that keeps records of the immigration statuses of those arrested and convicted of state-level crimes. Policymakers can learn much by analyzing the state-level-crime data from Texas and inferring average rates of illegal and legal immigrant criminality in the rest of the United States, both nationwide and by state. Although immigrants do occasionally commit heinous crimes, they are less likely to do so than native-born Americans. This is no comfort to the victims and their loved ones, but it is information that policymakers can use to craft better immigration and anti-crime policies. Illegal immigrants and lawfully present noncitizens who commit violent or property crimes should be removed from the United States after being convicted and punished. All states should learn from Texas and adopt its method of recording the immigration statuses of those convicted and arrested. In Texas, illegal immigrants and legal immigrants are much less likely than native-born Americans to be convicted or arrested for homicide and other crimes.

NOTES

1. *Hearing on How the Border Crisis Impacts Public Safety, Before the Committee on Oversight and Accountability, Subcommittee on National Security, the Border, and Foreign Affairs*, 118th Cong., 2nd Sess. (April 16, 2024) (statement of Hon. Ken Cuccinelli).

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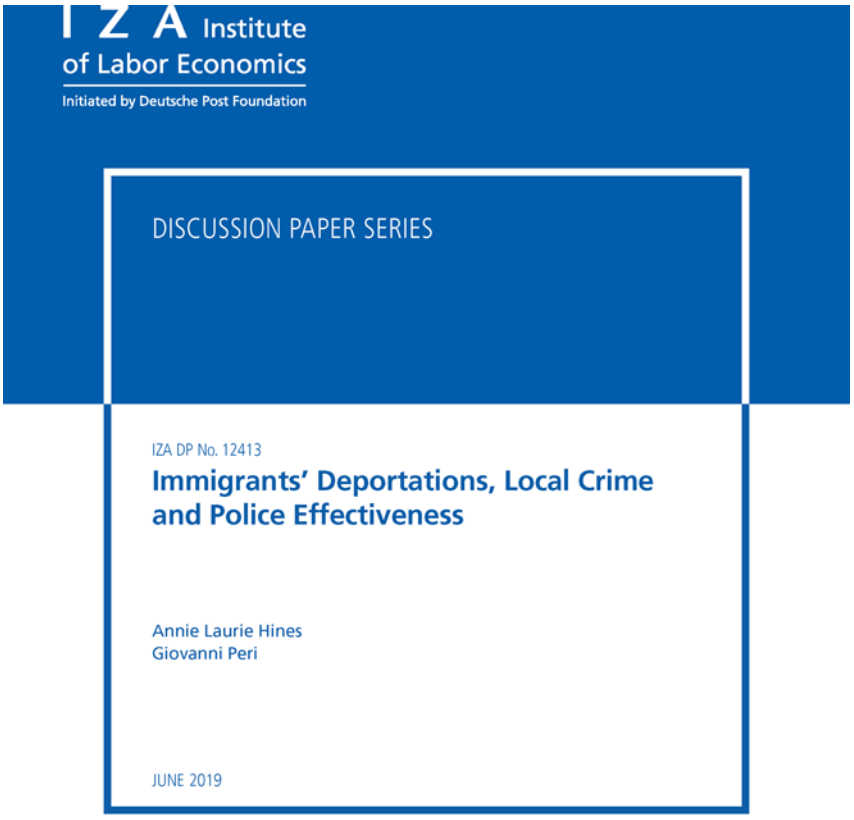
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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

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ABSTRACT

Immigrants' Deportations, Local Crime and Police Effectiveness*

This paper analyzes the impact of immigrant deportations on local crime and police efficiency. Our identification relies on increases in the deportation rate driven by the introduction of the Secure Communities (SC) program, an immigration enforcement program based on local-federal cooperation which was rolled out across counties between 2008 and 2013. We instrument for the deportation rate by interacting the introduction of SC with the local presence of likely undocumented in 2005, prior to the introduction of SC. We document a surge in local deportation rates under SC, and we show that deportations increased the most in counties with a large undocumented population. We find that SC-driven increases in deportation rates did not reduce crime rates for violent offenses or property offenses. Our estimates are small and precise, so we can rule out meaningful effects. We do not find evidence that SC increased either police effectiveness in solving crimes or local police resources. Finally, we do not find effects of deportations on the local employment of unskilled citizens or on local firm creation.

JEL Classification: K24, K37

Keywords: immigrants, deportation, crime, police effectiveness, secure communities

Corresponding author:
Giovanni Peri
Department of Economics
University of California, Davis
One Shields Avenue
Davis CA 95616
USA

Email: gperi@ucdavis.edu

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1 Introduction

Despite popular belief, academic studies find little correlation between immigration and crime rates in the US. Most find that immigrants are less likely to commit crimes and less likely to be incarcerated than similar natives (Butcher and Piehl, 2008), while undocumented immigrants have lower conviction and arrest rates (Nowrasteh, 2018). There is evidence that immigration decreases local crime rates (Chalfin, 2015), but no evidence that the presence of undocumented immigrants is associated with more crime (Light and Miller, 2018). More recent research examines the impact of increased enforcement on local crime. For example, Chalfin and Deza (2018) find that the E-Verify program, allowing employers to check the work eligibility status of their employees, reduced the population of young males in Arizona thus reduced the occurrence of property crime by changing Arizona's demographic composition. Overall, the literature finds a negative or null association between immigrants and crime.

Immigrant deportations that explicitly prioritize removing people convicted of a crime, some advocates claim, is a more direct enforcement policy to meet the goal of reducing crime. The Trump administration has claimed that undocumented immigrants pose a security risk to justify increased deportations. [1]. This recent surge in deportations under President Trump is not the first time the US federal government has taken a harder line on immigration enforcement. As early as 1929-1934, the US carried out a forced repatriation of about 500,000 Mexicans and Mexican Americans, claiming, incorrectly, that this measure would have improved the working opportunities for natives (Lee, Peri and Yasenov, 2017). More recently, in the period 2008-2012, the US deported substantially more undocumented immigrants than it had in past decades.[2] The stated intent was to remove serious criminals and to reduce local crime. However, few studies have attempted to test these claims. Were enforcement policies effective at reducing crime rates or increasing police effectiveness? This paper seeks to answer this question empirically by identifying the impact of deportations on local crime rates and on police effectiveness in solving and preventing crime.

Deportations should reduce crime if the deported people are serious criminals or more likely to commit crimes in the future. If people who commit immigration violations are also more likely to commit other offenses, as some politicians claim, enforcement programs

[1]For example, see Politifact, Friday, February 24th, 2017, "Trump moves on Immigration. Deportations in First month"

[2]While all non-citizens are eligible for deportation, most increases in deportation efforts focus on undocumented immigrants. Other non-citizens may be deported if, for example, they commit an aggravated felony.

that provide immigration information to the police may help local law enforcement prevent crimes. In 2008, the Department of Homeland Security (DHS) launched Secure Communities (SC) as a tool to engage the resources of local police in federal immigration enforcement efforts by screening anyone booked into jail, no matter the reason, for immigration violations. Immigration and Customs Enforcement (ICE), the federal immigration enforcement agency, could then take custody of any person found to have violated immigration law. Between its introduction in 2008 and temporary suspension in 2014, SC led to over 450,000 deportations.

This paper considers whether more intense immigration enforcement, as measured by deportations per population, decreased crime rates for violent offenses or property offenses. First, we document that the staggered introduction of SC significantly increased the risk of deportation for undocumented immigrants once introduced. Then, we estimate the effect the deportation rate, measured as the number of deportations divided by the adult population, on changes in crime rates. Of course, deportations may be endogenous: unobserved factors such as local attitudes towards undocumented immigrants, or changes in police behavior, may impact both the local intensity of immigration enforcement and crime rates. Therefore, to identify a causal effect of deportations on crime, we instrument for the deportation rate with the interaction of the adoption of SC and the local presence of likely undocumented immigrants in 2005. To capture potential local economic effects of the Great Recession, we control for industry-specific employment shares in construction and manufacturing. In addition, we check the correlation of the timing of the rollout of SC with pre-2008 trends in crime, economic characteristics, and demographics.

Our main findings show that increased deportations did not reduce local crime rates. The estimated effect on violent crime is small and not statistically significant. The effect on property crime, while positive and usually significant, is also small. In most cases, the number of people removed by deportation did not have a large impact on the undocumented population at the local level. On average, one percent of the likely undocumented population was deported, although the share was as high as ten percent in some counties. This is likely too small a share to see a significant effect on crime from compositional changes. Deportations may also affect the undocumented population if immigrants move in response to more intense enforcement in an area. However, we do not find a significant correlation between deportation rates and changes in the likely undocumented population, suggesting that SC did not reduce the undocumented population in net. Of course, heightened enforcement may still have deterred crime if it reduced the propensity of the targeted population to commit crimes, but this seems unlikely considering that we do not find an impact on crime rates or

demographic composition.

Beyond removing criminals, SC may have impacted police efficiency and resources indirectly by altering how local law enforcement allocated limited resources and changing the demands on those resources. To measure changes in police efficiency, we examine the local clearance rate, or the number of crimes cleared by arrest, relative to all crimes reported in a year. To check whether SC impacted police resources, as SC put higher demands on the local law enforcement by requiring jails to detain potential violators of immigration law, we examine the number of law enforcement personnel per capita. We do not find any evidence that increases in the deportation rate affected police efficiency or altered the level of police resources. Finally, SC could have affected crime indirectly by changing local employment opportunities or firm creation. However, we do not find evidence of unintended economic consequences through firm creation or the employment of less educated workers.

The paper is organized as follows. Section 2 reviews the literature on immigration and crime. Section 3 describes the features of the Secure Communities program, its timing and intensity and the consequences in terms of deportations. Section 4 describes the data we use and it shows the summary statistics and trends of the main crime, enforcement and deportation variables. Section 5 presents the empirical specification and discusses identification. Section 6 shows the main results on crime rates and section 7 presents results on police effectiveness and shows robustness checks. Section 8 explores local economic effects of enforcement, Section 9 discusses our results in relation to other policies aimed at reducing crime, and Section 10 concludes the paper.

2 Literature Review

While there is a series of studies by economists, sociologists and demographers on the correlation between immigration and crime, much less is known about the effectiveness of immigration enforcement in reducing crime. The second is a more specific question, but it is of great policy importance. First, policy evaluation is of direct interest to governments. Second, advocates of aggressive immigration enforcement policies often appeal to the need for public safety, hence implying that enforcement reduce crime. Evaluating the evidence of such claims is immediately relevant to forming effective public policy.

Butcher and Piehl were the first to show that immigrants are less likely to commit criminal offenses than natives (Butcher and Piehl, 1998a) and that, conditional on demographic characteristics, immigrants are less likely to be incarcerated (Bucther and Piehl

2007). Analyzing a panel of metropolitan areas, they show that there is no correlation between immigrant population share and crime, controlling for city demographic characteristics (Butcher and Piehl, 1998b). More recently Martinez, Stonewell and Lee (2010) find a negative correlation between homicides and immigration across census tracts in the San Diego metropolitan area, while Chalfin (2014) instruments for migration using rainfall shocks in Mexico and finds no correlation between immigration from Mexican and crime in the US. Likewise, Spenkuch (2014) does not find evidence of a correlation between immigration and violent crime in US states over three decades, but he does find a positive, though statistically insignificant, association with property crime. On the other hand Chalfin (2015) find that an increase in Mexican immigration, instrumented by the size of Mexican birth cohort, is associated with lower property crime and higher aggravated assaults.

Overall, this literature establishes that it is hard to find evidence of a positive relationship between immigration and crime in panel data. Several studies find that immigrants are less likely to commit crimes than similar natives, and this is now a well-established result. This paper asks a narrower and less understood question. Does immigration enforcement, in the form of deportations, reduce local crime? If immigrants targeted for deportation are more likely to be criminals then deporting them should reduce crime. And does it help local police to solve outstanding crimes and secure to justice its perpetrators?

The existing literature is sparse on both questions. Mastrobuoni and Pinotti (2015) and Baker (2014) analyze the effect of granting amnesty to undocumented immigrants on crime. These papers find that providing legal status reduces crime rates among immigrants. Ciancio (2017) uses variation from the drop in federal immigration enforcement that began in 2011 to examine crime rates and police efficiency. He finds that reductions in enforcement did not increase crime and may have increased police efficiency. Efficiency, measured as clearance rates could have increased with enforcement reductions because non-citizens were more willing to cooperate with the police, or because police could focus their efforts on more serious crimes rather than spending resources on immigration violations. Freedman, Owens and Bohn (2018) find that the 1986 Immigration, Reform, and Control Act increased crime among the recent immigrants most likely to be negatively impacted by IRCA employment regulations, suggesting that a lack of legal job opportunities drives crime; however, Freedman, Owens and Bohn (2015) suggest that IRCA changed police behavior, which may confound estimates of its effect on crime rates. We consider potential impacts of SC on police behavior by examining the local clearance rate, a measure of police efficiency, as well as considering potential employment effects of SC that might drive crime changes.

Miles and Cox (2014), the closest work to our research, also consider the effect of the

rollout of SC on local crime immigration enforcement under SC on crime; however, there are several contributions from our approach. First, we establish the role of SC in increasing deportation intensity and exploit variation across counties. This approach draws on the fact that communities with more undocumented immigrants are more affected by this policy. If SC impacts crime, we should see a larger effect in those places. Second, we provide additional validity checks by showing low correlation between the rollout of SC and pre-SC changes in crime rates, income, and the non-citizen population share. Third, we expand the analysis to look at potential effects of enforcement on local population, employment and firm creation, as well as discussing the potential channels through which changes in these variables can affect local crime. Finally, we analyze the effect of deportations on local police effectiveness and police resources, considering whether a larger or more effective police force accompanies higher enforcement. Overall, our analysis confirms some findings of Miles and Cox (2014), in that we also find small and generally not significant effects of deportation on crime. We also produce a more complete picture of the local effects of enforcement and deportations. We do not find any significant effect of deportations on crime reduction or police effectiveness, local population, firm creation, or low-skill employment.[3]

More papers have used variation in enforcement and deportation intensity from SC to examine other outcomes. East et al. (2018) find that SC adoption reduced the employment of low-skilled non-citizen workers and middle-skilled native workers. Wang and Kaushal (2018) find an increase in the share of Latino immigrants reporting mental health issues after SC, and Alsan and Yang (2018) find that SC decreased SNAP take-up and ACA sign-ups for Hispanic citizens, suggesting the presence of spillover effects from enforcement. Overall, the existing literature shows that increased enforcement, and specifically SC, has had unintended negative consequences on both US citizens and immigrants. This paper re-assesses the role of SC in increasing deportations and the impact of enforcement, as measured by deportations, on local crime, police effectiveness, and economic and demographic outcomes.

3 Deportations and Secure Communities

The Department of Homeland Security (DHS) introduced Secure Communities beginning in 2008 and expanding to the entire US by January 2013. The program flagged immigrants in violation of immigration laws when local authorities arrested them for any reason, vastly increasing the number of people deported from the US. Between SC's inception in October

[3]East et al. (2018) find negative effect on the employment of low-skilled non-citizen men and middle to high-skill citizen men, defining skill according to occupational skill rather than education.

2008 and its temporary suspension in October 2014[4], Immigration and Customs Enforcement (ICE) deported over 450,000 people under its jurisdiction.

Prior to SC, ICE and local law enforcement would have to collaborate on a case-by-case basis to check the immigration status of arrested persons. Therefore, police only checked the immigration records of a small fraction of people booked into jail. Under SC, police had to check the fingerprints and bio-metric information of any arrested person against the DHS immigration database, IDENT, in addition to the standard procedure of checking fingerprints against the FBI criminal database. Because federal authorities mandated the implementation of SC, local authorities could not opt out of the program[5]. DHS staggered the implementation of SC due to resource constraints, beginning in counties near the Mexican border in 2008 and expanding to the entire US by January 2013. In this paper, we take advantage of the variation in timing across counties from this staggered roll-out to identify the effects of deportation.

Any arrested person found in violation of immigration laws could be subject to deportation. Following a fingerprint match in IDENT, ICE could issue a detainer request to the local jail, telling local authorities to hold the individual for an additional 48 hours. During that period, ICE would take custody and potentially begin deportation proceedings. Noncitizens can be deported for a variety of reasons. For a person legally present in the country, felony convictions can result in deportation. For undocumented immigrants, even minor offenses can trigger deportations, as can an arrest resulting in no charges or no criminal conviction at all. Table 2 shows that nearly half of deportees under SC had only minor offenses as their most serious criminal conviction (MSCC) or no offense at all. Specifically, 5% had been convicted of a traffic violation, 8% had immigration violations, 11% had a DUI and 5% were convicted of marijuana possession. 16% of deportees had no conviction at all. In total, 45% of deportees had only minor convictions or no conviction, suggesting that the increase in deportations under SC was not necessarily concentrated on serious criminals.

Beyond whether SC benefited local communities by preventing crime, or increased crime due to a less efficient allocation of police resources, we may also be interested in the relative costs to evaluate the net contribution of the program. The costs of setting up SC are not entirely clear, but Congress appropriated funding to implement the program in 2008 and 2009[6]. However, local and state authorities undertook the cost of detaining

[4]The Trump administration reinstated the program as of January 2017.

[5]While there was some uncertainty as the program was implemented on whether counties could opt out, all counties eventually enrolled

[6]According to Alsan and Yang (2018), Congress appropriated $150 million in additional funding to ICE in FY 2009 for SC, as well as directing ICE to allocate an extra $850 million to the program. In FY 2010,

people prior to ICE taking custody [7]. These non-reimbursed costs could have led local authorities to redistribute their resources. If SC effectively targeted criminals or increased police effectiveness, this redistribution of resources may have helped reduce crime. However, if most people detained under SC were not serious criminals, and the program did not deter future crimes, then SC would be an ineffective and costly measure that may have further diverted resources in a way that reduced local capacity to fight crime. Despite the potential for resource displacement, we did not find evidence of any change in local resources.

4 Data and Trends

We merge data on deportations, immigration enforcement policies, crime, police performance, population, and economic outcomes from 2005 to 2015. Data on deportations under Secure Communities come from individual-level ICE records, which we obtained from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. These data contain each deportation record, including the location, the date of apprehension, the gender and the major criminal conviction of the deportee. The dates of SC implementation by county come from ICE publications[8]. Although these data only cover deportations under SC, so we cannot see prior patterns in deportations at the county level, Figure 1 shows the evolution of interior (non-border) ICE apprehensions in the US, which increased substantially after SC began in 2008. We also use information on the presence of county and state-level 287(g) agreements in each year. These voluntary agreements between ICE and local authorities deputized local police to enforce federal immigration law, including detaining people for suspected immigration violations[9]. These agreements were voluntary and their adoption signaled a more aggressive stance towards enforcement. We control for 287(g) agreements in some specifications, but do not include them in our instrument because the adoption was not random; rather, it was a decision by counties or states and likely to be strongly correlated with local attitudes towards immigration. In the case that these underlying attitudes are similarly correlated with crime, this would lead to biased estimates.

Congress increased the amount it directed ICE to allocate to SC to $1.5 billion. This suggests annual cost of SC in the first two years was over $1 billion.

[7] Some counties, such as Cook County, Illinois, passed local ordinances stating that they would not comply with ICE detainer request and citing the expenses that the county incurred from holding detained aliens for the additional 48 hours. For a list of county detainer policies, see ILRC (2015). For more background on the legal issues surrounding ICE detainer requests, see Manuel (2015).

[8] We thank Laura Bellows for sharing both the original ICE publications and her digitization of the roll-out-dates dates.

[9] We are grateful to Chloe East for sharing data on the presence of 287(g) agreements

We measure the presence of SC as the fraction of the year SC was in place in counties[10] As we discuss below, SC led to a large increase in deportations and its start date was staggered across counties, providing the first component of our identifying variation. One might be concerned that SC began earlier in counties with high crime rates, selected economic or demographic characteristics, or other unobservable features correlated with crime. While timing of the roll-out is correlated with some characteristics of counties such as the Hispanic share of the population and the distance from the border, we will show that there is no evidence of correlation with pre-2005 trends in economic and crime variables.[11]. The second source of identifying variation is the county population share of likely undocumented immigrants. Specifically, counties with a larger population of likely undocumented immigrants experienced larger increase in deportations per capita following the introduction of SC. While any non-citizen can be deported, undocumented immigrants are the most likely to be deported. As described below, our instrument proxies for the policy-driven increase in deportations by interacting the SC indicator with the population share of likely undocumented immigrants.

We obtain data on the likely undocumented immigrant population from the American Community Survey (ACS). Due to privacy concerns, the ACS does not provide county-level detail, so we aggregate the data by Public Use Microdata Area (PUMA) as our geographic unit of analysis.[12] We follow the literature in defining likely undocumented immigrants as low-education Mexican and Central American non-citizens who arrived in the U.S. after 1986.[13] We also use the ACS to measure population and employment across localities. We limit our sample to people between the ages of 16 and 64 when measuring population and employment rates in the ACS, and we exclude those living in group quarters. We define population in hundreds, so that variables defined relative to population can be interpreted as percents.[14] We fix the population values in the denominator to the 2005 level, so that our measures are not affected by potential endogenous changes of population after 2005.

[10]This definition follows East et al. (2018). We aggregate county data to the PUMA level and weight all variables by the PUMA population share attributed to each county in a given PUMA.

[11]Cox and Miles (2013) document the correlates with time of SC activation

[12]PUMAs can represent either aggregations of smaller counties or part of a larger county. They do not cross state boundaries

[13]We exclude non-citizens who entered the US prior to 1986 because the Immigration Reform and Control Act, passed in 1986, granted amnesty to many immigrants already in the US. Low-skill refers to people with a high-school degree or less. We grant that this is not a perfect proxy, but is the standard method in these data and allows a sub-state level of geography. See Warren (2014), Passel and Cohn (2016), and Capps et al. (2018) for a discussion of methods on estimating the US undocumented population.

[14]These variables include the population share of police and employment-to-population ratio. In calculating crime rates, we follow the FBI UCR in defining crime rates per 100,000 population.

Figure 2 shows the PUMA-level evolution of deportations relative to 2005 population (top panels) and SC activation (bottom panels) from 2008 through 2012. While all counties eventually adopt SC, deportation intensity does not increase uniformly across the US. Instead, deportation rates remain low in some places and increase substantially in others. A large part of this variation is due to geographic differences in undocumented immigrants, the population with the highest risk of deportation. Figure 2 also shows that counties at the U.S.-Mexico border adopted SC first; these counties also have a larger share of undocumented immigrants and may have different economic and demographic composition, or vary in unobservable dimensions correlated with crime rates. Therefore, our preferred specification excludes border counties.[15]

Table 1 describes the undocumented population share, the deportation rate, and deportations per undocumented across PUMAs.[16] The median deportation rate across PUMAs, relative to the total working-age population, is zero. As less than 1.6% of the the median PUMA population is likely undocumented, the share of people deported is relatively low even after the introduction of SC. However, the undocumented population constitutes over 15% of the population in places with the highest undocumented share. In places with the highest deportation intensity, ICE deported over 10% of undocumented in a given year, and 0.2% of the total population.

Figure 3 displays the total number of SC deportations and those associated with only minor and non-criminal offenses.[17] There is clearly a sharp increase in deportations as more counties implemented SC between 2008 and 2012. However, despite the jails being the initial contact between immigrants and potential deportation proceedings under SC, not all deportations removed serious criminals. As shown in 2 over 15% of deportations are for individuals without any criminal conviction, while minor offenses such as immigration violations and traffic offenses make up another 30% of deportations.

Data on crime rates by county and on reported offenses cleared by arrest comes from the FBI Uniform Crime Reporting, which we obtained through ICPSR. Our main dependent variable is the local crime rate, defined as the number of crimes reported per 100,000 population.[18] We separate crimes into violent crimes and property crimes, as the two types of

[15] We follow East et al. (2018) and Alsan and Yang (2018) in excluding border counties.

[16] These variables average the entire 2005-2015 period. Due to the nature of our data, which only measures deportations under SC, we do not observe deportations in the period prior to the SC rollout.

[17] Minor offense deportations refer to individuals whose most serious criminal conviction is an immigration violation, a traffic offense, or the possession of marijuana. Non-criminal deportations refer to individuals who have not been convicted of a crime.

[18] Our measure of county population is from the ACS.

crime often have different motives and consequences, and thus immigration enforcement may not impact them in the same way. On average there were 3033 property crimes per 100,000 people per year in this period and 427 violent crimes per 100,000 people per year.

Besides the impact on crime we are also interested in the impact of deportations, as a tool of law enforcement, on outcomes relative to police efficiency and police resources. We use police per capita and crime clearance rate as outcomes in our regressions. If the police resources did not increase with SC or with the increase in deportation, this may imply that Police had to divert some resources to implement this program. If a goal of SC was to give local police more tools to combat crime, especially crime committed by immigrants, one outcome could be higher ability to solve crimes and clear them with an arrest. Hence we use clearance rates as outcomes. We also examine the change in police per capita using the ACS data to measure employees in police occupations relative to local population.[19]

As a measure of police efficiency, we examine the clearance rate, or the share of reported offenses cleared by arrest. Specifically, the clearance rate is the total number of clearances divided by the difference of total offenses and unfounded offenses.[20] Because the UCR records clearance rate data by agency rather than county, and agencies may span multiple counties (or vice-versa), we aggregate to the county level. Our procedure allocates the total number of offenses and clearances for each category across the three largest counties covered by the agency in proportion to the population of the county within the borders of each jurisdiction.[21] We then calculate clearance rates at the county level for property offenses and violent offenses, and finally aggregate to the PUMA level as in the previous analyses. We express clearance rates in percents, or clearances per 100 offenses, and we exclude observations outside a range of two standard deviations from the mean clearance rate. Our sample excludes PUMAs with missing clearance rates and those with fewer than five years of data on clearance rates[22]. Table 1 describes the distribution of crime rates and clearance rates. The average clearance rate for violent crimes is over 52%, while the average clearance rate for property crimes is less than 18%.

Finally, we analyze whether deportation intensity had unintended effects on firm creation and employment. Data on firms by sector comes from the County Business Patterns.

[19]Specifically, we consider people employed as police, detectives, and private investigators, as well as other law enforcement officers: sheriffs, bailiffs, and correctional institution officers.

[20]Unfounded offenses are reported offenses that the police determine did not occur.

[21]Some agencies may map to only one county; others may cover multiple counties. Our data report only the three largest counties. Also note that a single county may include multiple agencies (for example, the state police and a local police agency).

[22]Excluding PUMAs with fewer than five years of data drops 1.5% of the sample. Results including these PUMAs are similar in both magnitude and precision.

We consider three types of sectors that employ a large share of undocumented immigrants. First, we examine firms in the agricultural industry, which we define according to the broader two-digit NAICS code and the narrower three-digit NAICS code. We also consider the construction industry and the service industry. We focus on the food service industry, building services, and landscape work as they employ the largest share of likely undocumented workers[23]. The dependent variable of interest in these specifications is the change in firms per 1,000 population. To examine employment shares of low-skill workers, we measure employed workers using the ACS and define skill according to education as before, where low-skill workers refers to individuals with a high school degree or less.

5 Empirical Model, identification and Discussion of Causality

Our main specification analyzes whether deportations affect local crime rates. We define deportations under SC relative to the working-age population to capture local enforcement intensity.

$$\Delta y_{c,t} = \alpha + \beta \frac{D_{c,t}}{Pop_{c,2005}} + \gamma X_{c,t} + \phi_c + \tau_t + \varepsilon_{c,t}$$

The dependent variable $\Delta y_{c,t}$ measures change in reported crimes per 100,000 residents in location c (PUMA) between year t and year $t-1$. Our analysis considers both property crimes and violent crimes. $\frac{D_{c,t}}{Pop_{c,2005}}$ is the number of deported people from location c in year t as a percent of 2005 working-age population. If SC effectively targets criminals, then deportations under this program should reduce the crime rate. $X_{c,t}$ includes controls for the manufacturing share and the construction share of employment in location c in year t, relative to total employment in 2005. The term τt represents year fixed effects, and ϕ_c represents PUMA-level fixed effects. Because the dependent variable is expressed in annual changes, ϕ_c captures a location-specific trend in the the level of the crime rate.

The goal of the analysis is to understand whether higher deportation rates, induced by tougher enforcement, reduce crime. Deportations could affect crime through multiple channels, primarily through the deportation of potential criminals, but also through changes in resource allocation and police efficiency or impacts on local labor markets and demographic

[23] We tabulate industries by share of likely undocumented workers using the ACS

composition. If deportations remove individuals who are more likely to commit crimes, then changes in the deportation rate should lead to a proportional reduction in the crime rate. However, OLS estimates of equation 5 are likely to be biased due to unobservable changes in local economic, political, and demographic conditions which may affect both deportations and crime rates. We address this problem by taking advantage of the staggered roll-out of SC across counties as quasi-experimental variation in the intensity of enforcement. Specifically, we instrument for the deportation rate, $\frac{D_{c,t}}{Pop_{c,2005}}$, with the interaction of an indicator for SC activation and the share of likely undocumented immigrants in 2005, or $SC_{c,t} * \frac{Undoc_{c,2005}}{Pop_{c,2005}}$.

As described above, proximity to the border was the main factor correlated with the timing of SC roll-out across countries. Table 3 shows a regression of time of SC activation on county characteristics. Time to activation is measured in months from January 2008. Again, we drop border counties from our sample, as they were the earliest to activate SC and may differ in other dimensions correlated with crime that would confound our estimates, and Table 3 examines correlates of SC activation among the remaining sample to ameliorate further concerns. Densely populated counties and counties with a larger Hispanic population share activated SC earlier (this is consistent with Miles and Cox (2014)). However, the time to activation is correlated with neither the trend in the average pre-activation (2005-2008) crime rate nor changes in construction and manufacturing employment that are indicative of the intensity of the Great Recession in the location.

As another source of variation comes from differential deportation intensity induced by differences in the initial population share of undocumented immigrants, Figure 4 shows an event study of deportations stratified by the 2005 undocumented share. The PUMAs with a high share of undocumented are those in the top quartile, and those with a low share undocumented are in the bottom quartile. As expected, most deportations occur in the counties with the largest share of people eligible to be deported. Table 4 shows the power of the instrument more formally. Column (1) shows the first stage regression of our main specification, which interacts SC with the population share of all likely undocumented immigrants as the group at risk of deportation. This regression suggests that SC activation increases the deportation rate by 0.007 percentage points for every percent of the population that is undocumented, or 33%relative to the mean deportation rate of 0.021. At the mean undocumented share of 3.3%, SC activation led to a 0.023 percentage point increase in the deportation rate. This coefficient is statistically significant and the F-statistic indicates it is a strong instrument. In column (2) we interact SC with the share of undocumented men in

constructing the IV, as men are at much higher risk of deportation[24] This regression suggests a 0.013% increase in deportations per year for every population percent undocumented when the SC program is activated.

6 Effects on Crime Rates

Table 5 shows the main 2SLS estimates from equation (5) when the dependent variable is the change in violent crime (Panel A) or property crime (Panel B)[25].

All regressions follow the specification in equation 5, with slight modifications. Column (1) is our preferred specification, which drops PUMAs bordering Mexico and includes controls for construction and manufacturing employment in addition to PUMA and year fixed effects. Column (2) drops controls for the manufacturing construction employment shares, while column (3) adds border PUMAs, which adopted SC earlier and where the rollout may be correlated with other factors related to crime.

Looking at the estimated coefficients, two results stand out. First, deportations have a small and non-significant impact on violent crime. To put the estimates in perspective, consider the relatively large increase in deportation rates between the median PUMA, with no deportations, and the 99th percentile PUMA, which deports 0.2% of its working age population per year. Our estimates suggest that a 0.2% increase in the deportation rate increases the violent crime rate by only 4.8 offenses per 100,000 persons per year, relative to the average violent crime rate of 654 offenses per 100,000. Not only is this positive coefficient inconsistent with deportations removing criminals and thus reducing crime, it is not statistically distinguishable from zero. Therefore, we do not find evidence that even a large increase in the local deportation rate was associated with any significant change in crime, particularly not a decrease in crime. Second, the impact of deportations on property crimes is also very small and positive, although it is statistically significant. In this case, a 0.2% increase in the deportation rate would increase property crimes by 205 per 100,000, relative to an average of 4534 property crimes per 100,000 people per year. Again, while statistically significant, this value is still quite small, and a positive effect is inconsistent with claims of deportations reducing crime.

[24] Approximately 96% of deportees are male in our data.

[25] Violent crimes include murder, manslaughter, rape, robbery, and aggravated assault. Property crimes include burglary, larceny theft, and motor vehicle theft. Crime variables are expressed as rates per 100,000 population.

While the roll-out of SC provides a more credible identification strategy, due to the quasi-experimental nature of its activation timing, we already mentioned an even more aggressive program during the same period, 287(g) agreements. These agreements between ICE and local law enforcement agencies gave police the authority to enforce immigration law more directly. Under the task-force model, the most forceful version of 287(g), local police had similar authority to ICE agents in arresting and detaining people for immigration violations. Only a select group of counties enacted such controversial policies, and entering 287(g) agreements likely corresponded to local preferences for an aggressive stand on immigration enforcement. To check whether the effect of deportations varied in places with 287(g) agreements, Table 6 includes an indicator for the PUMAs and years in which a 287(g) agreement was in place. The results for the change in violent crime rates (Panel A) and property crime rates (Panel B) are estimated using 2SLS and follow our preferred specification including controls for industry shares, PUMA and year fixed effects, and dropping border counties. Controlling 287(g) agreements does not change the estimated effect of deportations significantly. Considering the 287(g) indicator itself, we do not find a statistically significant association between 287(g) agreements and crime.

Next, we consider whether deportations had a measurable effect on the size of the low-skilled population, and specifically the low-skilled non-citizen or likely undocumented population. While low-skilled individuals, defined as those with a high school degree or less, are more likely to commit a crime[26], the average number of deportees per year was only 0.02% of the working-age population and likely too small to produce a measurable effect on crime. In principle, however, aggressive enforcement and deportations may induce other immigrants to leave, therefore magnifying any effects on local demographic composition. Table 7 examines the effect of deportations on changes in the low-skilled working-age population, the low-skilled non-citizen population, and the likely undocumented population. None of the estimated coefficients are statistically significant, and most estimates are positive[27]. This suggests that deportations did not have a significant effect on the size of the low-skilled population or the likely undocumented population in a PUMA and indicates that deportations did not affect crime through compositional change.

[26]Lochner and Moretti (2004), for instance, show that high school graduation and additional education are related to a significant decline in the probability of committing crimes, especially violent crimes.

[27]Bohn, Lofstrom and Raphael (2013) find that harsher sanctions of employers who hire undocumented immigrants, brought by the Legal Arizona Workers Act, reduced the state's likely undocumented population

7 Effects on Police Effectiveness and Police Resources

7.1 Clearance Rates

There is another potential role of the SC program and in general of providing more aggressive enforcement tools to local police. If immigrants, and especially undocumented immigrants, are involved in a large fraction of crimes, then the ability to detain them may be a tool for the police to pressure them and to solve crimes, ensuring more arrests of criminals. Alternatively, however, if few undocumented immigrants are criminals, then the procedures and costs involved in detaining them may divert resources from where they can be more productive in fighting crime. In this section we analyze the correlation between enforcement driven deportations and two measures related to the effectiveness of local police. One captures police efficiency in solving crimes, the other measures local police resources.

We measure police efficiency using clearance rates for violent and property crimes. The clearance rate is the number of crimes cleared by arrest[28], relative to all the reported (and confirmed) crimes (see Federal Bureau of Investigation (2017)). The clearance rate is significantly higher for violent crimes, averaging 52% from 2005-2015, than for property crimes, where the average clearance rate was only 18% from 2005-2015), and is a proxy for the investigative effectiveness of the police. Table 8 shows the impact of deportations on the annual change in PUMA-level clearance rates. The estimates are small, and only the estimate for the property crime clearance rate is statistically significant. These results imply that a large increase in the deportation rate of 0.2 percentage points will increase the clearance rate by about 0.23 percentage points for violent crimes (on an average of 53%) and by 0.04 percentage points for property crimes (relative to an average of 18%). These are negligible effects, supporting the hypothesis that increased enforcement did not result in efficiency gains for local law enforcement.

7.2 Police Resources

The mandate of SC to local police requires jails to keep people suspected of immigration violations in custody for up to 48 hours past the normal detention period, and this comes with a cost to local law enforcement. Jails must have space available to hold suspects, as well as agents to assist in the detention and transfer process. The federal government did not

[28]For a crime to be cleared, the police must arrest the perpetrator, who is then charged with the crime, and the government must begin prosecution.

directly appropriate funds to local agencies when implementing SC, so we must ask whether SC put a strain on local police resources.

First, we analyze whether counties with larger increases in the deportation rate also increased the number of law enforcement officers to manage any additional costs of the program. Specifically, we regress the change in number of police officers per 100 population the deportation rate at the PUMA-year level. Column (1) of Table 9 shows the reduced form and column (2) shows the estimates using 2SLS. These results show that deportation rates were not associated with changes police resources in this period. Therefore, counties with more deportations may have had to devote a larger fraction of prior local resources to enforcement, or divert them from other uses. This may be consistent with an actual increase in property crime rates due to more thinly spread resources for crime prevention[29].

7.3 Crime Reporting Rates

Official crime reports (such as the FBI Unified Crime Report) are based on reported offenses, so a concern related to any analysis relying on these data is that reporting rates, rather than in crime rates themselves, may have changed in response to a policy change. In our case, deportations may cause a change in crime reporting, perhaps due to fear of interacting with local law enforcement. If reporting changes are in the opposite (same) direction of crime changes, our analysis may underestimate (overestimate) the impact of enforcement on crime. Absent county-level data on crime reporting across the US, we cannot decompose the impact of deportations on reporting versus actual crime changes. However, we look at information on reporting rates by type of crime from the National Crime Victimization Survey across forty Metropolitan Statistical Areas (MSAs) in 1990 amd 2000. We match each MSA to its central county (or, in some cases, multiple counties), and obtain county-level data on reporting rates of violent and property crimes. Then, we analyze the correlation between the change in crime reporting rates and the change in the population share of foreign-born between 1990 and 2000.[30]

Figure 5 plots the correlation between the change in violent crime reporting (panel A) and property crime reporting (panel B) and the change in the foreign-born population. Both

[29] We do not find evidence, however, that controlling for changes in police resources attenuates the small positive effect of immigration enforcement on local property crime.

[30] In addition to standard categories of violent offenses and property offenses, the NCVS also includes reports of the verbal threat of assault. There are not many reports in this category, and we include it with violent crimes. We obtain data on the county-level population share of foreign-born from the NHGIS, which we to the MSAs in the NCVS

property crime and violent crime reporting rates are have a negative association with the change in foreign-born, although the correlation is only statistically significant for violent crime reporting. It is possible that deportations could decrease crime reporting, either through demographic changes or by changing the willingness of the immigrant community to report crimes to the police, we do not find any evidence of population changing as a result of SC. The small positive effect of SC on property crime could be biased down by a decrease in reporting, but violent crime, composed of more serious offenses, is less likely to be subject to under-reporting.

8 Local Economic Effects

While we do not find evidence for a significant impact of deportations on crime rates, the main motivation for more aggressive immigration enforcement, such policies may have unintended consequences on local economies. The removal of undocumented workers, or fear generated by deportations in immigrant communities, may disrupt sectors such as agriculture, construction, and service industries that employ a relatively large share of undocumented workers, reduce the employment rate of immigrant workers if undocumented employees are afraid to go to work, and potentially have spillover effects that reduce the employment of native workers. On the other hand, if deportations remove criminals, then more aggressive enforcement could attract more firms and increase employment.

Existing studies find negative employment effects of enforcement (East et al., 2018), and we supplement our analysis of crime by considering whether deportations affect local firm creation and employment using a different identification strategy. Table 10 considers the impact of deportations on the change in the number of firms relative to initial population in different sectors. We focus on industries that employ a larger share of low-skilled immigrants; specifically, we examine agriculture (Panel A), construction and food services (Panel B), and building and landscape services (Panel C). Column (1) of Panel A shows a possible negative effect of deportation on firm creation in agriculture, but the coefficient becomes positive, smaller, and only marginally significant when we define agriculture according to a stricter definition that excludes fishing and hunting. The estimates for other industries are not statistically significant and do not show convincing evidence of either a positive or negative effect of deportations on firm creation.

Table 11 analyzes the effect of deportations on the employment-to-population ratio for low-skilled workers and for low-skilled non-citizen workers. These estimates do not show any

statistically significant effect of deportations on employment. While our analysis focuses on crime rather than the potential economic effects of enforcement, these estimates do not show any support for claims that deporting undocumented immigrants will improve economic conditions or provide work opportunities for natives. Ifft and Jodlowski (2016) find evidence of a negative impact of 287(g) agreements on agricultural productivity and firms. While we find suggestive evidence to confirm this negative effect on firm creation in the broadly-defined agricultural sector, we do not find this effect using a narrower definition, nor do we find significant effects in other sectors. We also do not find any significant effects on low-skilled employment.

9 Comparison with other Crime-reducing Policies

A large literature in criminology has evaluated the effectiveness of different policing methods in reducing crime, especially violent crime and murder. In this section, we review some typical (or meta-) estimates of the effect of some these policies on crime and compare them to our estimates of the average change in violent crime due to ICE deportations of immigrants. As a case study, we consider the intensive immigration enforcement that occurred in Maricopa County, Arizona, where average deportation rate from 2008-2012 was 0.17% of the 2005 working-age population. We compare the estimated effect on crime in this county to estimates of crime reductions from well-known law-enforcement interventions designed to reduce crime rates. This allows us to consider whether small effects on violent crime are a constant feature of programs directed at fighting crime, or whether deportations have effectively no impact in comparison to more successful crime-reducing policies.

Maricopa County had the second largest number of total deportations from 2008-2012, with 20,603 immigrants deported under SC, and the third largest average deportation rate, as shown in Table 12[31]. Maricopa County is well-known for the extreme stance on immigration enforcement of local Sherrif Joe Arpaio, who held the office from 1993-2017 and was accused of police misconduct and racial profiling [32]. Our estimates, though not statistically significant, would predict that deporting such a large number of immigrants (0.173 percent of the working-age population, on average, in Maricopa County), increases the violent crime rate by 4.2 offenses per 100,000 working-age adults, relative to an average violent crime rate of 654 per 100,000. This impact is only 0.6% of the average violent crime

[31]This county, however, was not among those with highest total crime rate or the highest violent crime rate. Table 13 lists the areas with the highest crime rates over this period.

[32]See, for instance, the Wikipedia entry on Joe Arpaio, https://en.wikipedia.org/wiki/JoeArpaio

rate, and, though not statistically significant, the magnitude is positive, rather than the expected negative estimate if SC had successfully reduced crime.

Existing studies on crime reduction have several limitations. First, they often rely on correlation evidence in panel regression, rather than a well identified strategy to isolate a causal effect. Second, they often use a treatment and control framework to evaluate broad policies that act on many different dimensions at the same time. This makes it difficult to estimate a clear effect of a specific measure on crime rates. With these caveats in mind, we focus on three types of policies which share some underlying premises with immigration enforcement policies that are used to justify deportations as a crime-deterrent or crime-incapacitating policy.

The first type of policy is "broken window policies" that increases the probability of arrest and punishment for small crimes. The underlying assumption is that people who commit a misdemeanor would eventually be more likely to commit violent crimes. Therefore, being arrested earlier for a smaller crime prevents them, either through deterrence or incapacitation, from committing a more serious crime later. This premise has been criticized by several criminologists, but early studies of the connection between arrests for misdemeanors and crime found significant effects. Kelling and Sousa (2001) used precinct-level data from New York City to examine whether higher rates of misdemeanor arrest were associated with lower levels of crime, after taking account of other characteristics of the precincts. They concluded that aggressive misdemeanor arrests produced a statistically significant 5% reduction in violent crime. This would imply, in our sample, a reduction by 21 violent crimes per 100,000 individuals from an average of 416 crimes per 100,000. However, a more recent study by Rosenfeld, Fornango and Rengifo (2007) found smaller effects of increased misdemeanor arrests on crime incidence, and studies by Fagan and Davies (2003) and Harcourt and Ludwig (2006) found no evidence of a statistically significant effect at all. All of these studies are based on panel correlations and do not have an experimental or quasi-experimental design. A recent meta-analysis (Braga, Welsh and Schnell, 2015) of the relationship between misdemeanor arrests and crime, which includes experimental and quasi-experimental evidence, does not find significant evidence of misdemeanor arrests in reducing serious crime.

A second type of policy used to reduce crime is "Stop, question and frisk policies". Under such policies, police officers patrol areas by foot, and they actively stop pedestrians, question them, and often search them. This type of intervention is usually implemented in high-crime areas. While the correlational evidence on the the effectiveness of these methods is inconclusive, two randomized experiments in Philadelphia (the Foot Patrol Experiment, (Ratcliffe et al., 2011) and the Philadelphia Policing Tactics Experiment (Groff et al., 2015)),

allowed researchers to evaluate the impact of this type of method on randomly selected crime hot-spots. These studies find a 23% decrease in violent crime three months after the experiment. This effect corresponds to 96 fewer violent crimes per 100,000 citizens for the average PUMA in our sample. This would be a substantial decrease and much larger than the magnitudes of the effects we estimate in this paper.

Finally, a third type of intervention believed to be effective at reducing crime is the category of person-focused strategies and deterrence. These are strategies implemented to halt ongoing violence by gangs and other criminally active groups and to disrupt disorderly and violent drug markets. The idea is that by targeting groups likely to commit crimes and participating in crime-related activities, these policies will reduce violent crime. One important example of such a policy is Operation Ceasefire in Boston. The local police identified gangs and dangerous individuals, gave them warnings to deter crime, and used all available legal means to disrupt drug activity by enlisting informants, seizing drugs and proceeds, and maintaining strict enforcement of probation and parole. Several studies evaluated the effect of this policy on reducing youth homicide in Boston. They compare Boston with similar cities using a treatment-control analysis. Braga et al. (2001) find a 66% reduction of youth homicide and a 25% reduction in gun violence, which are remarkable results. Such results correspond to a decline of between 104 and 276 violent crimes per 100,000 people in the average PUMA in our sample. Again, this is a substantially larger impact than the deportation policy that we analyze in this paper.

While the policies considered above often involve a combination of actions, their estimated impact on violent crime rates give us an idea of the magnitude of effects one can expect form effective crime reducing policies[33]. In general, policies targeted to individuals more likely to be involved in violent crime (such as gangs and serious drug traffickers) are the most effective at reducing violent crime and can have strong impacts. Policies that target high crime areas and use foot patrols to stop, question and frisk may also be quite effective. There is much less evidence on the effectiveness of arresting people for committing minor crime (misdemeanors) as a way to prevent more serious crime. Our findings are consistent with these results: targeting a population that has a low probability of committing crimes, such as the undocumented immigrants, with deportations does not affect crime rates through deterrence or through incapacitation. Using deportation as a way to reduce crime can be seen as a more extreme extension of a policy that targets people for very minor violations under the assumption that they may commit more serious crimes in the future; however,

[33]For a more systematic summary and evaluation of the recent literature on policies reducing crime rates see National Academies of Sciences (2018).

empirical evidence does not support this argument.

10 Conclusions

Using county-level variation in deportation increases due to SC, we analyze the potential effects of deportation on local crime rates and police efficiency. We instrument for deportations using the staggered roll-out of SC interacted with the initial presence of likely undocumented immigrants in a county. If SC was effective at targeting serious criminals and removing them from the US, these deportations should decrease crime. However, we find that higher deportation rates are not associated with lower crime rates. In fact, accounting for the potential endogeneity of deportations, we find a very small and positive, though usually non-significant, effect of deportations on crime. In considering the potential mechanisms for a possible effect, or lack of effect, of SC on crime, we document that more enforcement-driven deportations do not increase police efficiency in solving criminal cases, nor they bring more police resources to the local community. Similarly, higher deportation rates do not attract businesses or increase job opportunities for low-skilled workers.

Of course, there are still caveats to our analysis. For example, if there are differential deviations from average changes in crime rates across counties with large and small shares of undocumented immigrants that are correlated with confounding factors, such as changes in local attitudes towards immigration, our location and time fixed effects will not account for such remaining confounding variation. However, we show that pre-existing trends in crime and economic characteristics are not correlated with the timing of SC activation, addressing concerns about endogenous program implementation, and we include controls for manufacturing and construction employment shares in an attempt to account for potential effects of the Great Recession that vary across places. Overall, our analysis suggests that there is not empirical support for the claims of the Trump Administration and other public figures that immigrant deportations reduce crime and make communities safer.

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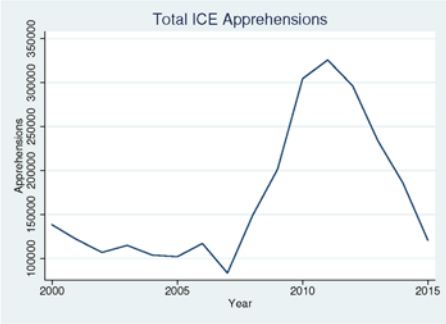
Figure 1: Total Interior Apprehensions: 2000-2015



Notes: Figure displays total yearly apprehensions by ICE Investigative Districts and ICE ERO.
Data source: Department of Homeland Security Yearbook of Immigration Statistics.

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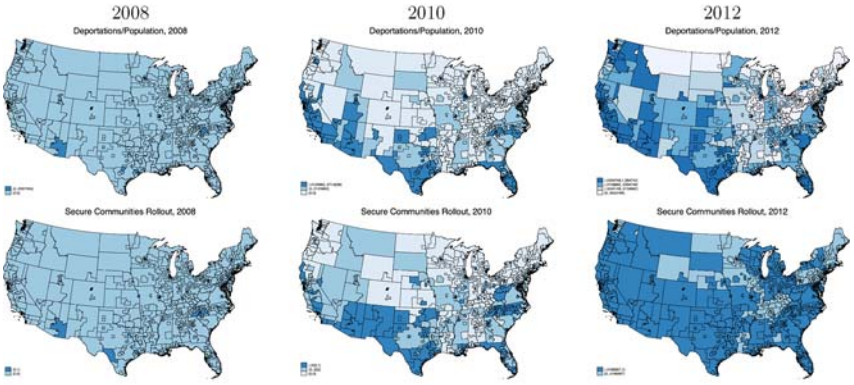
Figure 2: Deportation Rate and SC Rollout by PUMA



Notes: Map displays PUMA-level deportations per 100 working-age population in 2005. Population data are from the American Community Survey. Population measures total working age (16-64) population at the PUMA-level. Rollout of SC at the PUMA level is the average of counties in each PUMA, weighted by population. The variable is also weighted to account for the fraction of the calendar year during which SC was active.

Table 1: Deportations and Undocumented Share

	Mean	SD	Median	99th percentile
Undoc/Pop (pct), 2005	3.306	4.201	1.633	16.607
Deportations/Pop. (pct)	0.021	0.068	0.000	0.221
Deportations/Undocumented	0.998	3.195	0.000	11.236
Property crimes per 100,000 pop.	4783.894	1922.003	4444.875	10099.227
Violent crimes per 100,000 pop.	671.965	411.514	612.149	2137.917
Clearance rate - property (pct)	17.730	7.264	17.212	36.169
Clearance rate - violent (pct)	52.165	18.145	53.639	87.780

Notes: Population data are from the 2005-2014 American Community Survey. Undocumented/Pop is the undocumented share of the population (undocumented per 100 total working-age population). The deportation rate is the number of deportations in a given year divided by 100 working age population. Deportations/Undocumented is the number of deportations divided by 100 undocumented population. Deportation rates are for the entire period, coding pre-SC deportations to zero. All denominators for population are fixed to 2005 values. Undocumented immigrants are defined as non-citizens with a high school degree or less, arrived in the US after 1986, and are from Mexico or Central America (excluding Cuba). Low-skilled individuals are those with a high school degree or less. All variables are collapsed at the PUMA-Year level and weighted by the 2005 PUMA population.

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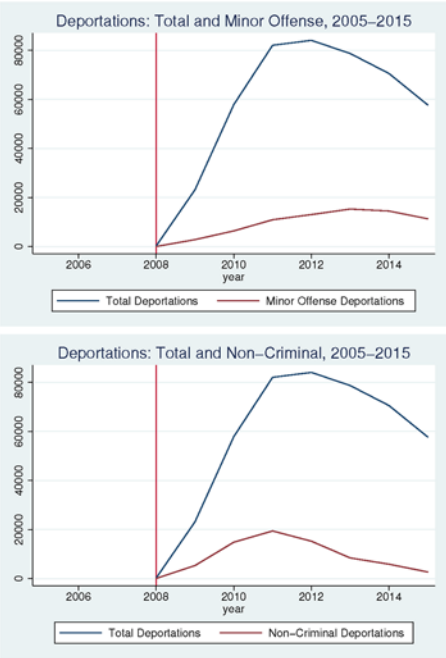
Figure 3: Total Deportations vs. Minor Offenses



Notes: Minor crimes are disorderly conduct, marijuana possession, traffic offenses (excluding DUIs), and immigration offenses (Illegal Entry, Illegal Re-Entry, and Possession of fraudulent immigration documents). Data obtained from ICE records via the Transactional Records Access Clearinghouse. We categorize crimes according to the most serious criminal conviction of the deportee (note that this is not necessarily the crime for which the person was apprehended prior to removal). Data not available prior to 2008.

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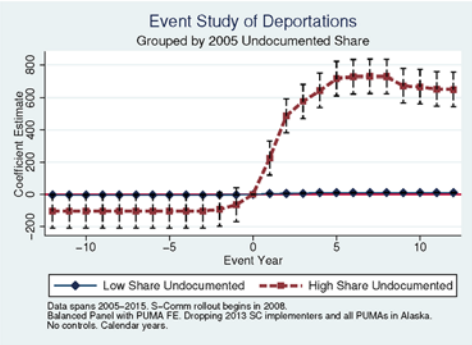
Table 2: Share of Deportations by Most Serious Criminal Conviction

Traffic MSCC (%)	5.18
Immigration MSCC (%)	8.39
DUI MSCC (%)	11.34
Marijuana MSCC (%)	5.16
No MSCC (%)	15.83
Other MSCC (%)	54.11

Figure 4: Event Study of Deportations by 2005 Undocumented Share



Notes: Figure displays an event study of deportations relative to SC activation across PUMAS. Deportation data comes from TRAC, and covers the 2008-2015 time period. In years and locations prior to the data coverage, deportations under SC take a value of zero.

Table 3: Regression of Time to SC Activation on 2008 county characteristics (cross-section)

	Dep. Var: Quarters to Activation	
	(1)	(2)
Avg. crime rate change 2005-2008	0.000	0.000
	(0.000)	(0.000)
Non-Citizen/Pop. (percent)	0.000	0.000
	(0.000)	(0.000)
Hispanic/Pop. (percent)	-0.001***	-0.001***
	(0.000)	(0.000)
ln(Population)	-1.316***	-1.312***
	(0.116)	(0.117)
Average total income (1000s)	0.015	0.015
	(0.012)	(0.012)
Avg. manuf. change 2005-2008		4.849
		(19.354)
Avg. constr. change 2005-2008		-14.087
		(20.725)
Border PUMAs	No	No
Y Mean	11.7095	11.7095
Observations	1044	1044
R2	0.2002	0.2006

Notes: Population data are from the 2005-2014 American Community Survey. Secure Communities Activation defined by quarter, relative to the first quarter of 2008. Controls include total crimes per 100,000 population, an indicator for PUMAs on the US-Mexico border, the non-citizen% of the population (non-citizens per 100 total population), the Hispanic% of the population (Hispanic per 100 total population), the log of the total working-age population, and average total income per person, in 2008 dollars in 1000s. All regressions are weighted by the 2005 PUMA population. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Dependent variable: Interaction between 2005 undocumented share and SC

	(1) Deport/Pop	(2) Deport/Pop
(Undoc/Pop)_05 *SC	0.007***	
	(0.001)	
(MaleUndoc/Pop)_05 *SC		0.013***
		(0.001)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Y Mean	0.0203	0.0203
Observations	9396	9396
R2	0.6376	0.6345
F	48.34	47.75

Notes: Population data are from the 2005-2014 American Community Survey. Deportations and detainers are collapsed at the PUMA-year level. The dependent variable is the deportation or detainer rate in each PUMA-year, calculated as the number of deportations divided by 100 working age population in 2005 (ages 16-64) . Undocumented immigrants are defined as non-citizens with a high school degree or less, arrived in the US after 1986, and are from Mexico or Central America (excluding Cuba). The regressor of interest is the interaction between the undocumented percent of the population in 2005 (undocumented/100 population) and the presence of Secure Communities (SC). The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 5: IV: Crime Rate

	Dep. Var:		
	(1)Δ Violent/Pop	(2)Δ Violent/Pop	(3)Δ Violent/Pop
A: Violent Crimes			
Deport/Pop	24.296	24.275	24.572
	(40.324)	(40.221)	(33.094)
Border PUMAs	No	No	Yes
Industry Shares	X		X
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	654.631	654.631	654.631
Observations	9396	9396	9594
R2	0.1445	0.1440	0.1430
	Dep. Var:		
	(1)Δ Property/Pop	(2)Δ Property/Pop	(3)Δ Property/Pop
B: Property Crimes			
Deport/Pop	1028.672**	1027.653**	760.910**
	(422.543)	(424.510)	(355.386)
Border PUMAs	No	No	Yes
Industry Shares	X		X
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	4534.522	4534.522	4534.522
Observations	9396	9396	9594
R2	0.1446	0.1444	0.1319

Notes: Population data are from the 2005-2014 American Community Survey. The total crime rate is the sum of violent crimes and property crimes. Crime rates are crimes divided by 100,000 working0-age population in 2005. Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Change in Crime Rates, 287g controls

	Dep. Var:	
	(1)Δ Violent/Pop	(2)Δ Violent/Pop
A: Violent Crime		
Deport/Pop	24.296	20.404
	(40.324)	(39.421)
Any 287(g)		5.016
		(6.382)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	654.631	654.631
Observations	9396	9396
R2	0.1445	0.1448
	Dep. Var:	
	(1)Δ Property/Pop	(2)Δ Property/Pop
B: Property Crime		
Deport/Pop	1028.672**	988.829**
	(422.543)	(448.390)
Any 287(g)		51.349
		(32.659)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	4534.522	4534.522
Observations	9396	9396
R2	0.1446	0.1457

Notes: Population data are from the 2005-2014 American Community Survey. Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 7: IV: Population Shares

	Dep. Var:		
	(1)Δ LS/Pop	(2)Δ LS/Pop	(3)Δ LS/Pop
A: LS Pop			
Deport/Pop	0.759	0.710	1.618
	(2.486)	(2.210)	(1.796)
Border PUMAs	No	No	Yes
Industry Shares		X	
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	51.079	51.079	51.079
Observations	10520	10520	10740
R2	0.0634	0.1167	0.1167
	Dep. Var:		
	(1)Δ LSNC/Pop	(2)Δ LSNC/Pop	(3)Δ LSNC/Pop
B: LSNC Pop.			
Deport/Pop	0.917	0.884	0.638
	(1.405)	(1.332)	(1.072)
Border PUMAs	No	No	Yes
Industry Shares		X	
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	6.12	6.12	6.12
Observations	10520	10520	10740
R2	0.0280	0.0535	0.0539
	Dep. Var:		
	(1)Δ Undoc./Pop	(2)Δ Undoc./Pop	(3)Δ Undoc./Pop
C: Undoc. Pop.			
Deport/Pop	-0.152	-0.175	0.019
	(0.959)	(0.947)	(0.772)
Border PUMAs	No	No	Yes
Industry Shares		X	
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	3.105	3.105	3.105
Observations	10520	10520	10740
R2	0.0218	0.0414	0.0419

Notes: Population data are from the 2005-2014 American Community Survey. Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 8: IV Change in Clearance Rate by type

	Dep. Var:	
	(1)Δ Violent Clr Rate	(2)Δ Property Clr Rate
Deport/Pop	6.331	-3.721**
	(5.424)	(1.566)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	52.897	18.483
Observations	7911	7911
R2	0.0484	0.0679

Notes: Data on clearance rates come from the FBI UCR, available through ICPSR. Clearance rates are the percent of reported offenses cleared in a given year. The denominator is the difference between total reported offenses and unfounded offenses. The sample is restricted to PUMAs with defined clearance rates and drops those with fewer than five years of data. Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

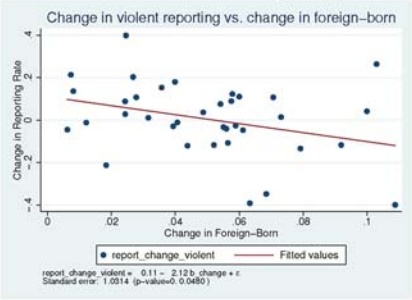
Table 9: Change in Police/Population

	Dep. Var:	
	(1) RF: Δ Police/Pop	(2) IV: Δ Police/Pop
(Undoc/Pop)_05 *SC	-0.000	
	(0.001)	
Deport/Pop		-0.034
		(0.150)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	.855	.855
Observations	9396	9396
R2	0.0180	0.0180

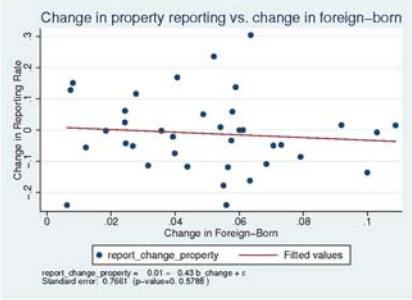
Notes: The dependent variable is the annual change in police per capita, where police per capita is defined as the number of police per 100 population in 2005, calculated using the ACS.Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Figure 5: Change in crime reporting vs. change in foreign-born population

a. Change in violent crime reporting rate



b. Change in property crime reporting rate



Notes: Crime reporting data come from the National Crime Victimization Survey (NCVS). County-level data on the population share of foreign-born comes from the NHGIS. Change refers to the change in the share of crimes reported or the change in the foreign-born share of total population between 1990 and 2010. The sample covers 37 MSAs in the US.

Table 10: IV: Change in Firms (per population)

	Dep. Var:	
	(1)Δ Firms (Agr.)/Pop	(3)Δ Firms (Agr2)/Pop
A: Firms: Total and Agr		
Deport/Pop	-1.579**	0.897*
	(0.769)	(0.490)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	11.68	5.61
Observations	9396	9396
R2	0.1668	0.0834
	Dep. Var:	
	(1)Δ Constr.	(2)Δ FoodSvc
B: Firms: ACS Ind		
Deport/Pop	13.049	5.977
	(19.436)	(5.304)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	382.51	312.29
Observations	9396	9396
R2	0.5327	0.3240
	Dep. Var:	
	(1)Δ BldgSvc	(2)Δ Landscape
C: Firms: Svc Ind, detail		
Deport/Pop	0.912	0.701
	(1.736)	(1.210)
Border PUMAs	No	No
Industry Shares	X	X
PUMA FE	X	X
Year FE	X	X
Avg. Rate	94.7	49.81
Observations	9396	9396
R2	0.2140	0.1829

Notes: Firm data come from the County Business Patterns survey, with firms defined as the number of establishments in a given PUMA, by industry. The dependent variable is the change in firms per 100,000 population, with population fixed to its 2005 level. Panel A shows results for all industries, agricultural industries according to the two-digit NAICS code, and agriculture more narrowly defined according to the three-digit NAICS code. Panel B shows results for construction, food, services, and other support services, according to the two-digit NAICS codes. These are the dominant industries employing low-skilled non-citizens and likely undocumented workers in the ACS. Panel C shows results for all support services, as well as separating building services and landscape work by the three digit NAICS codes. Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 11: IV: Employment Shares

	Dep. Var:		
	(1)Δ LS Emp./Pop	(2)Δ LS Emp./Pop	(3)Δ LS Emp./Pop
A: LS Employment			
Deport/Pop	2.603	2.570	2.104
	(1.956)	(1.692)	(1.401)
Border PUMAs	No	No	Yes
Industry Shares		X	
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	30.854	30.854	30.854
Observations	10520	10520	10740
R2	0.1130	0.1630	0.1621
	Dep. Var:		
	(1)Δ LSNC Emp./Pop	(2)Δ LSNC Emp./Pop	(3)Δ LSNC Emp./Pop
B. LSNC Employment			
Deport/Pop	0.812	0.785	0.374
	(1.190)	(1.113)	(0.893)
Border PUMAs	No	No	Yes
Industry Shares		X	
PUMA FE	X	X	X
Year FE	X	X	X
Avg. Rate	4.001	4.001	4.001
Observations	10520	10520	10740
R2	0.0258	0.0525	0.0538

Notes: Population data are from the 2005-2014 American Community Survey. Deportations and detainers are collapsed at the PUMA-year level. The low-skilled population is defined as working-age (16-64) individuals with a high school degree or less. SC is a weighted average of all counties in a PUMA and weighted by the month of implementation in the implementation year. All regressions are weighted by the 2005 PUMA population and include PUMA and year fixed effects. Standard errors are clustered by PUMA and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 12: Locations with the highest deportation intensity

Rank	PUMA	Avg. Deportation/Pop (2008-2012)
1	Walker County, TX	0.751
2	Collier County, FL	0.192
3.	Maricopa County, AZ	0.173
4.	Santa Barbara County, CA	0.155
5.	Harris County, TX	0.150
Rank	PUMA	Total Deportations, 2008-2012
1.	Houston, TX	27197
2.	Maricopa County, AZ	20643
3.	Orange County, CA	12914
4.	Dallas, TX	11600
5.	Central Texas, TX	8173

Notes: The deportation rate is the number of deportations in a given year divided by 100 working age population, based on data on deportations under SC from TRAC. The population denominator is fixed to 2005 levels using data from the ACS. Deportations prior to SC implementation are coded to zero. The table ranks PUMAs by total deportations and the average deportation rate, and it lists the main county or metropolitan area in that PUMA as the location. In the case that multiple PUMAs in the top 5 ranking are in the same county or city, the deportation number is an average of those PUMAs.

Table 13: Locations with the highest crime rates

Highest Crime	PUMA	Total Crime Rate (per 100,000)
1	St. Louis, MO	16631.26
2	Pulaski County, AR	13113.20
3.	Richmond County, GA	11961.59
4.	Marion County, IN	11795.02
5.	Bibb County, GA	11788.56
Highest Crime	PUMA	Violent Crime Rate (per 100,000)
1.	St. Louis City, MO	3142.09
2.	Baltimore City, MD	2506.96
3.	Philadelphia, PA	2462.53
4.	District of Columbia	2456.91
5.	Memphis, TN	2145.50

Notes: Crime rates defined as total offenses or violent offenses per 100,000 working-age population. The population denominator is fixed to 2005 levels using data from the ACS. Data on crimes reported comes from the FBI UCR. The table ranks the highest crime PUMAs and lists the main county or metropolitan area in that PUMA as the location. In the case that multiple PUMAs in the top 5 ranking are in the same county or city, the ranked crime rate reflects the average crime rate for those PUMAs.

Law-Abiding Immigrants: The Incarceration Gap Between Immigrants and the US-born, 1870–2020*

Ran Abramitzky (Stanford and NBER)
Leah Boustan (Princeton and NBER)
Elisa Jácome (Northwestern and NBER)
Santiago Pérez (UC Davis and NBER)
Juan David Torres (Stanford)

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Abstract:

We provide the first nationally representative long-run series (1870–2020) of incarceration rates for immigrants and the US-born. As a group, immigrants have had lower incarceration rates than the US-born for 150 years. Moreover, relative to the US-born, immigrants' incarceration rates have declined since 1960: immigrants today are 60% less likely to be incarcerated (30% relative to US-born whites). This relative decline occurred among immigrants from all regions and cannot be explained by changes in immigrants' observable characteristics or immigration policy. Instead, the decline is part of a broader divergence of outcomes between less-educated immigrants and their US-born counterparts.

* Abramitzky: Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305, and NBER (e-mail: ranabr@stanford.edu); Boustan: Princeton University, Department of Economics, Princeton, NJ 08544 and NBER (email: lboustan@princeton.edu); Jácome: Northwestern University, Department of Economics, Evanston, IL 60208 and NBER (email: ejacome@northwestern.edu); Pérez: Department of Economics, University of California, Davis, 1 Shields Ave, Davis, CA 95616, and NBER (e-mail: seperez@ucdavis.edu); Torres: Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305 (e-mail: jdtorres@stanford.edu). We thank Jing Wu for sharing historical prison admissions data from Missouri. We are grateful to Amy Finkelstein and five anonymous referees for most useful editorial guidance. We have greatly benefited from comments from Alvaro Calderón, Jenna Kowalski, Giovanni Peri, Ernesto Tiburcio Manón, Steven Messner, Carolyn Moehling, Melanie Morten, Aurelie Ouss, Jeremy Weinstein, and Gavin Wright, as well as seminar participants at the All CA-Mexico Economics of Migration seminar series, the ASSA annual meeting, Berkeley University, The Bissell-Heyd Symposium at the University of Toronto, the Center for Economic History reception at Northwestern University, Demography workshop at the University of Chicago, Harvard Kennedy School, NBER conference on Immigrants and the U.S. Economy, Royal Economics Society, Seymour E. and Ruth B. Harris Lecture Series at Harvard University, SITE conference on migration at Stanford University, University of British Columbia, University of Southern California and the WZB Berlin Social Science Center.

The tendency to associate immigration and crime has been pervasive throughout US history. For example, in 1891, Senator Henry Cabot Lodge advocated closing the border, warning that Italian immigrants were "members of the Mafia, a secret society… using murder as a means of maintaining its discipline" (Lodge 1891). Indeed, over the past 150 years, Congressional speeches about immigration were twice as likely to mention words related to crime (per speech) than were speeches on other topics (Card et al. 2022).

Contrary to this anti-immigrant rhetoric, we document that, as a group, immigrant men have had a *lower* incarceration rate than US-born men for the last 150 years of American history.[1] We combine newly assembled full-count Census data (1870–1940) with Census/ACS samples (1950–2020) to construct the *first nationally representative* series of incarceration rates for immigrants and the US-born between 1870 and the present day. From 1870 to 1950, immigrants' incarceration rate was only slightly lower than that of US-born men. However, starting in 1960, immigrants have become *significantly less* likely to be incarcerated than the US-born, even though as a group immigrants now are relatively younger, more likely to be non-white, have lower incomes, and are less educated – characteristics often associated with involvement in the criminal justice system.[2] Today, immigrants are 60% less likely to be incarcerated than all US-born men, and 30% less likely to be incarcerated relative to white US-born men. The similar incarceration rates between immigrants and the US-born in the past and the lower incarceration rates of immigrants today are broadly consistent with prior studies documenting immigrant-US-born incarceration gaps for specific states and time periods (Moehling and Piehl 2009, 2014; Butcher and Piehl 1998b, 2007).

With access to large samples, including the full-population Census before 1950, we are also able to provide the first investigation of incarceration rates by country of origin spanning 1870 to 2020. We find a substantial decline in incarceration rates relative to the US-born among immigrants from *all major sending regions*. European immigrants historically had slightly lower incarceration rates to US-born men, but recently experience far lower incarceration rates. Chinese immigrants had similar incarceration rates to the US-born before 1960, but today have significantly lower

[1] We focus on men because men constitute the vast majority of the incarcerated population both today and in the past (Freeman 1999). Our takeaways are unchanged if we include women (Figure A10).
[2] On average, immigrants were older than US-born male adults from 1870–1970 but have been relatively younger in the past 50 years. The share of immigrants that are Black, which used to be close to zero, has also grown since 1950; roughly 10% of immigrants are Black today.

incarceration rates. Mexican and Central American immigrants had particularly high incarceration rates in the past but have had lower incarceration rates than the US-born since 1960. From 2005 on, Mexican and Central American immigrants have been more likely to be incarcerated than *white* US-born men, although we note that a large portion of the increase in Mexican and Central American incarceration after 2005 is driven by detentions in federal immigration facilities, often for immigration-related offenses; when we drop areas home to the largest Immigration and Customs Enforcement (ICE) facilities, the gap relative to US-born white men moderates or disappears in most years.

Although our data do not enable us to precisely pinpoint why there has been a sharp relative decline in the immigrant incarceration rate since 1960, we are able to rule out three ex-ante plausible explanations. First, the relative decline in immigrant incarceration is not driven by rising rates of incarceration of US-born Black Americans; the decline is also apparent when comparing immigrants to US-born *white* men only. Second, the decline is not driven by changes in immigrants' observable characteristics, namely, their countries of origin, age, race, marital status, state of residence, or educational attainment. If anything, immigrants' lower educational attainment in recent decades would predict that they should have higher incarceration rates than they do. Third, the relative decline is not mechanically driven by immigrant offenders being more likely to be deported in recent years (and thus not being present in the incarceration data): the relative decline in incarceration is present even among immigrants who are US citizens and thus cannot be deported. Moreover, the timing of the decline is also inconsistent with this explanation; whereas the relative decline in immigrant incarceration emerges in the 1960s, the sharp rise in deportations took place around 2000.

We conclude the paper by showing that lower-educated immigrants and US-born men (the group that accounts for the vast majority of incarcerated individuals) *not only* diverged in their incarceration propensities in recent decades, but also diverged at a similar moment along other dimensions, including their labor force participation and likelihood of marriage. One potential explanation for this broad pattern of divergence is that less-educated immigrants might have remained relatively shielded from structural changes in the economy – such as globalization and skill-biased technological change – that negatively affected less-educated US-born men in recent decades. Immigrants are concentrated in manual tasks and service occupations (rather than routine

occupations), which did not experience large wage or employment declines in recent decades (Autor et al. 2006; Peri and Sparber 2009). Furthermore, immigrants may be more resilient to shocks, given that they are a self-selected group of individuals possessing traits such as a greater willingness to move long distances (Cadena and Kovak 2016), less risk aversion (Jaeger et al. 2010), higher adaptability and cognitive ability (Bütikofer and Peri 2021), and higher levels of entrepreneurship (Azoulay et al. 2022).

Related literature. Our work is most closely related to a set of papers that document immigrant-US-born incarceration gaps for specific states and time periods (summarized in Figure A1). Moehling and Piehl (2009) studies historical flows into prisons using state prison records from 1904, 1910, 1923, and 1930. Moehling and Piehl (2014) studies historical incarceration rates in eight states by locating individuals residing in state correctional facilities in full-count Census samples between 1900 and 1930. Consistent with our series, these papers find that immigrants' incarceration rate was similar, if not slightly lower, than that of the US-born during the Age of Mass Migration.[3] Butcher and Piehl (1998b, 2007) use 1980–2000 Census subsamples to compare immigrants' incarceration propensities to those of US-born men.[4] These studies find that recent immigrants have been less likely to be incarcerated than US-born men and that this difference widened between 1980 and 2000.[5]

Relative to these papers, we provide the first nationally representative, century-and-a-half-long series on the incarceration gap between immigrants and the US-born.[6] Our long-run perspective enables us to document that immigrants not only have lower incarceration propensities than the

[3] We used our methodology to compute incarceration rates in the eight states in Moehling and Piehl (2014) and find higher levels, although similar trends, of incarceration rates for immigrants and US-born men, presumably because our data include federal prisons and local jails.

[4] Using 2012–2018 Texas arrest records, Light et al. (2020) finds that unauthorized and legal immigrants are less likely to be arrested than US-born citizens. Landgrave and Nowrasteh (2017, 2018, 2019) show that immigrants have lower incarceration rates than the US-born in the 2014–2016 ACS. Related work in criminology and sociology confirms that immigrants today are less crime-prone than their US-born counterparts (e.g., Bersani 2014, Bucerius 2011, Sampson et al. 2005, and Kubrin and Ousey 2023 and cites therein).

[5] Figure A1 plots the incarceration rates of immigrants and US-born men from Moehling and Piehl (2014) and Butcher and Piehl (2007). The figure makes clear that despite the great progress made by these earlier studies, there were still significant gaps in our knowledge of immigrant-US-born incarceration gaps (the pre-1900 period, nationally representative coverage for 1900–1930, the 1930–1970 period, and the post-2000 period).

[6] A nationally representative series is key for studying the immigrant-US-born incarceration gap, as the gap can differ substantially across states. For example, in 1920 and 1930, 15 and 9 states had positive incarceration gaps (immigrants were *more* likely to be incarcerated than the US-born), respectively, whereas 13 and 24 states had negative gaps.

US-born today, but that they have experienced similar or lower incarceration rates than the US-born *throughout American history*. Importantly, we also document *when* the immigrant-US-born incarceration gap began to widen (circa 1960) and we rule out candidate explanations (i.e., rising deportations) for why such divergence has taken place. Finally, unlike prior work focusing on specific states and periods, our large samples allow us to create a consistent long-term series, disaggregate the series by country-of-origin group, and document that the relative decline in immigrant incarceration applies to immigrants from all regions.

This paper is also related to a large literature for the modern period studying how changes in the number of immigrants affect local crime rates (Adelman et al. 2017, Butcher and Piehl 1998a, Chalfin 2014, and Spenkuch 2014 in the US; Akbulut-Yuksel et al. 2022, Bell et al. 2013a, Bianchi et al. 2012, Gehrsitz and Ungerer 2022, and Piopiunik and Ruhose 2017 in Europe, among many others). A number of papers, primarily those based on European data, find that recent waves of immigrants *increase* crime rates. Others, including those based on US data, find null effects.[7] We contribute to this literature by documenting that immigrants *themselves* have been less likely to be incarcerated than the US-born for the last 150 years. If immigrant arrivals have no effect on crime rates (despite immigrants themselves being less prone to crime), one possibility is that the presence of immigrants increases the criminal propensities of other groups (e.g., by increasing population growth or racial diversity in local areas).

Finally, our study contributes to the literature studying long-term changes in immigrants' outcomes in the US (Abramitzky et al. 2020, 2021). We contribute to this literature by providing a past-present comparison on an as-yet unexplored dimension of immigrants' performance: incarceration rates.

1. Data and Methods

Sources. Ideally, to compare the criminality of immigrants and the US-born, we would want to measure whether an individual committed a crime. However, such data are not available because

[7] We refer the reader to Bell and Machin (2013b), Buonanno et al. (2022), Fasani et al. (2019) and Orrenius and Zavodny (2019) for reviews of the immigration-crime literature.

many crimes are not reported and many offenders are not arrested. As a result, two common proxies for crime are arrests and incarceration. We rely on incarceration as our proxy.

The advantage of using incarceration is that it can be measured in the Census, enabling us to build a nationally representative series on incarceration *by birthplace* starting in 1870.[8] For the 1870–1940 period, we use the full-count Census (Ruggles et al. 2021) to observe the universe of prisoners in the US every ten years (the exception is 1890, for which individual-level records did not survive).[9] We start in 1870, as this is the first Census to include the full population, including those formerly enslaved. Incarceration is a relatively rare occurrence (particularly in this earlier period), so the full-count Census allows us to more accurately measure incarceration rates for all immigrants as well as for immigrant subgroups. The 1940 Census is the last Census for which full-count data are currently available in digitized form. Hence, for the 1950–1990 period, we use the largest available sample in each decade (Ruggles et al. 2022).[10] For the most recent years, we use data from the ACS (annual versions or the 2008–2012 and 2015–2019 five-year samples for 2010 and 2020, respectively). We include details on these samples in Online Appendix B.

Measuring Incarceration and Sample Selection. Prior work (e.g., Butcher and Piehl 1998b, 2007) has typically relied on the group quarters type variable coded by IPUMS – indicating whether an individual lives in a "correctional institution" – to classify individuals as incarcerated. For the 1870–1940 full-count data, we improve on this classification using the original strings of the "group quarters," "occupation," and "relationship to household head" variables (e.g., using the

[8] Arrest data typically do not include information on birthplace. Moreover, these data are collected at the local level, making it impossible to build a long-run, nationally representative series. Finally, arrest data include minor offenses, which may be more subject to the bias of law enforcement officials (see Lang and Spitzer 2020); because incarceration typically relies on obtaining a criminal conviction, it is a better proxy for serious criminal offending. We discuss potential issues with using incarceration to proxy for criminality in Section 3.2.

[9] Figure A2 shows an example record of incarcerated individuals in the 1930 Census.

[10] For 1960, 1980, and 1990, we use the 5% samples. For 1950 we use the 1% sample and for 1970 we pool three 1% samples. When considering immigrant subgroups, we do not include the 1950 Census given its smaller size. Given that data availability requires switching from full-count data to sub-samples for 1950–1970 when incarceration was still relatively rare, we focus on trends in the immigrant-US-born incarceration gap in this period, rather than the exact magnitude of the gap. Nevertheless, we validate the incarceration rates against auxiliary sources: between 1950 and 1980, the incarceration rate using the Census falls between 200-300 per 100,000 residents, which is close to measures from the Bureau of Justice Statistics (Kearney et al. 2014). Finally, we do not include the 2000 Census in our main analysis due to potential mismeasurement of outcomes for immigrants (e.g., the difference in incarceration rates between immigrants and US-born men in 2000 is significantly larger than in adjacent data sources). Including the 2000 Census would only reinforce our takeaways (Figure A3).

fact that someone's occupation or relation is listed as "prisoner").[11] This refinement addresses potential misclassification of prisoners; for instance, some individuals whose occupation is listed as "prisoner" are not classified as living in a correctional institution (see Eriksson 2019, 2020 for more discussion).[12] Online Appendix B includes step-by-step instructions on how to implement these refinements. Nevertheless, our takeaways are similar if we use IPUMS' group quarters type variable (Figure A8).

For 1950 onward, we rely on the group quarters type variable to classify individuals as incarcerated. Starting in 1990, IPUMS data report whether individuals are institutionalized, but not the type of institution in which they reside (for instance, we do not know if someone is in a prison or a nursing facility). To address this issue, we focus on men ages 18–40 for whom institutionalization is a very close proxy of incarceration throughout the paper.[13]

To summarize, our baseline sample focuses on men ages 18–40 and compares immigrants (those born outside of the US) to all US-born men. Our main takeaways are nevertheless similar if we compare immigrants to US-born *white* men (Figure A4) or if we focus on other age groups (Figure A9).[14] Table A1 shows the sample sizes and the share incarcerated in each of our samples, by nativity status and disaggregated by immigrants' country-of-origin group.

2. The Immigrant-US-Born Incarceration Gap, 1870–2020

a. Main Results

Figure 1 plots the incarceration rates of immigrants and US-born men from 1870 through 2020. Panel (a) shows that immigrants as a group had similar incarceration rates to the US-born in 1870, slightly lower incarceration rates from 1880 to 1950, and have been *significantly less* likely to be

[11] These string variables are not available for later Censuses, preventing us from implementing these adjustments post-1940.

[12] As described by IPUMS, in the 1870–1930 and the 1960–1970 samples, non-inmates living in institutions are assigned an institutional group quarter type.

[13] Among those institutionalized in 2000 and 2019, 90% of men ages 18–64 and 96% of individuals ages 18–44, respectively, were incarcerated. For 2000, we calculate the number of men aged 18–64 who are in a correctional institution as a share of the institutionalized population (2000 Census Summary File 1 API). For 2019, we calculate the share of individuals aged 18–44 in a correctional institution as a share of the institutionalized population (2019 ACS Table S2603).

[14] We do not restrict the sample to non-Hispanic white men, as Hispanic ethnicity cannot be measured consistently over time.

incarcerated since 1960. Before 1960, the immigrant-US-born incarceration gap was relatively small. The gap then began to widen in 1960, as immigrants' incarceration rate dipped to around 300 per 100,000, whereas the incarceration rate of the US-born jumped to around 900. After 1980, incarceration rates rose dramatically for both groups, but the gap between them remained large so that immigrants are between 50–60% less likely to be incarcerated today. Although the magnitudes of the gaps are smaller, Figure A4 shows that the overall trend in the incarceration gap is similar when we compare immigrants to US-born white men only, in which case immigrants are 15–30% less likely to be incarcerated today.

The remaining panels of Figure 1 compare the incarceration rates of US-born men to those of immigrants from different country-of-origin groups. We split immigrants into five groups with large enough numbers to be followed both historically and today: immigrants from Northern and Western Europe (considered to be the "old immigrant stock" historically), Southern and Eastern Europe (the "new" immigrants historically), China, Mexico and Central America, and the "rest of the world" (those not included in the previous four groups).[15] Figure A5 displays the share of immigrants in each of these groups over time.

Figure 1 shows that the relative decline in immigrants' incarceration rates starting in 1960 has occurred among immigrants from all country-of-origin groups. Immigrants from groups with historically similar incarceration rates (the "old" and "new" Europeans, the Chinese, and those from the "rest of the world") have become significantly less likely to be incarcerated. Immigrants from Mexico and Central America, who featured higher incarceration rates than the US-born before 1960, have fully reversed the gap.[16] Figure A4 shows broadly similar patterns when comparing immigrants to US-born whites. In that case, all immigrant groups, except Mexicans and Central Americans, are less likely to be incarcerated today than US-born white men.

[15] Before 1950, immigrants from the "rest of the world" constituted 10–13% of all immigrants and came primarily from Canada, Japan, and the Caribbean. In the modern period, this group constitutes 40–45% of immigrants and come from the Caribbean, from other countries in South America, Asia, Africa, and the Middle East.

[16] One potential reason for the particularly high incarceration rates of Mexican and Central American immigrants in the past is that, historically, a large proportion of these migrants were seasonal workers. If migrants who were incarcerated could not return home, but those who were not incarcerated did so at high rates, the incarceration rate for this group might be artificially high.

In Figures A6 and A7, we plot differences in immigrant incarceration relative to the US-born separately by country-of-origin for immigrants hailing from the twenty largest sending countries for the past and today. These figures reinforce that immigrants from almost all countries were slightly less likely to be incarcerated historically and that incarceration gaps are wider across the board today.

Online Appendix A shows that the decline in immigrants' relative incarceration is robust to alternative measures of incarceration in the historical period (Figure A8) and alternative sample definitions (Figures A9, A10, and A11).[17] Figure A12 illustrates the importance of using *full-count* data in the historical period: incarceration gaps can be noisy or even the wrong sign for immigrant subgroups when using Census sub-samples.

b. Accounting for Changes in Immigrant Characteristics

A potential explanation for the decline in immigrants' relative incarceration rates is that their observable characteristics (e.g., their age distribution, educational attainment, or racial composition) might have changed in ways that make them less likely to be incarcerated than the US-born.

We begin by documenting changes in the characteristics of incarcerated and non-incarcerated individuals by nativity status and time period (Table A3). Compared to the 1940–1970 period, immigrants have become relatively less educated than the US-born: whereas the proportion of men without a high-school degree has declined by nearly 80% among non-incarcerated US-born men (from 45 to 10%), the same proportion only declined by half among non-incarcerated immigrants (52 to 25%). Given that high school dropouts are over-represented in the incarcerated population, such a change would tend to *increase* immigrants' relative incarceration rates.

We next directly compare the incarceration propensities of immigrants to observationally similar US-born men. Specifically, we use regressions to estimate the immigrant-US-born incarceration

[17] Figure A13 compares our Census-based incarceration measure (a stock) to prison admissions data (a flow) from Missouri for 1870–1920. The two data sources tend to agree on the direction of the immigrant-US-born incarceration gap. Figure A14 further documents that immigrants' lower rate of admission to prison in Missouri is present for both violent and property crimes.

gap and we quantify how this gap changes once we add observable characteristics to the regression. We estimate (separately for each Census year):

$$(1) \quad \text{Incarcerated}_i = \alpha + \beta \text{Immigrant}_i + X_i + \epsilon_i$$

where Incarcerated_i denotes if individual i was incarcerated in that Census year, and Immigrant_i is equal to one for foreign-born individuals. For ease of interpretation, the outcome variable is multiplied by 100 (so β captures percentage-point differences in incarceration rates). X_i reflects a set of individual-level fixed effects for age (one per age), race (white, Black, other), marital status (currently, previously, or never married), state of residence, and education (an indicator denoting literacy before 1940 and three educational categories from 1940 onward: less than high school, high school completion, and any college or more). We report robust standard errors.

Panel (a) in Figure 2 shows that adjusting for age, marital status, state of residence, and race leaves the incarceration gaps mostly unchanged. However, accounting for differences in education significantly widens the gap in recent decades, so that immigrants are even *less* likely to be incarcerated relative to US-born men (a fact noted by Butcher and Piehl 2007 for the 1980–2000 period). Figure A15 shows similar patterns when comparing immigrants only to US-born white men.

Panels (b)-(f) display analogous estimates for the five previously defined immigrant groups.[18] For all groups except for Mexicans and Central Americans, accounting for individual-level characteristics tends to shrink the immigrant-US-born incarceration gap in recent decades (although immigrants remain less likely to be incarcerated). This reduction is driven by accounting for educational differences, as immigrants from groups other than Mexico and Central America are on average more educated than the US-born. By contrast, adjusting for educational differences *amplifies* the incarceration gap between Mexicans and Central Americans (a group with relatively low levels of education) and the US-born. Once we compare this group to US-born men with similar levels of education, they are even *less* likely to be incarcerated in recent decades. Figure

[18] We include race fixed effects to assess the extent to which the changing racial composition of immigrants can account for the relative decline in incarceration. Including race fixed effects becomes redundant when looking at subgroups because there are limited changes in the racial composition of immigrants *within* country-of-origin groups.

A16 shows that the gap is driven by large differences in incarceration among high school dropouts. Of course, immigrants and US-born men who are high school dropouts may be quite different in terms of unobservable traits; however, insofar as criminal behavior is a function of labor market opportunities (Becker 1968), then this figure indicates that Mexican and Central American immigrants are significantly less likely to be incarcerated than US-born men with comparable labor market prospects.[19]

Figure A18 shows that the widening in the immigrant-US-born gap is also not driven by changes in immigrants' country-of-origin mix. This figure, which allows each immigrant group's incarceration rate to evolve over time but holds constant their 1940 share of the immigrant population, shows that the gap would be even *larger* today had the country-of-origin mix not shifted away from Europe toward a more diverse set of sending countries. Finally, the decline is also not driven by increases in the share of immigrants that are recent arrivals who may not have had sufficient time to be incarcerated. Appendix Figure A19 restricts the immigrant sample based on time since arrival to the US; the gap is present even among immigrants who have been in the country for at least five or ten years.

We conclude that changes in migrants' observable characteristics cannot explain the decline in immigrants' relative incarceration rates. If anything, once we account for these characteristics, the difference between immigrants and the US-born becomes even larger.

3. Possible Explanations for the Relative Decline in Immigrants' Incarceration

a. Changes in Immigration Policies: Deportations and Detentions

The number of immigrant deportations from the US began rising in the 1990s and reached record-high numbers around 2010 (Figure A20). Increased deportations may have affected immigrants' incarceration rates in two ways. First, surges in deportations increase the expected cost of committing a crime for non-citizens (and thus might lower their rates of criminal activity): these migrants can expect to serve a period of incarceration in the US *and* then may face deportation after serving their sentence (the so-called "double penalty"). Second, if immigrants who commit

[19] Figure A17 plots the income gap by educational group, showing that low-educated immigrants tend to have similar or lower incomes than low-educated US-born men.

crimes are deported without serving their sentence, then we might find that immigrants are less likely to be incarcerated – because immigrant offenders are removed from the data via deportation – even if they committed as many or more crimes than the US-born. We rule out these two possibilities in turn.

First, if the relative decline in immigrants' incarceration rates was solely driven by an increased risk of deportation, we would not expect to see the decline for immigrants who hold US citizenship and thus cannot be deported. However, Figure 3 shows that if anything, the relative decline is *more pronounced* when we focus on immigrants who are US citizens.

Second, the relative decline in immigrants' incarceration rates is unlikely to be mechanically driven by deportations. First, immigrants who have been convicted of a crime are typically deported *after* serving their sentence and immigrants may not have access to benefits that can shorten incarceration spells for citizens (e.g., participating in diversion programs; Watson and Thompson 2022).[20] Furthermore, the relative decline in immigrants' incarceration rates emerged by 1960, before the rise in mass deportations in the 2000s. Finally, more than 90% of individuals who are deported today are Mexican and Central American (Watson and Thompson 2022). Yet, the immigrant-US-born incarceration gap has widened for immigrants from all regions.

In addition to the recent rise in deportations, there has also been a rise in immigrant detentions for immigration-related violations. This surge in detentions, however, would bias us *against* finding a decline in immigrants' incarceration: if immigrants are held in detention facilities for immigration violations (e.g., overstaying their visa), they would likely be counted as "incarcerated" by our metric and hence inflate immigrants' (and especially Mexican and Central American immigrants') incarceration rates.

[20] Immigration law states that "the Attorney General may not remove an alien who is sentenced to imprisonment until the alien is released from imprisonment" (8 U.S.C. sec. 1231[a][4][A]). However, this law may not be strictly enforced, and some non-citizen immigrant offenders might be deported before the end of their sentence. To assess this possibility, we use Department of Homeland Security data on the number of deported individuals who had a previous criminal conviction (i.e., individuals who could have plausibly remained incarcerated had they not been deported). These data are not restricted to men ages 18–40, so we are likely overestimating the number of deportations in our target population. Yet, even under the extreme assumption that half of these individuals would have remained in prison rather than being deported, immigrants' incarceration rates would *still* be lower than those of US-born men.

Indeed, Figure A21 shows that if we exclude from the sample individuals residing in areas containing large ICE facilities, then the incarceration gap between Mexican and Central American immigrants and US-born men becomes even larger. Excluding areas with *any* ICE facilities (~100 out of 1,000+ total areas) eliminates Mexicans and Central Americans' higher incarceration rates relative to US-born *white* men in thirteen out of the fourteen most recent years.[21] By contrast, excluding these areas does not change the gap in 1970–1990, *prior* to the large increase in immigrant detention and deportation. These patterns suggest that immigrant detentions are overstating the degree to which immigrants, especially those from Mexico and Central America, engage in serious criminal behavior.

b. Changing Relationship Between Incarceration and Criminality

Another potential explanation for the patterns that we document is that, for any given level of underlying criminal activity, the probability of incarceration may differ for immigrants relative to the US-born. In particular, we might observe a widening incarceration gap if immigrants are (increasingly) *less* likely than the US-born to be incarcerated for a given offense. We argue that such an explanation is unlikely to account for our findings.

First, incarceration rates would understate immigrants' true levels of criminality if aspects of the criminal justice system are biased *in favor* of immigrants. This possibility is unlikely to be true, as prior work shows that noncitizens tend to receive longer prison sentences than citizens for comparable crimes (Light et al. 2023), and that the modern criminal justice system is biased against Hispanics (Goncalves and Mello 2021, Tuttle 2023). Thus, unless the criminal justice system has become substantially *less* biased toward immigrants, and now favors immigrants over the US-born (including US-born white men, since we also see a decline when they are the main reference point), it is unlikely that such biases can explain the relative decline in immigrants' incarceration.

Second, incarceration rates might understate immigrants' criminality if unauthorized immigrants are less likely to report crimes due to fear of deportation (Comino et al. 2020, Jácome 2022). Yet, we see the relative decline among immigrants from all sending regions (with significantly different

[21] See Online Appendix B for details on excluded facilities.

shares of unauthorized populations), among citizen migrants (who cannot be deported), and decades prior to the rise in deportations.

4. The Widening of the Incarceration Gap is Part of a Trend of Growing Differences between Immigrants and the US-born

Numerous studies have shown that less-educated men – the group that accounts for most of the recent increase in incarceration; panel (a) of Figure 4 – have experienced a deterioration in outcomes, including their employment, family formation, incarceration, and health (Abraham and Kearney 2018, Binder and Bound 2019, Coile and Duggan 2019, Case and Deaton 2020). This deterioration has been attributed to declines in labor demand from globalization (e.g., Autor et al. 2013) and skill-biased technological change (e.g., Acemoglu and Autor 2011), among other forces.

We conclude the paper by showing that, beyond incarceration, this broader deterioration in outcomes has been significantly more muted among less-educated *immigrant* men. The remaining panels of Figure 4 confirm that low-educated immigrants and US-born men, particularly high school dropouts, have diverged along several dimensions since the 1960s. Panels (b) and (c) show that there has been a divergence in the degree of labor force attachment: among men without a high school degree, immigrants were employed at similar rates than their US-born counterparts in the past but are 30 percentage points more likely to be employed today. Figure A22 shows this same divergence when comparing immigrants to white US-born men only.[22]

Panels (d) and (e) show that low-educated immigrants and US-born men have also diverged with respect to family formation rates. Again, we find that low-educated immigrants and US-born men were comparable prior to 1960 and then began to diverge, with low-educated immigrants now being significantly more likely to be married and living with children. This divergence has been mostly driven by the US-born having a lower probability of marriage and living with children, rather than by increases among immigrants, suggesting that the pattern is not driven by family reunification rules in the immigration system.

[22] The figures in this section start in 1940 because this is the first Census that records education. Figures A23 and A24 show analogous figures for all men and for low-educated women.

Finally, panel (f) uses data from the General Social Survey to show that there has been a divergence in self-reported health status. By 1980, the proportion of US-born men without a high school degree who reported having "excellent" or "good" health (as opposed to "fair" or "poor") was about 63%, 8 percentage points below the corresponding proportion among immigrants without a high school degree. Today, the gap is much larger (closer to 20 percentage points).

Of course, the outcomes in this subsection are correlated with each other and with criminality and incarceration, so the direction of causality is not obvious. On the one hand, worse employment prospects (Gould et al. 2002, Britto et al. 2022), lower marriage rates (Dustmann and Ladersø 2021, Massenkoff and Rose 2022), and lower parenthood rates (Sampson et al. 2006) may all contribute to higher incarceration. On the other hand, higher incarceration rates among low-educated men may have negatively impacted their labor market outcomes (Agan and Starr 2018, Dobbie et al. 2018) and their family formation (Charles and Luoh 2010). Regardless of the direction of causality, these patterns highlight that incarceration is part of a broader divergence of outcomes between less-educated immigrants and their US-born counterparts.

Why have less-educated immigrants remained relatively insulated from the forces that negatively affected low-educated US-born men? Our data do not allow us to pinpoint precise reasons, but we offer two possible explanations. First, lower-educated immigrants have specialized in manual, non-routine occupations, which are often located at the bottom of the wage distribution (Peri and Sparber 2009). Hence, immigrants were relatively shielded from the "hollowing out" of the middle of the wage distribution (Autor et al. 2006, 2008).[23] Second, immigrants are a self-selected group of individuals that likely differs from their US-born counterparts in characteristics such as their risk aversion (Jaeger et al. 2010) or their adaptability and cognitive ability (Bütikofer and Peri 2021). Immigrants have revealed that they are willing to travel long distances for opportunity, a trait which is consistent with immigrants' higher rates of entrepreneurship across the firm-size

[23] In contrast, Figure A25 shows that immigrants were equally likely to be concentrated in the declining manufacturing sector.

distribution (Azoulay et al. 2022).[24,25] Such characteristics may have helped immigrants to weather the negative shocks that affected less-educated US-born men.[26]

5. Conclusion

We construct the first nationally representative series of immigrant-US-born incarceration gaps from 1870 until present day. We find that, as a group, immigrant men have had a *lower* incarceration rate than US-born men for the last 150 years of American history. The differences in incarceration have become more pronounced starting in 1960, with recent waves of immigrants being 50–60% less likely to be incarcerated than US-born men (30% when compared to US-born white men). This relative decline in incarceration has occurred among immigrants from all major countries of origin, and it cannot be explained by changes in immigrants' observable characteristics or in immigration policies. Moreover, we show that the divergence in outcomes between less-educated immigrants and US-born men occurred along dimensions beyond incarceration, including labor force participation and family formation rates.

Although this paper rules out several potential explanations for the decline in immigrants' relative incarceration rates that took place since the 1960s, future work might delve deeper into why immigrants' outcomes differ so significantly from those of their US-born counterparts. The fact that less-educated immigrants and the US-born have diverged along multiple dimensions – ranging from labor market outcomes, to incarceration, to health – suggests that the relative decline in immigrants' incarceration might reflect deeper structural forces disproportionately affecting low-educated US-born men (and not their immigrant counterparts) in the past half century.

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[24] Prior work (Amior 2020, Basso and Peri 2020, Cadena and Kovak 2016) shows that immigrants have greater migration responsiveness to economic conditions. Nevertheless, we note that differences in location cannot explain the gaps between lower-educated immigrants and their US-born counterparts. Figures A26 and A27 show that labor market and family formation gaps are stable after accounting for granular geographic (county or PUMA) fixed effects.

[25] Additional figures consider and rule out other reasons for immigrants being relatively less affected by these forces. Figure A28 shows that low-educated *citizen* immigrants also have higher employment and labor force participation rates than US-born men, making it unlikely that differences in the availability of social insurance can explain the widening of the gap. We also do not find any evidence that differences in the likelihood of committing drug-related offenses can explain the immigrant-US-born incarceration gap (Figure A29).

[26] A potential explanation for these patterns is that the group of less-educated US-born men might have become smaller in size and increasingly negatively selected over time, but that such a process did not occur to the same degree among immigrants (Novosad et al 2022). Note, however, that this explanation cannot account for the fact that the incarceration rate of immigrants *as a whole* declined relative to that of the US-born.

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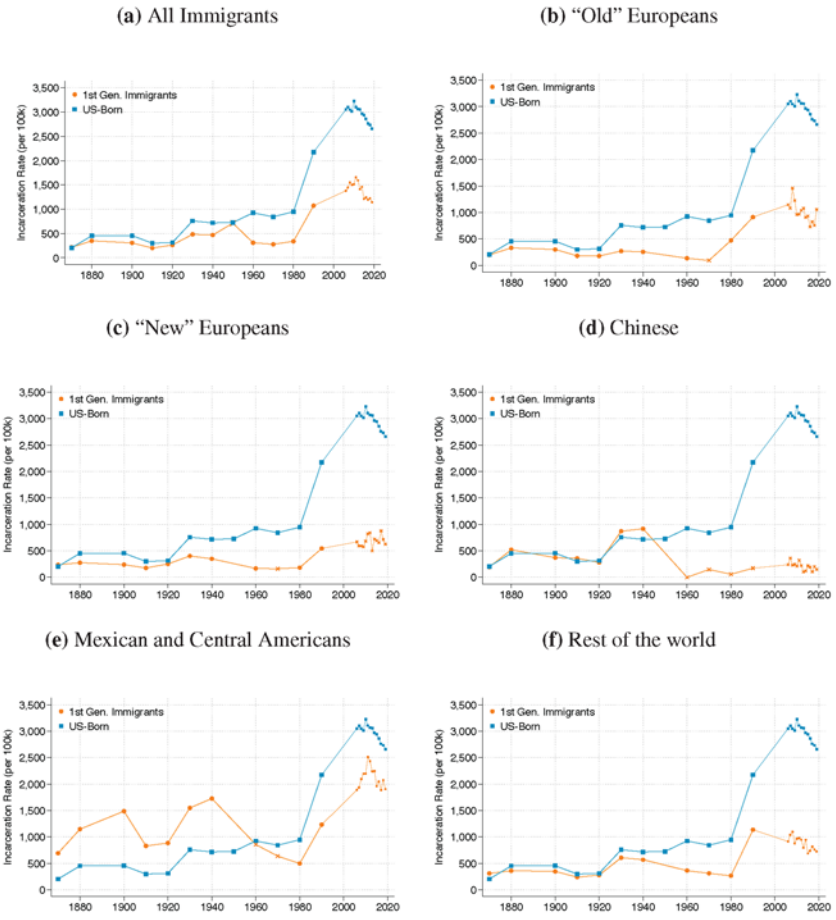
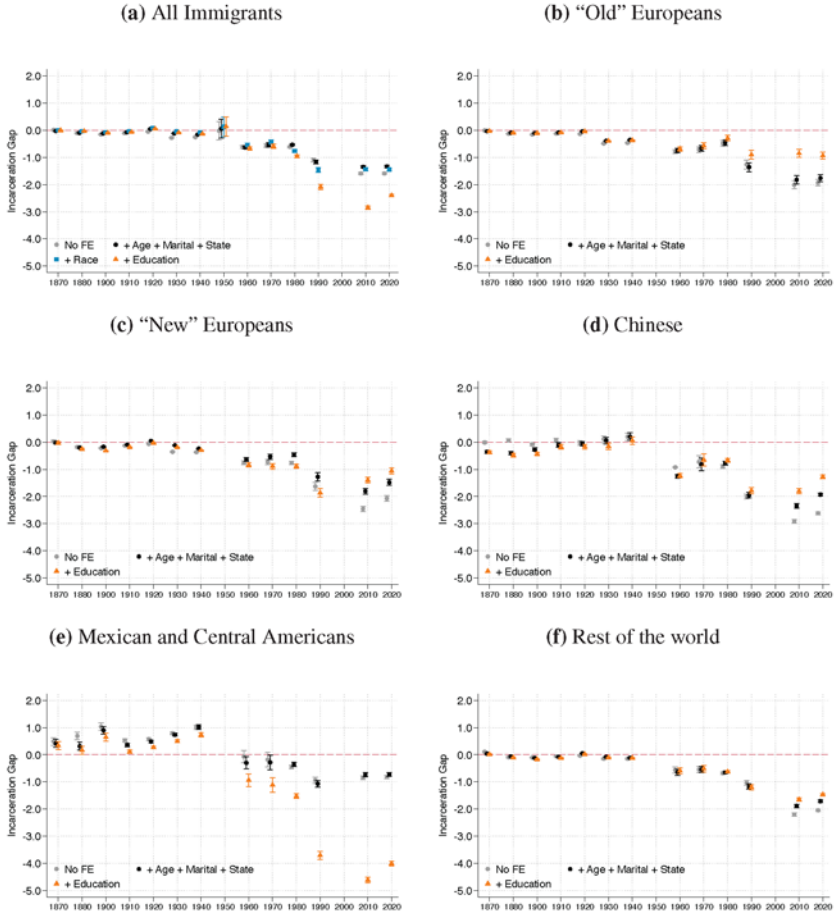
Figure 1: Incarceration Rates of Immigrants and US-born Men, 1870-2019



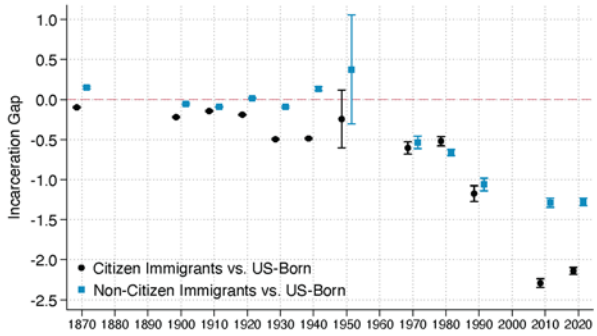
Notes: Each panel plots incarceration rates for immigrants and US-born men between 1870 and 2019. Data are restricted to males ages 18-40. Data spanning 1870 to 1940 are from the full-count decennial Censuses. Data spanning 1950 to 1990 are from the largest available sub-samples from the corresponding decennial Censuses. Data from 2005 onward are from the annual American Community Surveys (ACS). Cross markers indicate that fewer than 10,000 immigrants were used to calculate the corresponding incarceration rate. Panel (a) compares US-born men to all immigrants. Panels (b)-(f) compare US-born men to immigrants from a particular country-of-origin group. "Old Europeans" are immigrants from countries in the North and West of Europe. "New Europeans" are immigrants from countries in Eastern and Southern Europe. The "Rest of the world" category includes immigrants from countries not included in panels (b)-(f). For more details, see Online Appendix B.

Figure 2: Difference in Incarceration Rates of Immigrants and US-born Men, Adjusting for Individual-Level Characteristics, 1870-2019



Notes: Each panel plots the estimated values of β using equation (1) for each Census year. The first series includes no individual-level control variables, and each subsequent series sequentially adds control variables. The second series includes individual age fixed effects, marital status (currently, previously, and never married) fixed effects, and state-of-residence fixed effects. In panel (a), the third series includes race fixed effects. The final series in each panel adds education fixed effects. Education refers to an indicator denoting literacy before 1940 and educational attainment from 1940 onward (HS dropout, HS graduate, any college). Panel (a) compares US-born men to all immigrants. Panels (b)-(f) compare US-born men to immigrants from a particular country-of-origin group. See Figure 1 and Online Appendix B for data sources and definitions of each country-of-origin group. All estimates report robust standard errors.

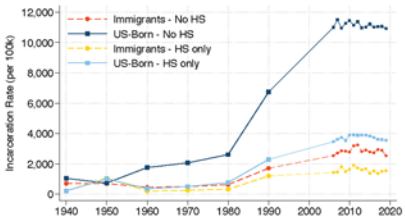
Figure 3: Differences in Incarceration Rates of Citizen and Non-Citizen Immigrants, 1870-2019



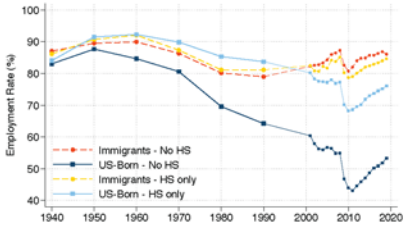
Notes: Each series plots the estimated values of β using equation (1) and varying the sample of immigrants. The first series restricts the sample of immigrants to those that are US citizens. The second series restricts the sample of immigrants to those that are non-citizens. Data are restricted to males ages 18-40. In 1870, 1900, and 1910, data are restricted to males ages 21-40 because citizenship was not defined for individuals under 21 in these Censuses. Data from 1880 and 1960 are omitted because the Census did not include a citizenship question in those years. See Figure 1 and Online Appendix B for more details on data sources. All estimates report robust standard errors.

Figure 4: Incarceration, Labor Market, Family Formation, and Health Outcomes of Immigrants and US-born Men Without Any College Education, 1940-2019

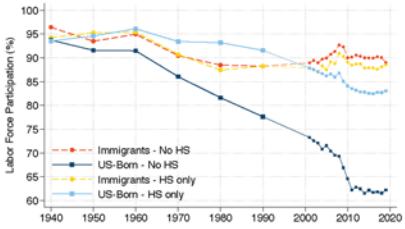
(a) Incarceration Rate (per 100,000)



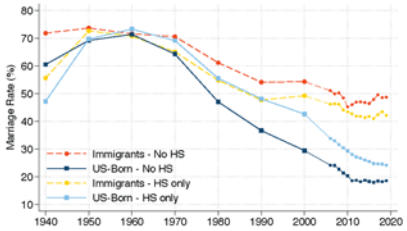
(b) Employment Rate (%)



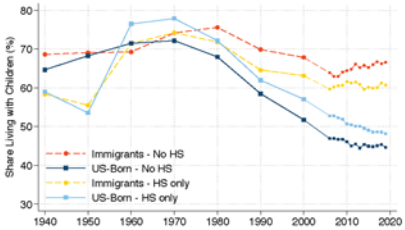
(c) Labor Force Participation Rate (%)



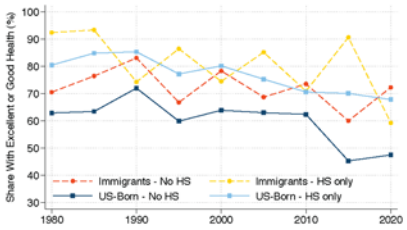
(d) Marriage Rate (%)



(e) Share Living with Children (%)



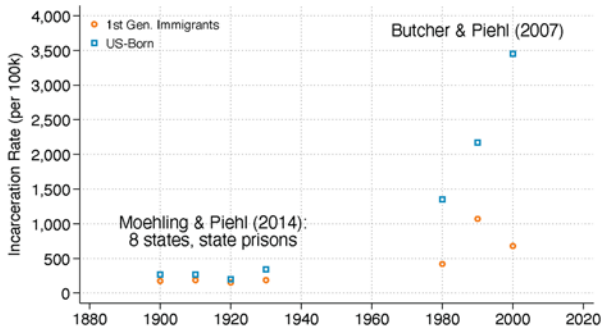
(f) Share with Excellent or Good Health (%)



Notes: This figure plots the outcomes of immigrant and US-born men by educational attainment between 1940 and 2019. "No HS" refers to individuals with 11 or fewer years of schooling. "HS Only" refers to individuals with exactly 12 years of schooling. Panels (a)-(d) are restricted to males ages 18-40. Panels (e) and (f) are restricted to males ages 30-50 and 18-65, respectively. Panels (b)-(e) restrict the sample to non-institutionalized individuals. For panels (a)-(e), data spanning 1950 to 1990 are from the largest available sub-sample from the decennial Census, and data from 2000 onward are from the annual American Community Survey (ACS). Panel (f) uses data from the 1977–2020 General Social Survey (GSS) and plots the share of individuals who report being in excellent or good health. Each data point in this final panel reflects information from various survey waves around that year. For more details, see Online Appendix B.

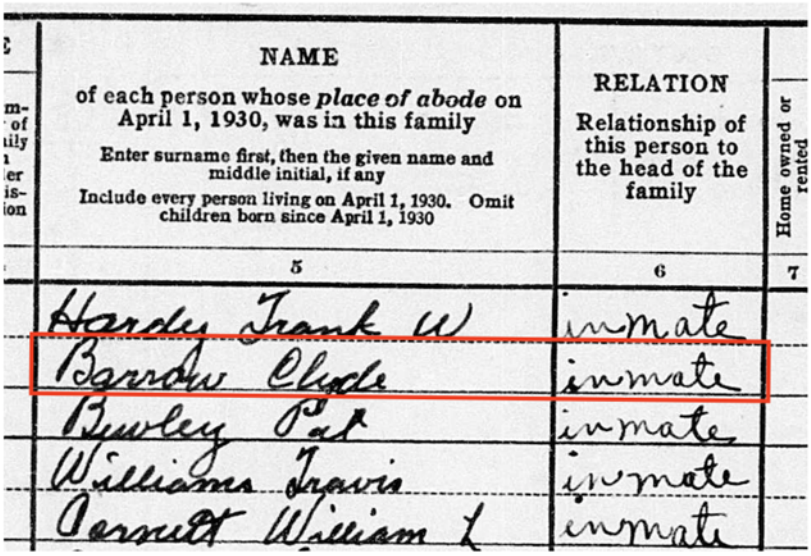
Online Appendix A: Appendix Figures and Tables

Figure A1: Existing Evidence on Immigrant and US-born Incarceration Rates



Notes: This figure plots historical incarceration rates of immigrants and US-born individuals from Moehling and Piehl (2014) as well as modern incarceration rates from Butcher and Piehl (2007). The historical incarceration rates are based on US-born and immigrant individuals ages 18-44 who were incarcerated in state correctional facilities in eight states: Massachusetts, Connecticut, New York, New Jersey, Pennsylvania, Illinois, Michigan, and California. The modern incarceration rates correspond to institutionalization rates among all US-born and immigrant men ages 18-40 from sub-samples of the decennial Censuses.

Figure A2: Example Record of Incarcerated Individuals in 1930 Census

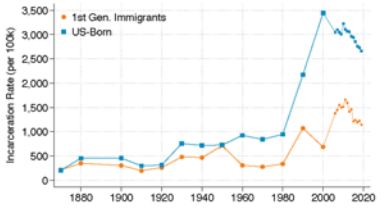


	NAME of each person whose *place of abode* on April 1, 1930, was in this family. Enter surname first, then the given name and middle initial, if any. Include every person living on April 1, 1930. Omit children born since April 1, 1930	RELATION Relationship of this person to the head of the family	Home owned or rented
	5	6	7
	Harder Frank W	inmate	
	Barrow Clyde	inmate	
	Bowley Pat	inmate	
	Williams Travis	inmate	
	Barnett William L	inmate	

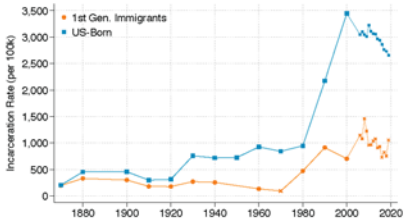
Notes: This figure shows an example record of incarcerated individuals in the 1930 population Census.

Figure A3: Incarceration Rates of Immigrants and US-born Men for 1870-2019, Including 2000

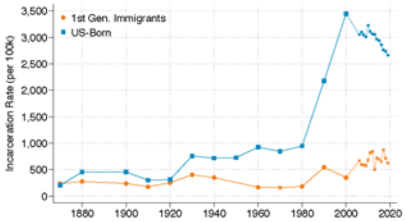
(a) All Immigrants



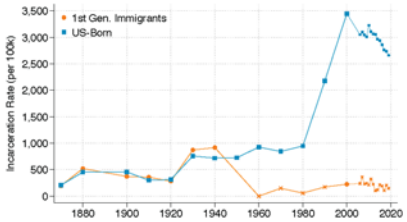
(b) "Old" Europeans



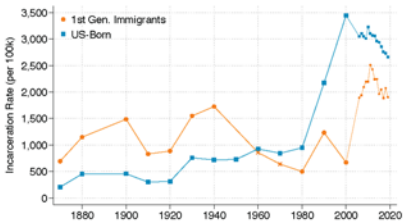
(c) "New" Europeans



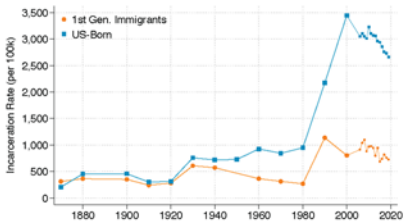
(d) Chinese



(e) Mexican and Central Americans



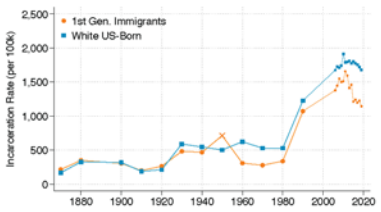
(f) Rest of the world



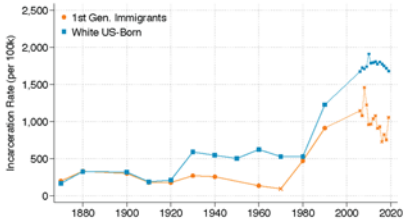
Notes: Each of the panels in this figure plots incarceration rates for immigrants and US-born between 1870 and 2019 as in Figure 1, but including the corresponding points for the 2000 Census. For more details, see the note to Figure 1 and Online Appendix B.

Figure A4: Incarceration Rates of Immigrants and White US-born Men, 1870-2019

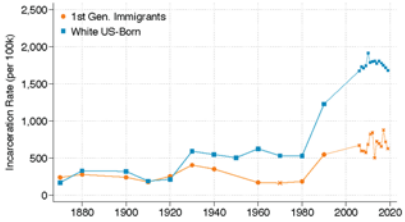
(a) All Immigrants



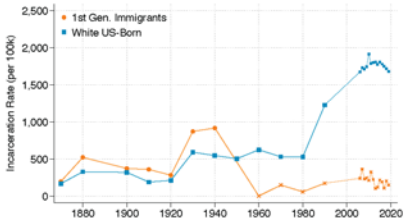
(b) "Old" Europeans



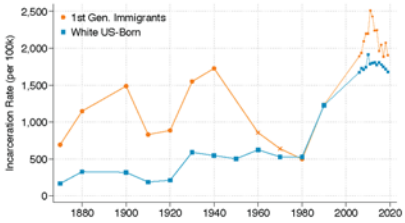
(c) "New" Europeans



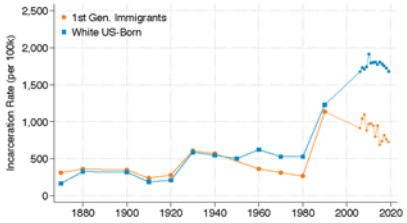
(d) Chinese



(e) Mexican and Central Americans



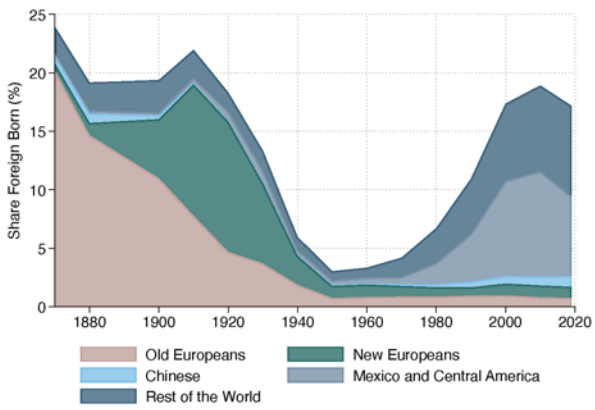
(f) Rest of the world



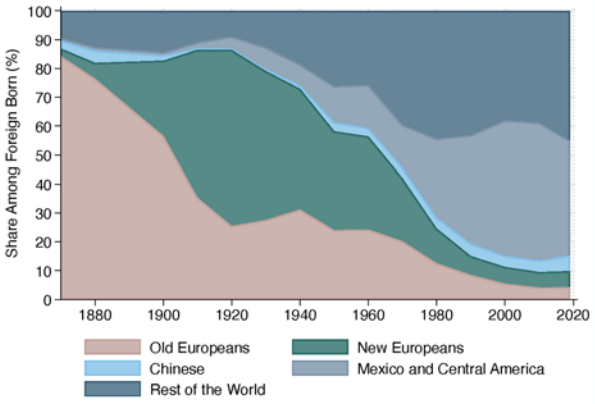
Notes: Each of the panels in this figure plots incarceration rates for immigrants (regardless of their race) and white US-born men between 1870 and 2019. For more details, see the note to Figure 1 and Online Appendix B.

Figure A5: Immigrant Composition in the US, 1870-2019

(a) Within the US Population

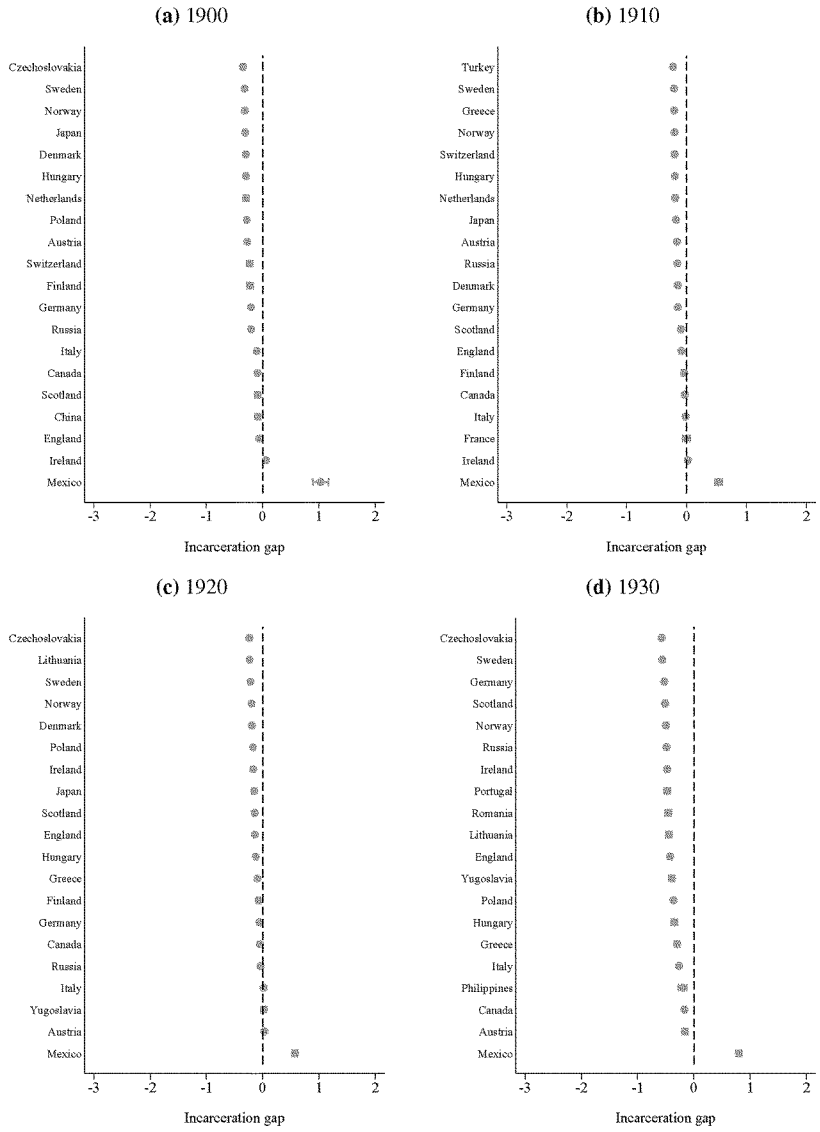


(b) Within the Immigrant Population



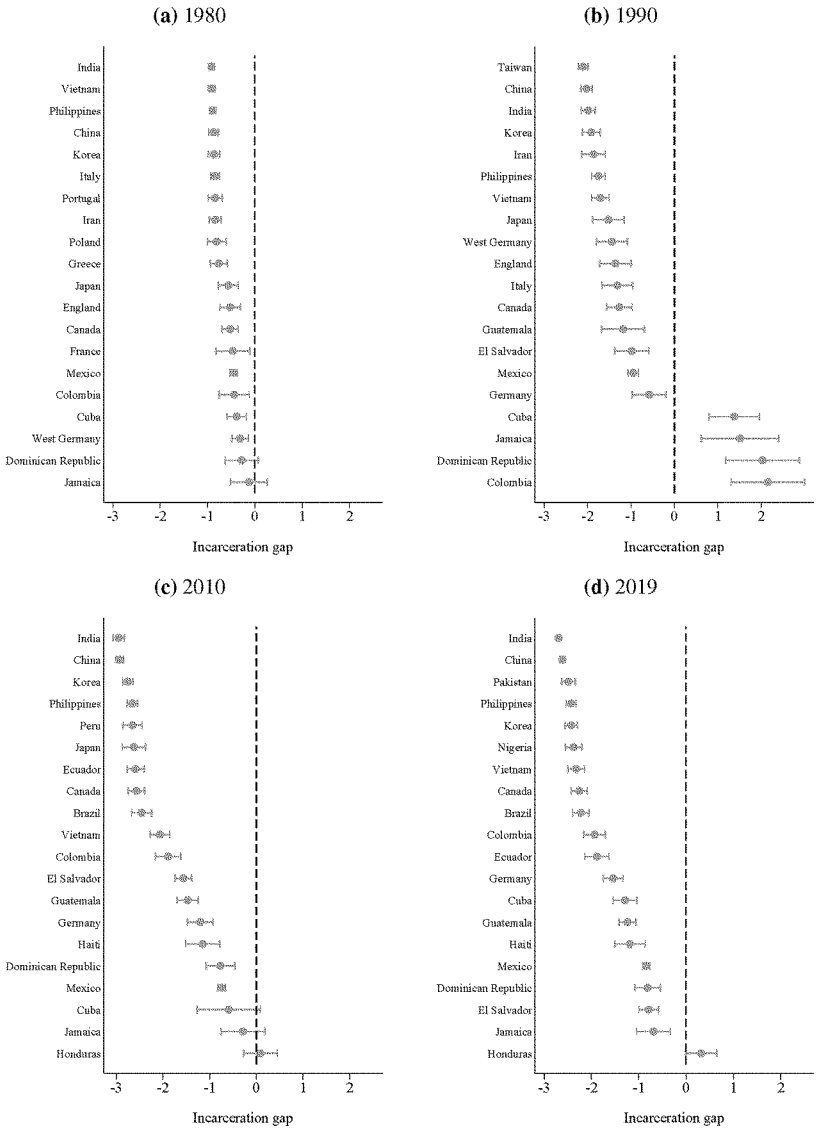
Notes: Panel (a) illustrates the share of men ages 18-40 that are foreign-born between 1870 and 2019. Panel (b) shows the composition of each immigrant group among foreign-born individuals. Each color depicts immigrants from a specific country-of-origin group, showing that immigrants today are more likely to come from Mexico and Central America as well as from the "rest of the world" group. For more details on the definition of each country-of-origin group, see Online Appendix B.

Figure A6: Incarceration Gap of Immigrants and US-born Men by Country of Origin, 1900-1930



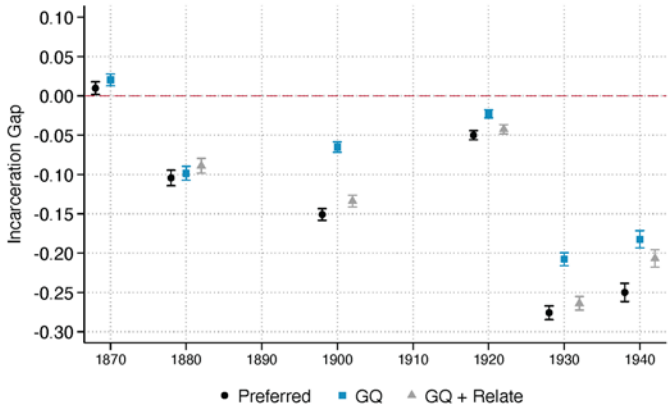
Notes: This figure plots the difference in incarceration propensities between US-born men and immigrants from each of the 20 sending countries with the largest populations in the US that year (each estimate is the value of β using equation (1) without any individual-level characteristics). All estimates report robust standard errors.

Figure A7: Incarceration Gap of Immigrants and US-born Men by Country of Origin, 1980-2019



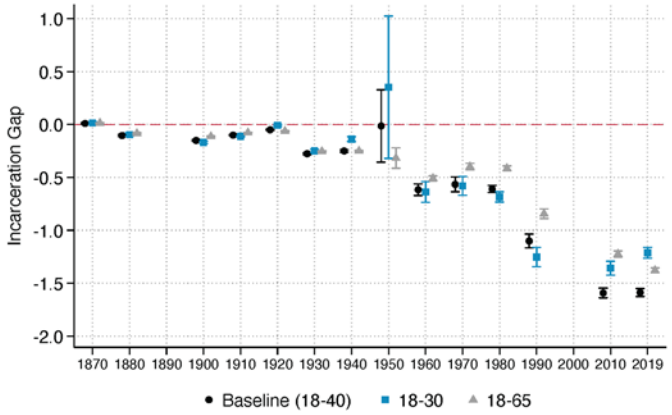
Notes: This figure plots the difference in incarceration propensities between US-born men and immigrants from each of the 20 sending countries with the largest populations in the US that year (each estimate is the value of β using equation (1) without any individual-level characteristics). All estimates report robust standard errors.

Figure A8: Incarceration Gap between Immigrants and US-born Men Using Alternative Incarceration Measures, 1870-1940



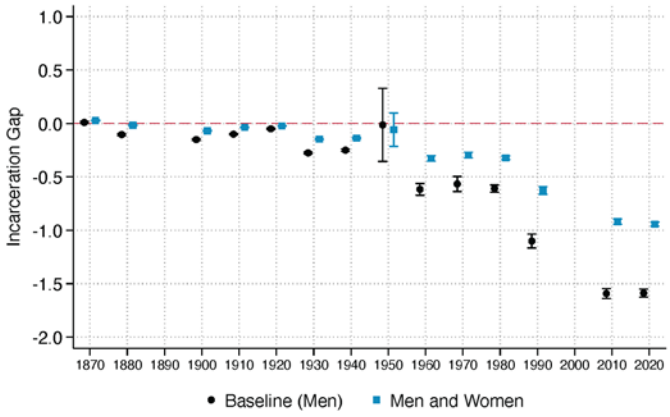
Notes: This figure plots the estimated values of β using equation (1) and varying the definition of incarceration in the 1870-1940 full-count decennial Censuses. The first series utilizes the baseline measure of incarceration. The second series uses the IPUMS group quarters variable only to classify an individual as incarcerated. The third series uses the group quarters variable and the variable denoting an individual's relationship to the household head to classify an individual as incarcerated. The 1910 Census does not identify group quarter types, so we omit this year in the comparison. The 1870 Census does not include a question on relationship to household head. For more details on these measures, see Online Appendix B. All estimates report robust standard errors.

Figure A9: Incarceration Gap between Immigrants and US-born Men, Varying the Age of the Sample



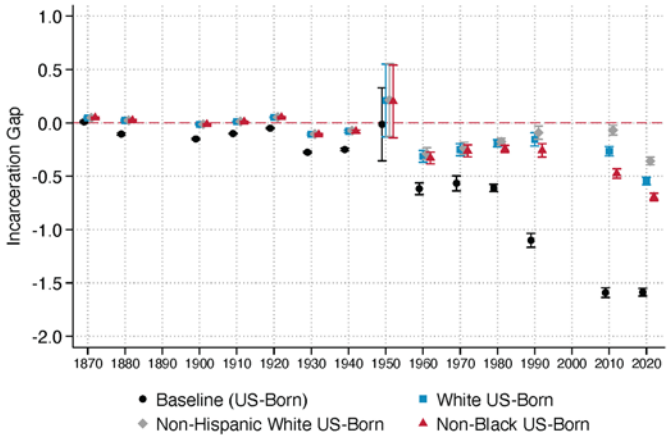
Notes: This figure plots the estimated values of β using equation (1) and varying the age of the individuals in the sample. The first series reproduces the baseline estimates using men ages 18-40. The second and third series consider men ages 18-30 and 18-65, respectively. All estimates report robust standard errors.

Figure A10: Incarceration Gap between Immigrants and US-born Individuals, Including Women



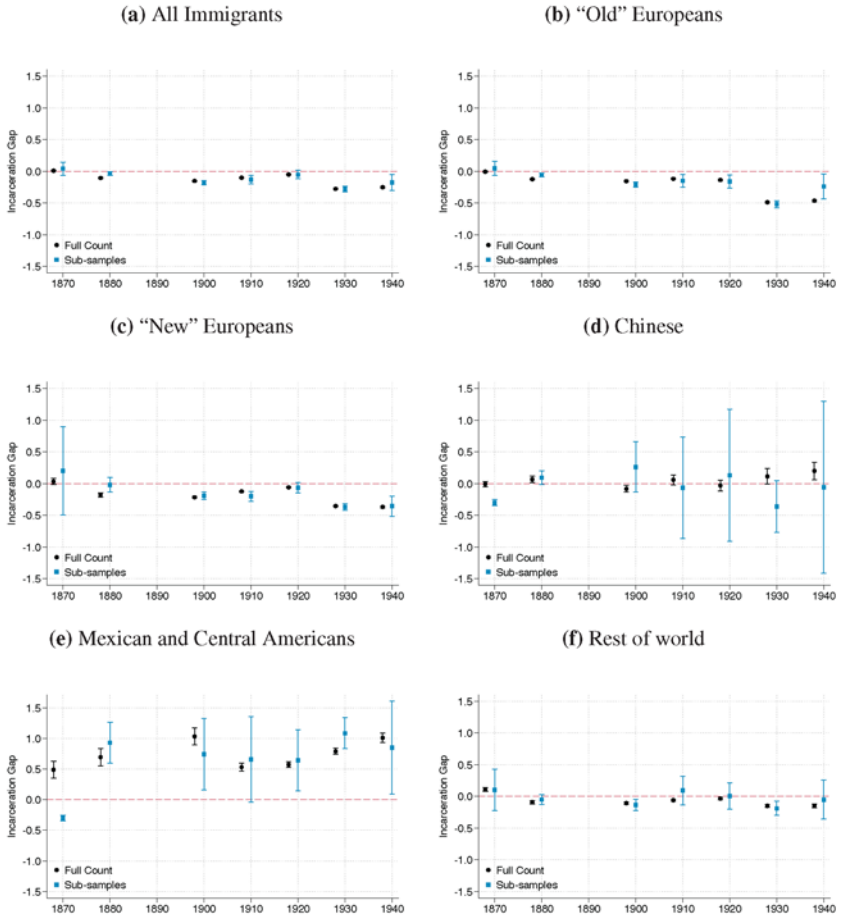
Notes: This figure plots the estimated values of β using equation (1) for individuals ages 18-40. The first series reproduces the baseline estimates restricting the sample to men. The second series expands the sample to include women. All estimates report robust standard errors.

Figure A11: Incarceration Gap between Immigrants and US-born Men, Using Alternative Groups of US-born Individuals



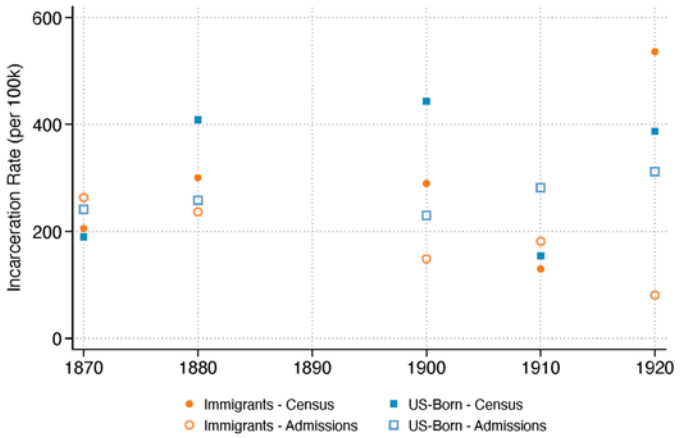
Notes: This figure plots the estimated values of β using equation (1) and varying the sample of US-born men. The first series reproduces the baseline estimate considering all US-born men. The second series only considers white US-born men. The third series considers non-Hispanic white US-born men. Hispanic individuals are identified using the "Hispan" variable provided by IPUMS. Before 1980, individuals were classified as Hispanic based on their country of birth, parental country of birth, Spanish surname, or relationship to someone identified as Hispanic through these characteristics. The fourth series considers US-born men whose race is not classified as Black. All estimates report robust standard errors.

Figure A12: Incarceration Gap between Immigrants and US-born Men, Comparing Full Count Census with Sub-samples, 1870-1940



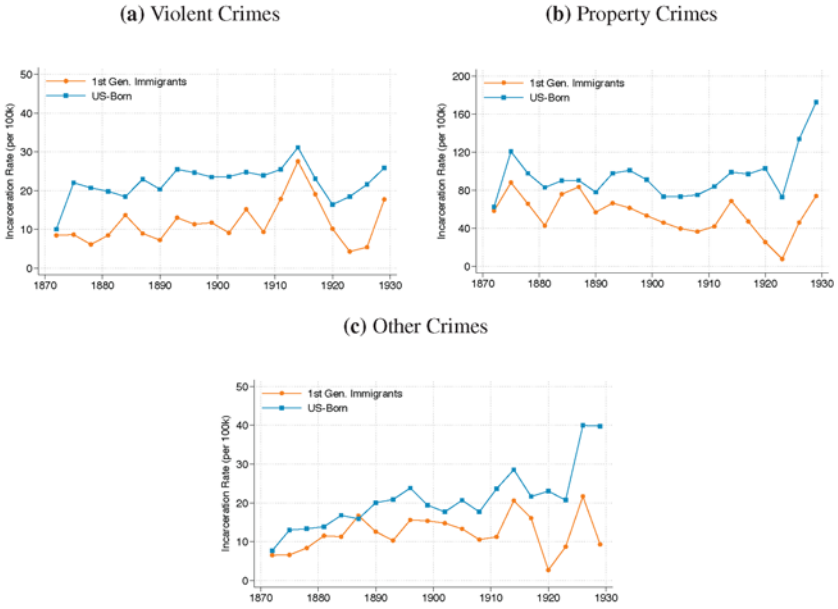
Notes: This figure plots the estimated values of β using equation (1) for 1870-1940. The first series reproduces the baseline estimates using the full-count Censuses. The second series utilizes the largest available sub-sample from each decennial Census. Panel (a) compares US-born men to all immigrants. Panels (b)-(f) compare US-born men to immigrants from a particular country-of-origin group. For more details, see the note to Figure 1 and Online Appendix B. All estimates report robust standard errors.

Figure A13: Comparison of Census-based Incarceration Rates in Missouri to Prison Admissions Rates from the Missouri State Penitentiary



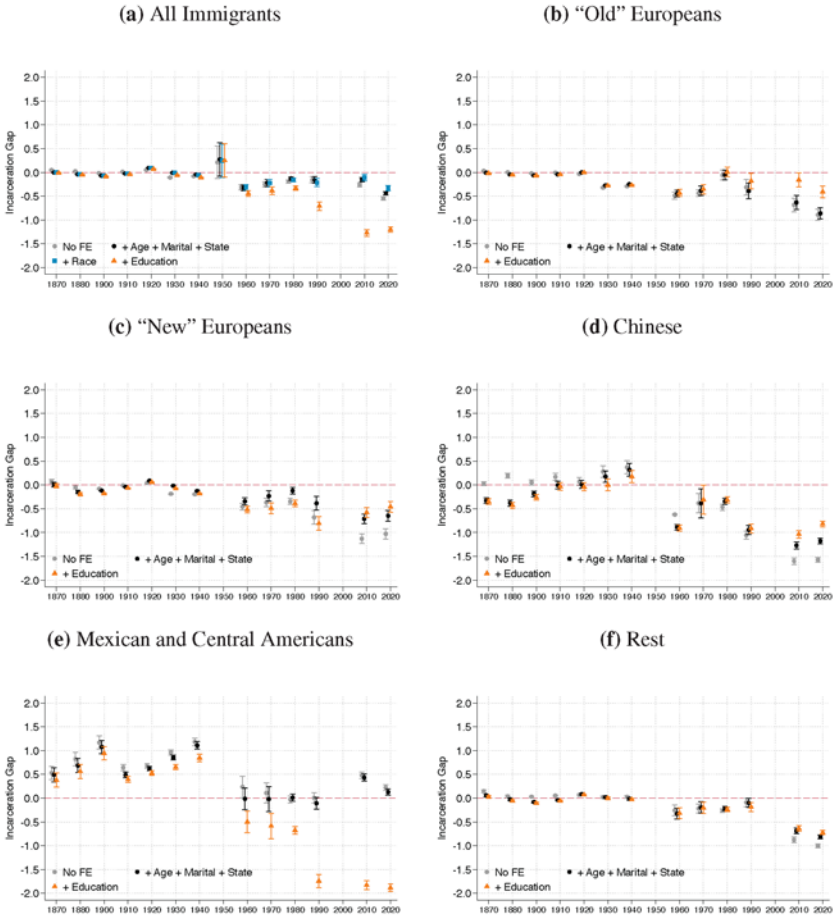
Notes: This figure compares the incarceration rates of immigrants and US-born men residing in Missouri (based on Census data) with prison admissions rates by nativity based on prison admission records from the Missouri State Penitentiary. The data on prison admissions come from digitized administrative records of the Missouri State Penitentiary, which covers the universe of prison inmates in Missouri. Population counts, used to calculate rates, come from the full-count Census.

Figure A14: Prison Admissions Rates of Immigrants and US-born Individuals in Missouri by Type of Crime, 1872–1929



Notes: This figure plots prison admissions rates of immigrants and US-born individuals between 1872 and 1929 separately by crime type. Data are based on prison admission records from digitized administrative records of the Missouri State Penitentiary, which covers the universe of prison inmates in Missouri. Panels (a), (b), and (c) consider admissions for violent, property, and other crimes, respectively. Population counts, used to calculate rates, come from the full-count Census and are interpolated between Census years.

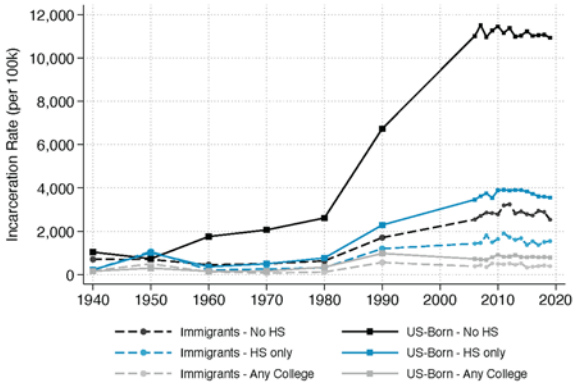
Figure A15: Difference in Incarceration Rates of Immigrants and White US-born Men, Adjusting for Individual-Level Characteristics, 1870-2019



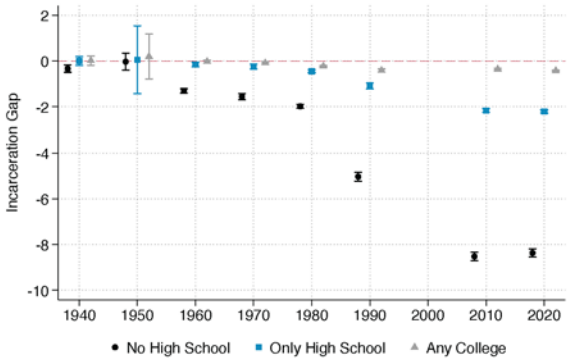
Notes: This figure is analogous to Figure 2 but restricts the sample of US-born men to white US-born men. For more details, see the note to Figure 2 and Online Appendix B. All estimates report robust standard errors.

Figure A16: Incarceration Gap Between Immigrants and US-born Men, by Educational Attainment, 1940-2019

(a) Incarceration Rates

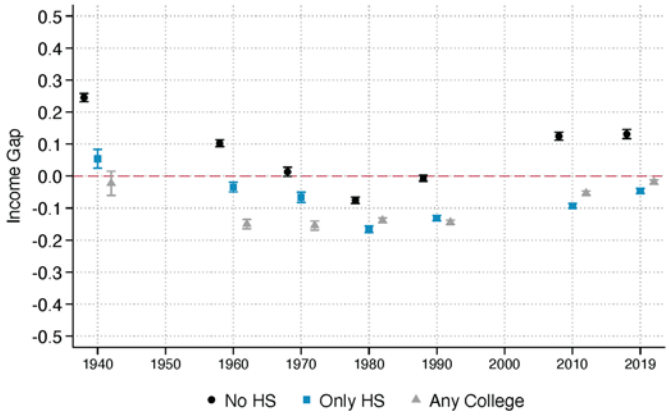


(b) Differences in Incarceration



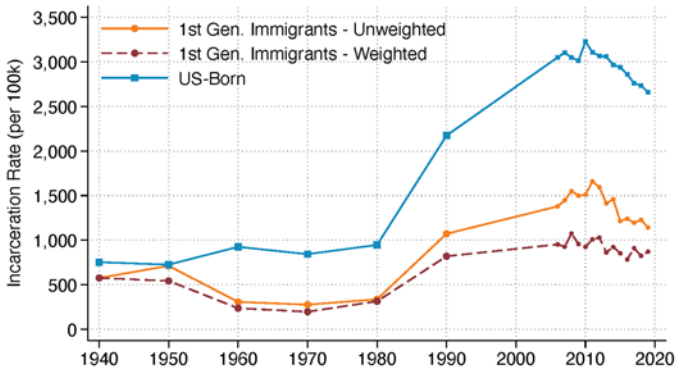
Notes: Panel (a) plots incarceration rates for immigrants and US-born men between 1940 and 2019 separately by educational attainment. Panel (b) plots the estimated values of β using equation (1) separately by individuals' educational attainment. "No High School" refers to individuals with 11 or fewer years of schooling. "High School" refers to individuals with exactly 12 years of schooling. "Any College" refers to individuals with one or more years of college. In panel (b), all estimates report robust standard errors.

Figure A17: Differences in Logged Income Between Immigrants and US-born Men, by Educational Attainment, 1940-2019



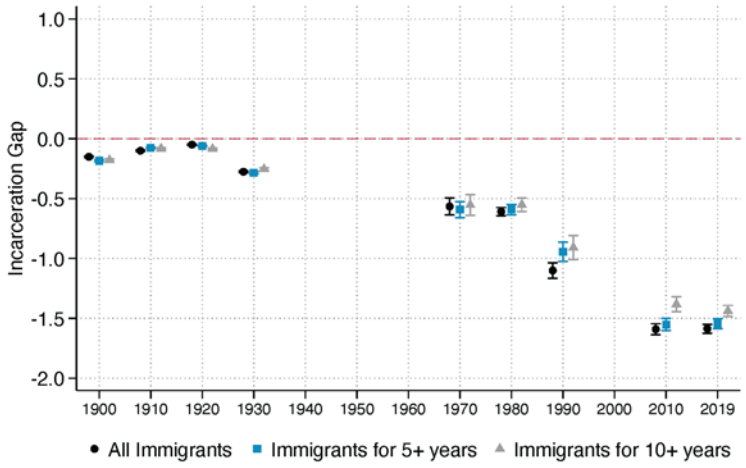
Notes: The figure plots the estimated values of β from equation (1) using logged income as the outcome variable and separately by individuals' educational attainment. The sample is restricted to men ages 18-40 who are in the labor force and have positive income. "No High School" refers to individuals with 11 or fewer years of schooling. "High School" refers to individuals with exactly 12 years of schooling. "Any College" refers to individuals with one or more years of college. All estimates report robust standard errors.

Figure A18: Incarceration Rate of Immigrants and US-born Men, Fixing the Immigrant Country-of-Origin Composition at 1940 Levels



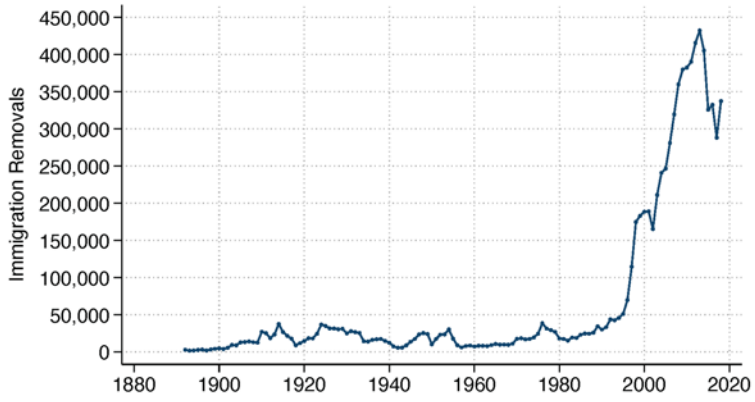
Notes: The first (orange) and third (blue) series plot the raw incarceration rates of immigrant men and US-born men, analogous to those in Figure 1. The second series (dashed red) holds fixed the immigrant composition in 1940 using the five country-of-origin groups ("old" Europeans, "new" Europeans, Chinese immigrants, Mexican and Central American immigrants, and immigrants from the "rest of the world") and calculates the counterfactual incarceration rate after 1940 if each group's incarceration had evolved naturally but their proportion in 1940 (as a share of all immigrants) remained fixed. This figure makes clear that if the immigrant composition had not changed since 1940, the immigrant incarceration rate would be lower than it actually is, and the immigrant-US-born incarceration gap would thus be even larger today.

Figure A19: Incarceration Gap between Immigrants and US-born Men, Excluding Recent Immigrants



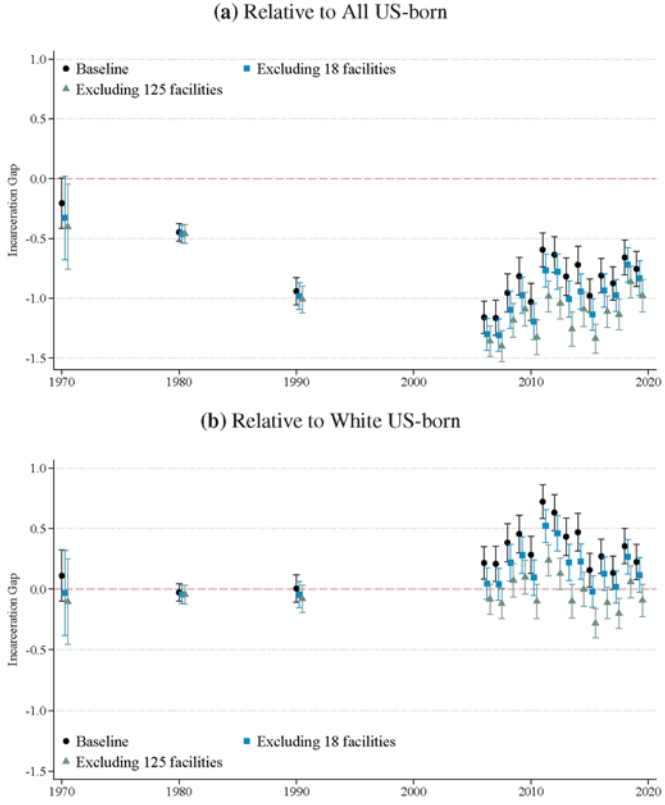
Notes: This figure plots the estimated values of β from equation (1) and varying the sample of immigrants. The first series reproduces the baseline estimate including all immigrants regardless of time since arrival. The second and third series exclude individuals who arrived to the US within five and ten years, respectively. Estimates for 1940–1960 are omitted because the Census did not include a question about time since arrival to the United States in these years. All estimates report robust standard errors.

Figure A20: Number of Removals, 1892-2018



Notes: This figure plots the annual number of removals of inadmissible or deportable individuals between 1892 and 2018 using data from the 2018 Yearbook of Immigration Statistics of the Department of Homeland Security.

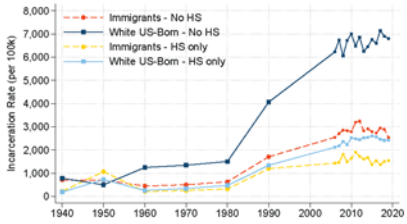
Figure A21: Incarceration Gap between Mexican and Central American Immigrants and US-born Men, Excluding Areas with ICE Facilities



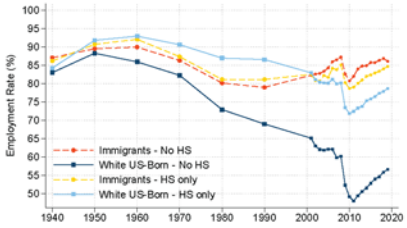
Notes: This figure plots the estimated values of β from equation (1) for Mexican and Central American immigrants and US-born men ages 18-40. Panel (a) compares these immigrants to all US-born men. Panel (b) restricts the comparison to white US-born men. The first series in each panel uses the baseline sample. The second series excludes the areas that included the 18 Immigration and Customs Enforcement (ICE) contract detention facilities and service processing centers as of 2022 (14-17 areas depending on the year). The third series excludes the areas that included the 125 ICE contract detention facilities, service processing centers, facilities under intergovernmental service agreements, and US Marshall's administered facilities as of 2022 (63-110 areas depending on the year). For more details on the areas excluded from the sample, see Online Appendix B. All estimates report robust standard errors.

Figure A22: Incarceration, Labor Market, Family Formation, and Health Outcomes of Immigrants and White US-born Men Without Any College Education, 1940-2019

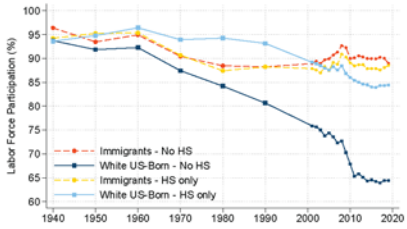
(a) Incarceration Rate (per 100,000)



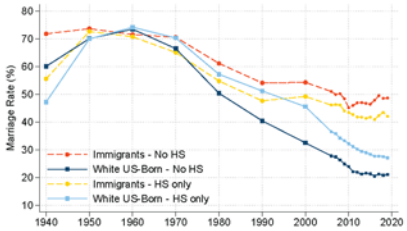
(b) Employment Rate (%)



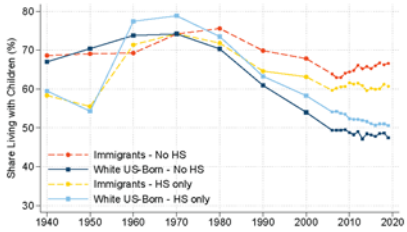
(c) Labor Force Participation Rate (%)



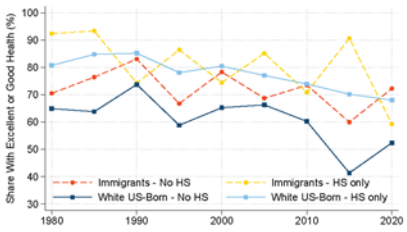
(d) Marriage Rate (%)



(e) Share Living with Children (%)



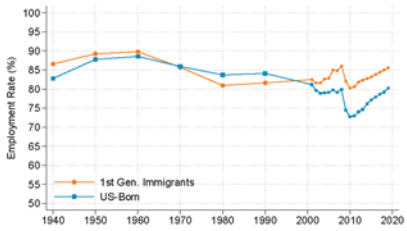
(f) Share with Excellent or Good Health (%)



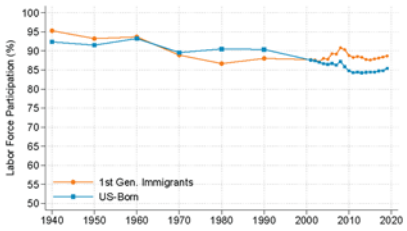
Notes: This figure is analogous to Figure 4 but restricts the sample of US-born men to white US-born men. For more details, see the note to Figure 4 and Online Appendix B.

Figure A23: Labor Market, Family Formation, and Health Outcomes of Immigrants and All US-born Men (Regardless of Educational Attainment), 1940-2019

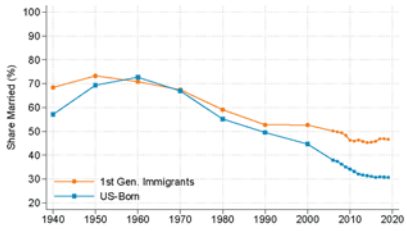
(a) Employment Rate (%)



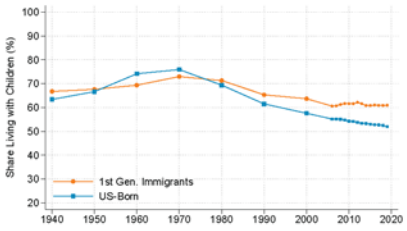
(b) Labor Force Participation Rate (%)



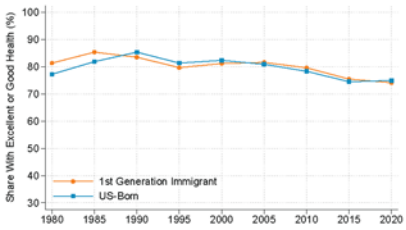
(c) Marriage Rate (%)



(d) Share Living with Children (%)



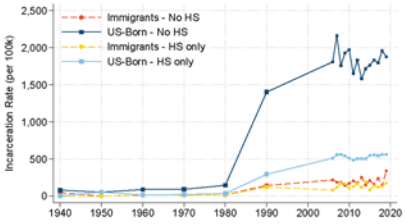
(e) Share with Excellent or Good Health (%)



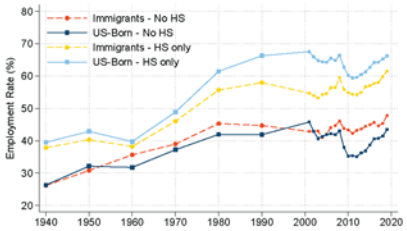
Notes: This figure is analogous to panels (b)-(f) of Figure 4 but considers all immigrants and US-born men regardless of educational attainment. For more details, see the note to Figure 4 and Online Appendix B.

Figure A24: Incarceration, Labor Market, Family Formation, and Health Outcomes of Immigrants and US-born Women Without Any College Education, 1940-2019

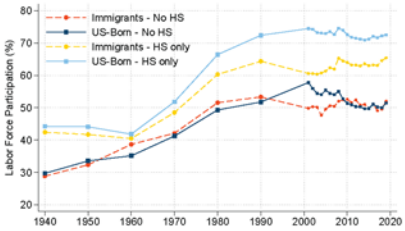
(a) Incarceration Rate (per 100,000)



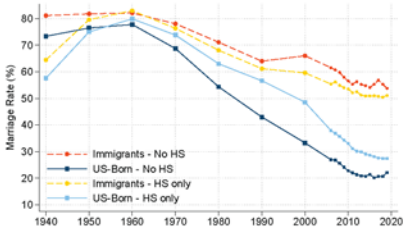
(b) Employment Rate (%)



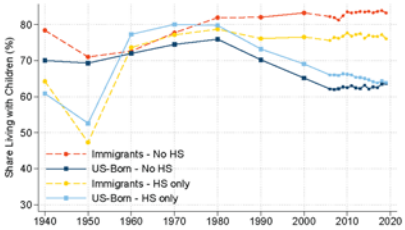
(c) Labor Force Participation Rate (%)



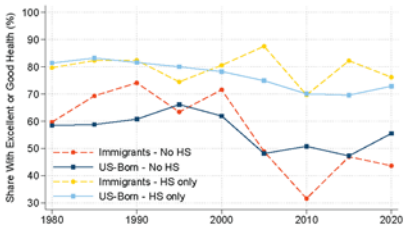
(d) Marriage Rate (%)



(e) Share Living with Children (%)

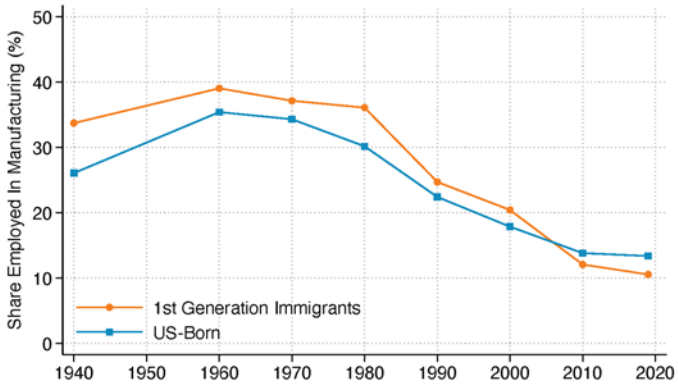


(f) Share with Excellent or Good Health (%)



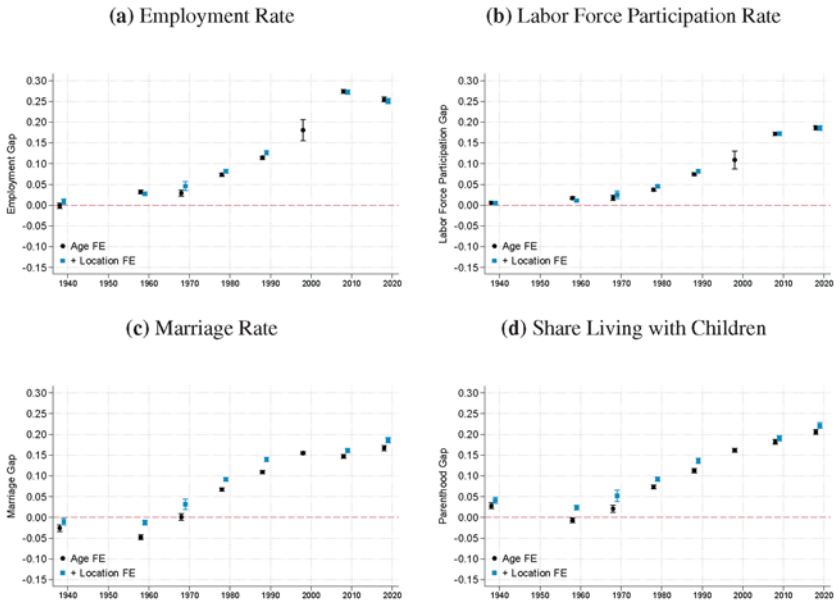
Notes: This figure is analogous to Figure 4 but expands the sample to include female immigrants and US-born individuals. For more details, see the note to Figure 4 and Online Appendix B.

Figure A25: Share of Low-Educated Immigrants and US-born Men Employed in Manufacturing



Notes: This figure plots the share of immigrants and US-born men ages 18-40 that were employed in manufacturing between 1940 and 2019. The sample is restricted to non-institutionalized men without a high school degree and who were in the labor force. This figure shows that the shares resembled each other until 2010, suggesting that compositional differences across declining industries cannot alone explain the immigrant-US-born differences in labor market outcomes (depicted in Figure 4).

Figure A26: Differences in Labor Market and Family Formation Outcomes of Immigrants and US-born Men Without a High School Degree, Adjusting for Geography, 1940-2019



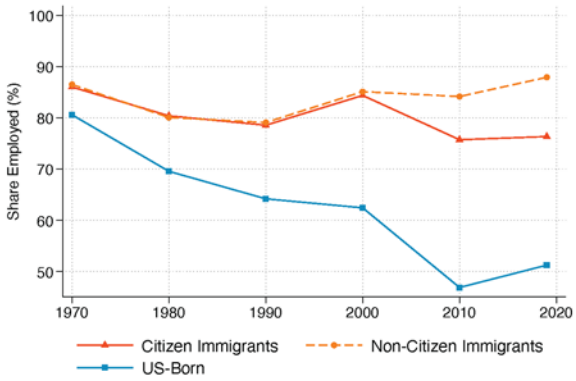
Notes: This figure plots the estimated values of β from equation (1) for immigrants and US-born men without a high school degree. Each panel considers a different outcome. The sample is non-institutionalized men ages 18-40 in panels (a)-(c) and ages 30-50 in panel (d). The first series plots the estimated gaps including individual age fixed effects. The second series adds location fixed effects. For 1940, we include county-of-residence fixed effects. For 1970 and 1980, we include fixed effects for each county group. For 1960 and 1990 onward, we include Public Use Metropolitan Area (PUMA) fixed effects. For more details, see the note to Figure 4 and Online Appendix B. All estimates report robust standard errors.

Figure A27: Differences in Labor Market and Family Formation Outcomes of Immigrants and US-born Men With Only a High School Degree, Adjusting for Geography, 1940-2019

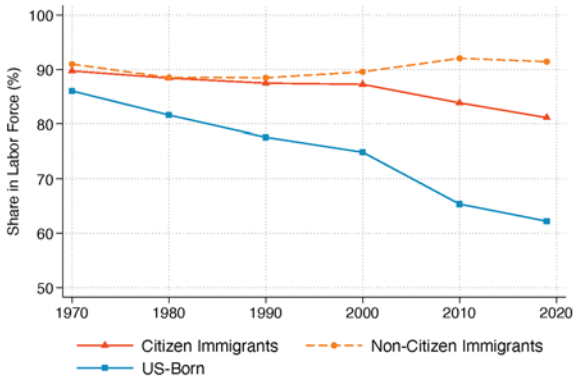
Notes: This figure plots the estimated values of β from equation (1) for immigrants and US-born men with only a high school degree. Each panel considers a different outcome. The sample is non-institutionalized men ages 18-40 in panels (a)-(c) and ages 30-50 in panel (d). The first series plots the estimated gaps including individual age fixed effects. The second series adds location fixed effects. For 1940, we include county-of-residence fixed effects. For 1970 and 1980, we include fixed effects for each county group. For 1960 and 1990 onward, we include Public Use Metropolitan Area (PUMA) fixed effects. For more details, see the note to Figure 4 and Online Appendix B. All estimates report robust standard errors.

Figure A28: Employment and Labor Force Participation Rates of Citizen and Non-Citizen Immigrants and US-born Men Without a High School Degree

(a) Employment Rate

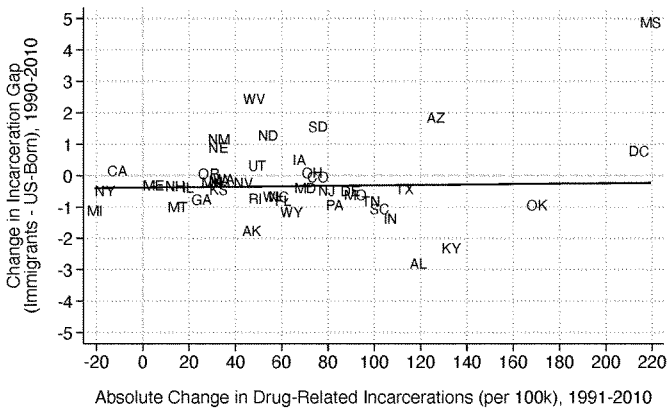


(b) Labor Force Participation Rate



Notes: This figure plots employment and labor force participation rates for citizen immigrants, non-citizen immigrants, and US-born men between 1970 and 2019. The sample is restricted to non-institutionalized men ages 18-40 who did not complete high school. Although the magnitude of the gaps between citizen migrants and the US-born are somewhat smaller in recent decades, the figure shows that less-educated citizen immigrants also have significantly higher employment and labor force participation rates than their US-born counterparts. It is thus unlikely that the availability of social insurance can explain the immigrant-US-born differences in labor market outcomes (depicted in Figure 4).

Figure A29: State-Level Changes in Drug-Related Incarcerations and the Immigrant-US-born Incarceration Gap Between 1990 and 2010



Notes: This figure plots state-level (absolute) changes in the incarceration rate of individuals convicted of drug-related offenses between 1991 and 2010 (x-axis) against changes in the immigrant-US-born incarceration gap, estimated using equation (1), in that same state and time period (y-axis). We use data from the National Corrections Reporting Program to calculate incarceration rates for drug-related offenses (averaging incarcerations between 1991 and 1993 and between 2008 and 2010 to calculate differences over this time period). This figure considers the potential role of drug crimes in explaining the widening of the immigrant-US-born incarceration gap: if US-born men are more likely to commit drug-related offenses and they are more likely to be incarcerated for these offenses than immigrants in the modern time period, then this difference could explain the relative decline in immigrants' incarceration rate. Put differently, if drug-related incarcerations are driving the increase, then we should find that the immigrant-US-born gaps are larger in states that experience large increases in drug-related incarcerations. This figure shows that at least when looking at state-level correlations, this does not seem to be the case.

Table A1: Sample Size for Immigrants, by Year and Country Group

	US-born			All Immigrants			"Old" Europeans			"New" Europeans			Chinese			Mexican & Central Americans			Rest of the world		
	Inc. (1)	Total (2)	Inc. Rate (3)	Inc. (4)	Total (5)	Inc. Rate (6)	Inc. (7)	Total (8)	Inc. Rate (9)	Inc. (10)	Total (11)	Inc. Rate (12)	Inc. (13)	Total (14)	Inc. Rate (15)	Inc. (16)	Total (17)	Inc. Rate (18)	Inc. (19)	Total (20)	Inc. Rate (21)
1870	10,836	5,299,875	204	3,573	1,667,878	214	2,792	1,406,536	199	101	42,743	236	91	46,680	195	96	13,855	693	493	158,064	312
1880	34,615	7,625,747	454	6,322	1,808,660	350	4,572	1,383,229	331	272	98,575	276	404	77,618	520	251	21,868	1,148	823	227,370	362
1900	53,626	11,761,318	456	8,623	2,826,309	305	4,810	1,600,221	301	1,761	737,100	239	196	52,810	371	435	29,255	1,487	1,421	406,923	349
1910	43,631	14,574,042	299	8,165	4,101,636	199	2,621	1,451,220	181	3,697	2,095,851	176	83	23,196	358	687	82,717	831	1,077	448,652	240
1920	51,132	16,339,910	313	9,624	3,661,154	263	1,659	934,362	178	5,618	2,228,389	252	42	14,975	280	1,395	157,491	886	910	325,937	279
1930	149,380	19,709,041	758	14,609	3,030,274	482	2,261	836,267	270	6,278	1,558,613	403	192	22,048	871	3,537	228,384	1,549	2,341	384,962	608
1940	165,699	23,081,996	718	6,826	1,458,866	468	1,166	456,094	256	2,124	607,595	350	168	18,327	917	1,819	105,338	1,727	1,549	271,512	571
1950	556	302,177	726	17	8,946	712	3	2,085	523	2	3,162	246	1	226	1,334	10	1,239	3,346	1	2,234	160
1960	11,515	1,244,704	925	132	42,800	308	14	10,393	135	23	13,746	167	0	1,237	0	55	6,418	857	40	11,006	363
1970	7,179	851,088	844	103	37,146	277	7	7,511	93	13	8,083	161	2	1,358	147	35	5,484	638	46	14,710	313
1980	17,992	1,900,112	947	461	136,617	337	81	17,182	471	30	16,429	183	3	5,243	57	185	37,082	499	162	60,681	267
1990	29,169	1,984,280	2,173	1,909	229,569	1,072	157	19,745	913	72	14,786	545	18	9,960	172	750	89,747	1,232	912	95,331	1,136
2000	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
2010	90,995	1,758,597	3,165	10,336	340,376	1,574	329	17,368	1,156	263	20,280	708	70	16,146	241	7,185	149,787	2,319	2,489	136,795	964
2019	97,028	1,892,429	2,790	8,284	326,127	1,203	263	17,020	857	264	21,283	720	79	22,060	174	5,402	116,614	1,970	2,276	149,150	745

Notes: This table presents the sample size and incarceration rates for US-born men and for immigrant men from different country groups. The sample is restricted to men ages 18—40. Whenever available, we use sample weights provided by IPUMS to calculate incarceration rates (per 100,000). For more information about each data source, see Online Appendix B.

Table A2: Overlap Between Alternative Incarceration Measures in the Full Count Censuses

	US-Born				Immigrants			
	GQ (1)	Preferred (2)	Both (3)	Share (%) (4)	GQ (5)	Preferred (6)	Both (7)	Share (%) (8)
1870	9,012	10,836	9,012	83	3,174	3,573	3,174	89
1880	28,613	34,615	28,262	82	5,006	6,322	4,970	79
1900	35,904	53,626	33,748	63	6,788	8,623	6,554	76
1910	–	43,631	–	–	–	8,165	–	–
1920	38,689	51,132	36,949	72	7,829	9,624	7,561	79
1930	125,993	149,380	122,197	82	13,077	14,609	12,672	87
1940	126,576	165,699	57,691	35	4,758	6,826	2,320	34

Notes: This table displays the number of incarcerated individuals in each Census year separately by nativity and by measure of incarceration. "GQ" refers to the number of men classified as incarcerated using the IPUMS group quarters variable. "Preferred" refers to the number of men classified as incarcerated using our preferred measure that combines information from the group quarters variable with the original strings of the "group quarters," "occupation," and "relationship to household head" variables. "Both" refers to the number of men classified as incarcerated under both approaches. "Share" refers to the share of incarcerated men under the preferred measure that would have also been classified as incarcerated using only using the group quarters variable (column 3 divided by column 2 and column 7 divided by column 6). For more details, see Online Appendix B.

Table A3: Characteristics of Immigrants and US-Born Men, by Incarceration Status and Period

	1870–1940				1940–1970				1980–2019			
	US-Born		Immigrants		US-Born		Immigrants		US-Born		Immigrants	
	Incarcerated (1)	Not Incarcerated (2)	Incarcerated (3)	Not Incarcerated (4)	Incarcerated (5)	Not Incarcerated (6)	Incarcerated (7)	Not Incarcerated (8)	Incarcerated (9)	Not Incarcerated (10)	Incarcerated (11)	Not Incarcerated (12)
Age	26.9	27.9	29.3	30.5	27.2	28.3	28.5	30.5	29.2	28.4	29.9	30.3
Married	28.8	51.6	26.8	56.3	31.0	63.2	33.4	65.3	14.3	34.6	24.4	46.9
Region												
North	20.8	24.5	44.6	47.9	18.4	23.5	26.6	41.3	13.9	16.9	13.8	19.8
Midwest	29.0	33.2	26.6	33.4	23.3	28.3	13.1	20.1	20.1	23.3	9.0	12.4
South	41.7	34.7	9.8	5.8	39.6	32.2	28.7	13.3	45.6	37.1	43.0	33.5
West	8.4	7.5	18.9	12.9	18.7	16.0	31.6	25.3	20.4	22.7	34.1	34.3
Education												
Illiterate	15.6	7.5	16.1	11.9	.	.	.	.	.	.	.	.
Literate	84.4	92.5	83.9	88.1	.	.	.	.	.	.	.	.
No High School	.	.	.	.	77.8	45.2	78.7	52.3	36.3	9.6	50.6	25.4
Only High School	.	.	.	.	17.3	30.2	13.9	19.6	49.8	40.1	35.9	31.6
Any College	.	.	.	.	4.9	24.6	7.4	28.0	13.9	50.3	13.5	43.0
Race												
White	63.5	87.3	94.4	96.5	58.5	89.1	84.0	88.7	45.0	77.8	50.2	46.2
Black	36.0	12.5	2.5	0.6	39.8	10.1	8.2	3.2	43.8	13.2	11.7	8.1
Other	0.5	0.3	3.1	2.8	1.6	0.8	7.7	8.1	11.1	9.0	38.1	45.7
Observations	343,220	74,966,713	50,916	17,044,995	1,301,780	156,554,705	27,550	6,891,281	19,443,177	666,781,639	1,880,911	140,088,216

Notes: This table presents characteristics of immigrant and US-born men by incarceration status for three sample periods. The sample is restricted to men ages 18–40. Whenever available, we use sample weights provided by IPUMS to calculate averages for each variable. refers to the unweighted number of observations. For more information about each data source, see Online Appendix B.

ONLINE APPENDIX B: ADDITIONAL DETAIL ON DATA SOURCES

Data Sources: Census and ACS

We combine the full-count decennial Censuses between 1870 and 1940 (excluding 1890) with the largest available subsample of each Census between 1950 and 2000 and the American Community Survey for the more recent period. We recover the full-count decennial Censuses from the IPUMS datasets in the NBER server (Ruggles et al. 2021) and the Census subsamples and the ACS from the IPUMS website (Ruggles et al. 2022). In particular, we use the following:

- 1870, 1880, 1900, 1910, 1920, 1930, and 1940 full-count decennial Censuses.[1]
- 1950 1% weighted sample
- 1960 5% unweighted (flat) sample
- 1970 pooled 1% FORM 1 unweighted state, metro and neighborhood samples. Form 1 compiles a set of variables that were asked to 5% of the population, which is included in these samples
- 1980 5% unweighted (flat) state sample
- 1990 5% weighted state sample
- 2000 5% weighted state sample
- 2005–2019 annual ACS weighted sample corresponding to 1% of the population in each year
- 2008–2012 5-year ACS weighted sample corresponding to 5% of the population
- 2015–2019 5-year ACS weighted sample corresponding to 5% of the population

We also collect historical subsamples from IPUMS for robustness exercises:

- 1870 1% unweighted (flat) sample
- 1880 10% weighted sample
- 1900 5% unweighted (flat) sample
- 1910 1% unweighted (flat) sample
- 1920 1% unweighted (flat) sample
- 1930 5% unweighted (flat) sample
- 1940 1% weighted sample

We use annual ACS samples to plot incarceration rates and five-year samples to estimate differences in incarceration between immigrants and the US-born. We do not pool annual and five-year samples for the same analysis.

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[1] For 1870–1940, we use the full-count Census files located in the following directory of the NBER server: /home/data/census-ipums/v2021/dta/. For 1940, we use the file located in /homes/data/cens1940/20180316/100files/ to produce alternative measures of incarceration (i.e., our "GQ" and "Relate" measures, as described in this appendix).

Our baseline results restrict the sample to men ages 18–40. Given its small sample size, we exclude the 1950 Census from results that split immigrants by country-of-origin group. Throughout the analysis, we utilize person weights provided by IPUMS.

Defining US-born, immigrants, and country groups

We define immigrants as individuals who were not born in any US state or outlying US area or territory. The US-born includes every individual not coded as an immigrant under this definition. Following Butcher and Piehl (2007), we exclude from the sample individuals born in outlying areas of the United States as well as those born abroad to US citizens.

We define the following five countries-of-origin groups for immigrants:

- "Old Europeans": individuals born in the countries that belong to Northern and Western Europe including Germany (IPUMS codes 400–429 and 453).
- "New Europeans": individuals born in the countries that belong to Southern Europe, Central/Eastern Europe, and the former USSR (IPUMS codes 430–499 excluding 453).
- Individuals born in China.
- Individuals born in Mexico and Central America.
- "Rest of the World": individuals born in other countries in Asia, Africa, Oceania, the Caribbean, and South America.

Measuring incarceration

Full-count censuses

Incarceration can in principle be measured in the full-count data using the "group quarters" and "group quarter type" variables available from the Census. Prisoners are defined as those who reside in institutional and other group quarters and whose group quarter type corresponds to correctional institutions. Correctional institutions include federal and state correctional facilities, prisons, penitentiaries, military prisons, local correctional facilities, jails, school juvenile delinquents, reformatory, camp or chain gangs, and houses of correction.

However, these variables were not consistently coded to identify prisoners in the full-count Census data (see Eriksson 2020 for a discussion).[2] Common issues with these variables involve individuals who were not incarcerated but were counted as such, individuals that were actually incarcerated but appeared in households, and individuals that lived in prisons but were not incarcerated (such as prison guards). An additional issue is the classification of individuals defined solely as inmates, who may not be incarcerated in a correctional facility (e.g., inmates who frequent or live in mental and elderly institutions or those in non-institutional group quarters).

To account for these issues, we construct our preferred incarceration measure for the full-count Census data using the following procedure:

[2] Eriksson (2020) implements a different classification procedure from us. Specifically, that paper uses the 1920–1940 full-Count Census along with images looked up by hand to classify individuals born in fourteen states in the US South as incarcerated.

1. For each individual in the data, we retrieve their "group quarters," "group quarter type," "relate," and "occupation" variables (i.e., the code as well as the original strings as reported in the Census).
2. Next, we define individuals as incarcerated using information in the "relate" string variable if they meet *any* the following requirements:
    a. *Explicit correctional string:* Individuals who have the following words and their spelling variations in the "relate" string variable: "Prisoner," "Convict," or "Jail." At this step, we exclude individuals whose "relate" string variable conveys a relationship to "Prisoner," "Convict," or "Jail," such as "Daughter," "Son," "Wife," "Head," as well as "Guard," "Jailer," "Chief," "Helper," "Officer," "Manager," "Charge," "Superintendent," including their spelling variations. (i.e., we exclude an individual whose "relate" string variable is "Prisoner guard," "Convict daughter," etc.).
    b. *Inmate and explicit correctional institution string*: Individuals who have the following words and their spelling variations combined with the word "Inmate" in the "relate" string variable: "Prison," "Jail," "Penitentiary," "Reformatory," and "Correction." We exclude individuals classified by the "group quarter type" variable as part of a mental institution, an institution for the elderly, handicapped, and poor, or a non-institutional group quarter. This avoids counting individuals who reside in these institutions as inmates, but for whom it is not clear that they are serving a criminal sentence.
    c. *Inmate with missing information in the string variable:* Individuals who have the word "Inmate" (without any additional words) in the "relate" string variable or who have a missing value, an "X," or a "*" in the "relate" string variable. These individuals are classified as incarcerated if either:
        i. their "group quarters" string variable contains the words "Prison," "Jail," "Penitentiary," "Reformatory," "Correction," "Convict," "Delinquent," "Penal," and other grammatical variations of these words; or
        ii. their "group quarters type" variable code corresponds to a correctional institution when the relate string says "Inmate." For individuals with missing values, "X," or "*" in the relate string variable, we additionally condition on whether the individual is an institutional inmate based on their "relate" variable code.
3. We follow the steps in (2) to classify individuals as incarcerated using the "occupation" string variable.
    a. We follow the procedure in (2.a) (i.e., an individual is identified as incarcerated if their occupation includes "Prisoner," "Convict," or "Jail."). Because the "occupation" string does not convey familial relationships, we do not exclude any individuals in this step based on their relationship to household. However, we do exclude individuals if their occupation denotes a potential non-prisoner occupation ("Guard," "Jailer," "Chief," "Helper," "Officer," "Manager," "Charge," and "Superintendent").
    b. We replicate step (2.b) exactly.

c. We replicate step (2.c), but in addition to "Inmate," "X," and "*," we also include individuals in this step whose occupation string variable says: "No Occupation," "No," "None," "Without Occupation," "Nothing," or has a missing value.[3]

In our preferred measure of incarceration, we define an individual as incarcerated if they are classified as such in steps one through three.[4]

The 1870 Census does not include the "relate" string variable. We classify individuals as incarcerated in these years using the "occupational" string variable (step 3). In addition, we include individuals as incarcerated if their "relate" variable code is "institutional inmate" and their "group quarter type" variable code corresponds to correctional institutions.

The 1910 Census does not identify group quarter types. In this case, we rely on our preferred measure to classify prisoners based on strings of the "relate" and "occupation" variables that clearly identify individuals as prisoners (as in step 2.a). However, due to the lack of the "group quarter" string variable and the "group quarter type" variable, we are unable to implement steps 2.b, 2.c., 3.b, and 3.c.

For robustness checks, we also construct two alternative measures of incarceration, which we refer to as the "GQ measure" and the "relate measure." The "GQ measure" refers to individuals who reside in institutional and other group quarters and whose group quarter type corresponds to correctional institutions (without any additional modifications). The "relate measure" refers to individuals who satisfy the "GQ measure" and either steps (2.a) or (2.b). In the "relate measure," we exclude individuals who appear to be incarcerated via the "GQ measure," but who are coded as family members of the household head in their "relate" variable code.

We note that the paper's main takeaways are similar when using just IPUMS group quarters variable, rather than this more detailed approach. Indeed, Table A2 shows that between 1870–1930, more than three-fourths of individuals that we classify as incarcerated are coded as living in a correctional institution, and this share is comparable across immigrants and the US-born.

Census subsamples and ACS

Between 1950 and 1980, we define prisoners as those who belong to institutional and other group quarters and whose group quarter type corresponds to correctional institutions (analogous to the GQ measure described above). For 1910, group quarter types were imputed by IPUMS. Between 1990 and 2019, the "group quarter" variables only allow us to identify institutionalized individuals, but not those who are institutionalized in adult correctional facilities. In this case, we identify incarcerated individuals as those who are classified as living in institutional group quarters.

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[3] To be conservative, when an individual is classified as incarcerated using missing information under the relate string (step 2c), but not under the occupation string (step 3c), we only identify an individual as incarcerated if they are classified as institutional inmates in their "relate" variable code or if their "relate" variable string is the word "Inmate."

[4] The 1940 Census presents a comparability issue among large households. According to IPUMS: "Before 1940 and in 1980–1990, units with 10 or more individuals unrelated to the householder are considered group quarters." We adjust our "preferred" measure in 1940 to include individuals whose "relate" variable string says "Inmate" (in cases where the "group quarters" variable code is "Other Group Quarters" and the "group quarter type" variable code indicates a "Non-group quarter household"). For more details, see https://usa.ipums.org/usa-action/variables/GQ#comparability_section.

Other variable definitions

Education

We use the "education" variable in each sample to assign individuals to three educational groups: high school dropouts (i.e., those with no schooling up to those who completed grade 11), high school only (grade 12), and any college (1 or more years of college). These three groups comprise the educational fixed effects used in our analysis. This variable is defined starting with the 1940 Census.

Race

We use the "race" variable in each sample to assign individuals to three racial groups: white, Black, and "other" (referring to individuals whose race classification is neither white nor Black). These three groups comprise the race fixed effects used in our analysis.

Marital status

We use the "marital status" variable in each sample to assign individuals to three marital status groups: married (married, spouse present or absent); separated, divorced, or widowed; and never married/single. These three groups comprise the marital status fixed effects used in our analysis, and we use the married category to construct marriage rates. This variable is defined for every year.

State of residence

To compare individuals living in similar geographies, we use state-of-residence fixed effects. Although most individuals convicted of crimes are incarcerated in their state of residence, we cannot control for geography below the state level because inmates can be incarcerated in correctional facilities far from their initial residential location (i.e., their county of residence at the time of the Census may not reflect their county of residence prior to incarceration).[5]

Parenthood status

We utilize the variable "NCHILD" available via IPUMS to calculate the share of men living with children of their own among individuals who are not incarcerated. This variable is defined for every year.

Citizenship status

This variable is not available in 1880 and 1960. In 1870, 1900, and 1910, citizenship status was defined for foreign-born men older than 20. From 1920 onwards, it was defined for all foreign-born individuals. Individuals born in any US state are classified as citizens in all of these samples.

ICE Facilities and Deportations Data

We identify Immigration and Customs Enforcement (ICE) detention facilities from the list provided in the 2022 ICE Detentions Statistics Appendix (available at https://www.ice.gov/detain/detention-management). We identify two groups of facilities: The first group includes 18 ICE-owned service processing centers and privately-owned contract detention

[5] This assumption may not be true for those incarcerated for federal offenses because individuals might be sent to federal prisons outside of their state of residence. Nevertheless, the share of inmates in federal prisons is generally small (5–7% of incarcerated individuals in 1990 and 2000; Beck and Harrison 2001).

facilities.[6] In 2017, these types of facilities were 6% of the total number of facilities used for detention, but held approximately 28% of detainees.[7] The second group extends this list to 107 facilities (for a total of 125 facilities) operated under agreements with local and state governments and federal agencies. This group includes facilities under intergovernmental service agreements and US Marshall's administered facilities.

We geolocate these facilities and assign them to their corresponding PUMA in 1990 (1,726 total PUMAs), in 2006–2011 (2,069 total PUMAs), and 2012–2019 (2,351 total PUMAs) using shapefiles provided by IPUMS. For 1970 and 1980, we follow the same procedure using county group shapefiles provided by IPUMS (309 and 1,154 county groups in 1970 and 1980, respectively). Given changes in PUMA/county group geographic areas across time as well as the proximity of certain facilities to each other, we end up tagging 14–17 areas as including ICE facilities in the first group and 63–110 areas as including facilities in the second group. We exclude these areas from the sample in the second and third series of Figure A21, respectively.

To consider how the incarceration rate would change after including deportations, we use the 2006–2019 reports from the Department of Homeland Security on Immigration Enforcement Actions. We focus on removals of individuals with criminal histories.

Health

We use data from the General Social Survey (GSS) to measure health outcomes. We focus on the 1977–2021 period, in which individuals can be classified as foreign-born. We group annual data into five-year bins (e.g., the 2000 point includes the 1998–2002 survey waves). We rely on the "health" variable, identifying individuals who report an "excellent" or "good" health condition. Given small samples, we focus on men ages 18–65.

Admissions for Drug-Related Offenses

We use data from the National Corrections Reporting Program (NCRP; ICPSR 36404) between 1991 and 2010. We derive the stock of incarcerated individuals for each year by keeping all records of individuals admitted to prison before or during that calendar year who are released after that same year. We then sum the number of drug-related incarcerations in each state and year and compute average drug incarceration rates at the state level for the 1991–1993 and 2008–2010. To calculate incarceration rates, we use state population counts from the 1990 and 2010 Census.

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[6] See also https://www.ice.gov/doclib/foia/media-requests/09foia5638detentionfacilitylist.xls.

[7] "ICE Does Not Fully Use Contracting Tools to Hold Detention Facility Contractors Accountable for Failing to Meet Performance Standards." Office of Inspector General, Department of Homeland Security. https://www.oig.dhs.gov/sites/default/files/assets/2019-02/OIG-19-18-Jan19.pdf.

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Mood, anxiety, and personality disorders among first and second-generation immigrants to the United States

Christopher P. Salas-Wright[a,*], **Njeri Kagotho**[b], and **Michael G. Vaughn**[c]

[a]School of Social Work, The University of Texas at Austin, 1925 San Jacinto Blvd D3500, Austin, TX 78712-0358, United States. salaswright@utexas.edu

[b]School of Social Work, Adelphi University, School of Social Work, P.O. Box 701, Garden City, NY 11530-0701, United States. nkagotho@adelphi.edu

[c]School of Social Work, College for Public Health and Social Justice, Saint Louis University, Tegeler Hall, 3550 Lindell Blvd., jSt. Louis, MO 63103, United States. mvaughn9@slu.edu

Abstract

A careful examination of the multigenerational relationship between immigrant status and mental disorders can provide important information about the robustness and nature of the immigrant-mental health link. We examine immigrant status as a protective factor against mental illness, assess intergenerational effects, examine differences across race/ethnicity, and report the prevalence of mood, anxiety, and personality disorders of immigrants across major world regions. We employ data from the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) and compare first (n = 5,363) and second-generation (n = 4826) immigrants from Asia, Africa, Europe, and Latin America to native-born Americans (n = 24,461) with respect to mental disorders. First-generation immigrants are significantly less likely than native-born Americans to be diagnosed with a mood, anxiety, or personality disorder, though the prevalence of mental health diagnoses increases among second generation immigrants. Similar results were observed for immigrants from major world regions as the prevalence of psychiatric morbidity was lower among immigrants from Africa, Latin America, Europe, and Asia compared to native-born Americans. Findings provide evidence in support of the notion that the immigrant paradox may be extended to include mood, anxiety, and personality disorders in the United States.

Keywords

immigrant; mental health; immigration; mood disorders; anxiety disorders; personality disorders; immigrant paradox

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*Corresponding Author, Christopher P. Salas-Wright, 1925 San Jacinto Blvd D3500, Austin, TX 78712-0358. salaswright@utexas.edu, Phone: (512) 471-0765.

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1. Introduction

In recent years, a growing number of studies have examined the relationship between immigrant status and mental disorders (Shrout et al., 1992; Vega et al., 1998; Grant et al., 2004; Cantor-Graae and Selten, 2005; Breslau et al., 2007; Takeuchi et al., 2007; Alegría et al., 2008; Breslau et al., 2009; Bourque et al., 2011; Cantor-Graae and Petersen, 2013). This body of work points to a multifaceted relationship in which the nature of the association between immigrant status and mental disorders varies according to both the particular characteristics of the immigrant and the nature of the disorder in question. For instance, prior research suggests that immigrants to the United States are a heterogeneous population (Salas-Wright et al., 2014a) and that factors such as immigrant generation (i.e., first-generation, second-generation), age of immigration, and duration as an immigrant in the receiving nation have important implications for the relationship between immigrant status and mental disorders (Vega et al., 1998; Breslau et al., 2007, 2009; Cantor-Graae and Petersen, 2013). Mental health is also a heterogeneous construct and the various types of mental disorders appear to be differentially related to immigrant status in both United States and European samples. Indeed, while meta-analytic reviews have now demonstrated quite convincingly that the prevalence and incidence of psychotic disorders tends to be greater among immigrants than among the native-born (Cantor-Graae and Selten, 2005; Bourque, Van der Ven, and Malla, 2011), research also points to immigrant status as a protective factor for most mood and anxiety disorders (Vega et al., 1998; Grant et al., 2004; Breslau et al., 2007; Takeuchi et al., 2007; Alegría et al., 2008; Breslau et al., 2009; Cantor-Graae and Petersen, 2013).

Despite the manifold advances made by previous studies, several important facets of the immigrant-mental disorder link have yet to be fully explored. To begin, while numerous studies have examined mood and anxiety disorders among particular immigrant groups in the United States (i.e. Hispanics, Asian/Pacific Islanders), few studies have systematically examined the multigenerational relationships between immigrant status and an extensive array of mood, anxiety, and personality disorders among immigrants in general. That is, additional research is needed with respect to the prevalence of particular mood (i.e., bipolar disorder, major depression, dysthymia), anxiety (i.e., generalized anxiety disorder, panic disorder, social phobia, specific phobia, and posttraumatic stress disorder), and personality (i.e., antisocial, avoidant, borderline, narcissistic, obsessive-compulsive, paranoid, schizoid, schizotypal) disorders among first and second-generation immigrants in contrast with native-born Americans. Second, few studies have examined the stability of the relationship between immigrant status and mental disorders among racial/ethnic minority and non-minority immigrants. In light of research suggesting a link between experiences of discrimination and mental health (Pascoe and Richman, 2009), further research into the potentially segmented nature of the relationship between immigrant status and mental disorders is warranted. In all, while much has been learned in recent years, a careful examination of the multigenerational links between immigrant status and mood, anxiety, and personality disorders can provide important information about the robustness and nature of the immigrant-mental health link.

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1.1. Research Context

Each phase in the migration process presents unique migration-related stressors. The pre-flight phase—the period preceding the actual migration event—is especially dire for individuals coerced into undertaking this relocation. Mental illness risk factors experienced in this phase include prolonged civil conflicts and wars (Goodman, 2004), sexual and physical abuse (Crisp, 2000), separation from family (Paardekooper et al., 1999), and prolonged stays in refugee and other transitionary camps (Steel et al., 2006). Individuals who have endured these experiences report mental health and behavioral disorders, including depression, anxiety, psychosis, anger, violence, and posttraumatic stress disorder (Paardekooper et al., 1999; Steel et al., 2002; Fazel et al., 2005; Pumariega et al., 2005; Steel et al., 2006; Liddell et al., 2013; Salas-Wright and Vaughn, 2014). In the resettlement phase, immigrants often have to contend with and acclimatize to a new socio-cultural and economic environment. Factors such as a drop in socio-economic status (Aycan and Berry, 1996; Porter and Haslam, 2005), changing family and gender roles and expectations (Potocky-Tripodi, 2002), cultural dissonance, and cultural bereavement (Choi et al., 2008) are cited as post-migration mental illness triggers.

In addition to migration-related stressors, immigrant populations face barriers to mental health services both before and after relocation. Inadequately structured and financed mental health systems across many developing economies means that a significant number of individuals migrating from these regions have had limited contact with mental health professionals (World Health Organization, 2009). They are less likely to have had access to preventive or curative mental health services and are more likely to experience undiagnosed mental disorders, and have minimal knowledge regarding the primacy of mental health in overall health and well-being (Derose et al., 2009). After migration, low rates of health insurance access (Derose et al., 2009), and other socio-cultural impediments including stigma (Nadeem et al., 2007), linguistic challenges (Sentell et al., 2007; Kim et al., 2011), and lack of ethnically-matched providers (Sue et al., 1991) account for obstacles faced in mental health services utilization.

It is therefore of interest to note that, despite the manifold mental illness triggers associated with immigration, a number of studies have found that immigrant populations tend to report lower rates of most mood and anxiety disorders as compared to their native-born peers (Shrout et al., 1992; Vega et al., 1998; Alegría et al., 2008; Grant et al., 2004; Breslau et al., 2007; Takeuchi et al., 2007; Breslau et al., 2009; Cantor-Graae and Petersen, 2013). Several protective factors are postulated to explain these counter-intuitive results, including: social support, cultural beliefs and practices, extended family support networks, and lower rates of substance misuse (Escobar, 1998; Escovar et al., 2000; Schwartz et al., 2010; Marks et al., 2013; Salas-Wright et al., 2014b; Salas-Wright and Vaughn, 2014; Suárez-Orozco et al., 2014). In addition, the healthy immigrant effect—the selective process whereby only those in relatively good health elect to migrate—is also postulated to explain these health dissimilarities (Jasso et al., 2004; Akresh and Frank, 2008). This effect may be particularly applicable for immigrants to the United States due to geography (greater distance and cost necessary to travel from major population centers) and post-migration experiences (employment and educational opportunities). However, the mental health advantage enjoyed

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by first-generation immigrants may not hold fast for second and even third-generation immigrants. First-generation immigrants report better mood/anxiety disorder outcomes compared to subsequent immigrant generations (Harker, 2001; Alegríaet al., 2007; Takeuchi et al., 2007; Williams et al., 2007) whose mental health outcomes mirror those of native-born individuals. Furthermore, even among first-generation immigrants, additional time spent in the diaspora is also associated with declining mental health (Alegría et al., 2007).

It should also be noted that the nature of the relationship between immigrant status and mental health may be distinct among individuals from different racial/ethnic groups and, potentially, among immigrants from various regions of the world. For instance, in light of research on the relationship between discrimination and mental health (Pascoe and Richman, 2009), scholars have increasingly begun to highlight the importance of considering the degree to which factors such as racism, discrimination, and social marginalization may differentially impact racial/ethnic minority and non-minority immigrants (Viruell-Fuentes, 2007; Chung et al., 2008). Moreover, numerous studies have examined how the disruption of family networks and social support may contribute to increased mental distress among immigrants, particularly immigrants from collectivist cultures (Garcia Coll et al., 1996; Oppedal et al., 2004; Alegria et al., 2007). As such, it may be that the links between immigrant status and mental health are distinct among immigrants from global regions with a greater collectivist orientation (i.e., Africa, Asia, Latin America) than among those from regions that tend to be less collectivist in nature (i.e., Europe). Recent studies of immigrant well-being have pointed to regional differences among immigrants across various global regions (Vaughn et al., 2014a, 2014b).

1.2. The present study

This paper explores the immigrant paradox argument by examining the similarities and differences in mental health diagnoses between immigrants to the United States and their native-born peers. Using Wave I and Wave II of the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC), we examine the prevalence of mood, anxiety, and personality disorders across immigrant groups and compare these rates with the native-born population in the United States. In particular, we explore the following questions. First, is immigrant status protective for mood, anxiety, and personality disorders? Second, do the effects of immigrant status hold across immigrant generations and among both racial/ethnic minority and non-minority populations? Finally, what is the prevalence of mood/anxiety and personality disorders of immigrants representing major regions of the world?

2. Method

2.1. Participants

Study findings are based on data from Wave I (2001–2002) and Wave II (2004–2005) of the NESARC. The NESARC is a nationally representative sample of non-institutionalized U.S. residents aged 18 years and older. The design and methods are presented in a summarized form; however, a detailed description of the NESARC procedures is available elsewhere (Grant et al., 2003). The NESARC utilized a multistage cluster sampling design, oversampling young adults, Hispanics, and African-Americans in the interest of obtaining

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reliable statistical estimation in these subpopulations and ensuring appropriate representation of racial/ethnic subgroups.

2.2. Diagnostic assessment

Data were collected through face-to-face structured psychiatric interviews conducted by U.S. Census workers trained by the National Institute on Alcohol Abuse and Alcoholism and U.S. Census Bureau. Interviewers administered the Alcohol Use Disorder and Associated Disabilities Interview Schedule – DSM-IV version (AUDADIS-IV), which provides diagnoses for mood, anxiety, personality, and substance use disorders. The AUDADIS-IV has shown to have good-to-excellent reliability in assessing mental disorders in the general population (Grant et al., 1995; Hasin et al., 1997). Participants had the option of completing the NESARC interview in English, Spanish, or one of four Asian languages (Mandarin, Cantonese, Korean and Vietnamese).

2.3 Measures

2.3.1. Immigrant status—Respondents were asked whether they and their parents were born in the United States of America. Three mutually exclusive categories were created: Respondents who reported having been born outside the United States (n = 5363, 13.86%) were classified as first-generation immigrants. Respondents who reported that they had been born in the United States but at least one parent had been born outside the United States (n = 4826, 15.02%) were classified as second-generation immigrants. Respondents who reported that they and their parents were born in the United States were considered native-born Americans (n = 24,461). It should be noted that both "native-born" and "second-generation" respondents were born in the United States and therefore these terms are slightly imprecise. Nevertheless, we elected to use these terms are they are consistent with the work of scholars in prior research on immigration and health (e.g., Escobar et al., 2000; Glaesmer et al., 2011).

2.3.2. Mental health—Using the AUDADIS-IV, we examined lifetime DSM-IV mood (i.e., bipolar disorder, major depression, dysthymia), anxiety (i.e., generalized anxiety disorder, panic disorder, social phobia, specific phobia, and posttraumatic stress disorder) and personality disorders (i.e., antisocial, avoidant, borderline, narcissistic, obsessive-compulsive, paranoid, schizoid, and schizotypal). To ensure model stability, we limited our analyses to mental disorders with a prevalence of at least 2% in the general population. For each of these disorders, respondents who were identified as having met diagnostic criteria during their lifetime were coded as 1 and all other individuals coded as 0.

2.3.3. Sociodemographic and behavioral controls—The following sociodemographic variables were included as controls: age, gender, race/ethnicity, household income, education level, marital status, region of the United States, and urbanicity. Additionally, in examining the associations between immigration status and mental health disorders, we controlled for diagnoses of lifetime alcohol use disorders (abuse/dependence) as well as lifetime drug use disorders (abuse/dependence on heroin, hallucinogens, cocaine/crack, marijuana, stimulants, painkillers, tranquilizers, or sedatives). Prevalence estimates for substance use disorders among native-born Americans and

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immigrants are not reported in this study, but are available elsewhere (Salas-Wright and Vaughn, 2014).

2.4. Statistical analysis

Logistic regression analyses were conducted that compared non-immigrants with first- and second-generation immigrants to the United States in terms of mood, anxiety, and personality disorders. Adjusted odds ratios (AORs) were considered to be statistically significant if the associated confidence intervals did not cross the 1.0 threshold when controlling for sociodemographic factors and lifetime alcohol and illicit drug use disorders. For all statistical analyses, weighted prevalence estimates and standard errors were computed using Stata 13.1 SE software (StataCorp, 2013). This system implements a Taylor series linearization to adjust standard errors of estimates for complex survey sampling design effects including clustered data. Additional information about the sampling weights used in the NESARC is described in greater detail elsewhere (Grant et al., 2003).

3. Results

3.1. Sociodemographic characteristics of first- and second-generation immigrants

Table 1 displays the sociodemographic characteristics of native-born and *first-generation* immigrant adults ages 18 and older in the United States. No significant differences were observed for age or gender. However, first-generation immigrants to the United States were significantly more likely to be a racial/ethnic minority, to reside in households earning less than $70,000 per year, and to not have completed high school (AOR = 1.29, 95% CI = 1.20–1.39). Compared to native-born Americans, first-generation immigrants were significantly less likely to report a marital status other than married/ cohabitating.

We also examined the sociodemographic characteristics of native-born and *second-generation* immigrant adults ages 18 and older in the United States (not shown). Compared to native-born Americans, second-generation immigrants were significantly more likely to be 65 years or older (AOR = 2.33, 95% CI = 2.17–2.50), and significantly less likely to be between the ages of 35 and 49 (AOR = 0.83, 95% CI = 0.78–0.88) or 50 and 64 (AOR = 0.92, 95% CI = 0.85–0.98). Second-generation immigrants were significantly more likely to be Hispanic (AOR = 0.72, 95% CI = 8.31–9.39) or other race (AOR = 2.65, 95% CI = 2.35–3.00) and significantly less likely to be non-Hispanic black (AOR = 0.72, 95% CI = 0.67–0.77). Second-generation immigrants were also significantly less likely to reside in households earning less than $70,000 per year and to have completed less than a post-high school degree program. Compared to native-born Americans, second-generation immigrants were significantly more likely to be divorced or widowed (AOR = 1.25, 95% CI = 1.16–1.35) or to never have been married (AOR = 1.41, 95% CI = 1.33–1.50). Second-generation immigrants were significantly less likely to reside in either the Northeastern (AOR = 0.82, 95% CI = 0.77–0.88) or Southern United States (AOR = 0.93, 95% CI = 0.87–0.98). No significant differences were identified in contrasting second-generation immigrants and native-born Americans in terms of gender or urbanicity.

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3.2. Mental disorders among first- and second-generation immigrants

Figure 1 displays the prevalence of lifetime mood, anxiety, and personality disorders among native-born Americans and first- and second-generation immigrant adults ages 18 and older in the United States. For all disorders examined in this study, the prevalence of morbidity was lowest among first-generation immigrants followed by second-generation immigrants, and, finally, native-born Americans. For instance, a steady increase in the prevalence of major depressive disorder can be identified in comparing first-generation immigrants (15.28%), second-generation immigrants (19.14%), and native-born Americans (22.98%). Similarly, the prevalence of borderline personality disorder is lowest among first-generation immigrants (3.32%) as compared to second-generation immigrants (5.93%) and native-born Americans (6.30%).

Results presented above are for Wave II data; however, we assessed the stability of the results using Wave I data collected three years prior. Although the prevalence of mental disorders was slightly lower at Wave I than at Wave II and several disorders examined in Wave II were not included in the Wave I data collection (see Figure 2), results of logistic regression analyses for Wave I largely mirrored the overall pattern found for Wave II. Namely, controlling for the same list of sociodemographic factors utilized in the present study, immigrants were found to be significantly less likely to meet diagnostic criteria for nearly all mood, anxiety, and personality disorders examined in the study. Sensitivity analysis could not be conducted for second-generation immigrants as this information was not available in the Wave I data.

Table 2 compares the prevalence of mood, anxiety, and personality disorders among native-born Americans and first-and second-generation immigrants to the United States. Controlling for sociodemographic factors as well as lifetime diagnoses of alcohol and illicit drug use disorders, *first-generation immigrants* were significantly less likely to have met criteria for one or more mood/anxiety disorders (AOR = 0.70, 95% CI = 0.67–0.73). In terms of particular disorders, first-generation immigrants were significantly less likely to have met criteria for most mood disorders (i.e., major depression, bipolar disorder), all anxiety disorders (i.e., generalized anxiety, panic disorder, social phobia, specific phobia, posttraumatic stress disorder) and the majority of personality disorders (i.e., antisocial, avoidance, borderline, paranoid, and schizotypal) examined in this study. Significant associations were not observed for dysthymia and narcissistic, obsessive-compulsive, paranoid, and schizoid personality disorders. However, with the exception of narcissistic personality disorder, all of the personality disorders that were found to be not significantly associated with immigration status when controlling for sociodemographic and substance use factors, were significantly associated with first-generation immigration status at the bivariate level.

Table 2 also compares the prevalence of mood, anxiety, and personality disorders among native-born Americans and *second-generation* immigrants to the United States. Controlling for the same list of sociodemographic and substance use factors, second-generation immigrants were significantly less likely to have met criteria for one or more mood/anxiety disorder (AOR = 0.92, 95% CI = 0.88–0.96). In terms of particular disorders, second-

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generation immigrants were also significantly less likely to have met criteria for the majority of mood and anxiety disorders examined in this study (i.e., bipolar disorder, major depression, panic disorder, specific phobia, and posttraumatic stress disorder). However, in contrast to first-generation immigrants, no significant associations were identified in terms of generalized anxiety disorder, dysthymia, or social phobia. Moreover, as evidenced by non-overlapping confidence intervals, the strength of the association between second-generation immigrant status and major depressive disorder (AOR = 0.86, 95% CI = 0.82–0.90), panic disorder (AOR = 0.88, 95% CI = 0.83–0.93), specific phobia (AOR = 0.84, 95% CI = 0.80–0.88) and posttraumatic stress disorder (AOR = 0.84, 95% CI = 0.79–0.91) was significantly weaker among second-generation immigrants as compared to first generation immigrants.

In terms of personality disorders, compared to native-born Americans, second-generation immigrants were significantly less likely to meet criteria for antisocial (AOR = 0.86, 95% CI = 0.77–0.96) and avoidant personality disorder (AOR = 0.82, 95% CI = 0.71–0.95). With respect to antisocial personality disorder, the strength of the association between immigrant status and morbidity was significantly weaker among second-generation immigrants as compared to first-generation immigrants. Finally, with respect to borderline personality disorder (AOR = 1.11, 95% CI = 1.03–1.20), second-generation immigrants were found to be significantly more likely to meet criteria for this disorder as compared to native-born Americans when controlling for sociodemographic and substance use factors.

3.3. Mental disorders among first and second-generation immigrants by racial/ethnic minority versus non-minority status

To assess the impact of racism/discrimination on the mental health of immigrants, we conducted supplementary analyses examining the lifetime prevalence of mood/anxiety and personality disorders among native-born, first-generation, and second-generation respondents while stratifying by racial/ethnic minority (i.e., Black, non-Hispanic; Hispanic; other race/ethnicity) and non-minority (i.e., non-Hispanic white) status. Controlling for sociodemographic and behavioral confounds, first-generation immigrants from both racial/ethnic minority (AOR = 0.64, 95% CI = 0.60–0.67) and non-minority (AOR = 0.82, 95% CI = 0.77–0.88) groups were significantly less likely to meet criteria for a mood/anxiety disorder. Notably, as evidenced by nonoverlapping confidence intervals, effects were significantly greater for racial/ethnic minority immigrants than for non-minority immigrants. Among second-generation immigrants, the link between immigrant status and mood/anxiety disorders was significant for racial/ethnic minority immigrants (AOR = 0.84, 95% CI = 0.78–0.89) but no significant relationship was observed for second-generation non-minority immigrants. Similarly, first (AOR = 0.70, 95% CI = 0.65–0.75) and second-generation (AOR = 0.79, 95% CI = 0.73–0.86) racial/ethnic minority immigrants were significantly less likely to meet criteria for a personality disorder, but no relationship was identified for non-minority immigrants.

3.4. Mental disorders among immigrants from major global regions

Figure 2 presents the prevalence of mood, anxiety, and personality disorders among immigrants from four major global regions as well as that of native-born Americans. The

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prevalence of mood/anxiety and personality disorders of native-born Americans was greater than the prevalence identified among immigrants from Africa, Latin America, Europe, and Asia. European immigrants were identified as having the highest prevalence of mood/anxiety disorders (35.48%, 95% CI = 34.30–36.68) followed by Latin American (30.41%, 95% CI = 29.78–31.05) and Asian (28.09%, 95% CI = 26.94–29.26) immigrants. The lowest prevalence of mood/anxiety disorders was identified among African immigrants (18.39%, 95% CI = 16.01–21.04). With respect to personality disorders, less variability was observed as the prevalence of personality disorders among immigrants from Africa (14.67%, 95% CI = 11.90–70.94), Europe (15.61%, 95% CI = 14.61–16.65), and Latin America (17.24%, 95% CI = 16.90–17.58) was found to be roughly 15%. The prevalence of personality disorders among Asian immigrants was slightly lower than that of immigrants from other regions of the world (12.98%, 95% CI = 12.32–13.66).

4. Discussion

Present study results indicate that the prevalence of mood, anxiety, and personality disorders among first-generation immigrants is significantly less than that of native-born Americans. This is noteworthy given that immigrants were found to report lower income and education levels, both of which have been identified as risk factors for psychiatric morbidity among individuals in the general population (Lorant et al., 2003). We also found that, independent of what region of the world they came from, immigrants were less likely to be diagnosed with a mood/anxiety or personality disorder than native-born Americans. The disparities in prevalence between immigrants from these major regions of the world and native-born Americans were more pronounced for mood/anxiety disorders than for personality disorders. Asian immigrants were the least likely to be diagnosed with a personality disorder while African immigrants were least likely to be diagnosed with a mood or anxiety disorder.

With respect to second-generation immigrants, who also tend to be disproportionately disadvantaged in terms of income and education, the protective effect of immigrant status was somewhat attenuated. While second-generation immigrants were still less likely to be diagnosed with a mood/anxiety disorder, the effect wanes for general anxiety, dysthymia, and social phobia. With the exception of antisocial and avoidant personality disorders, the effect is also less pronounced for personality disorders. In fact, second-generation immigrants were more likely to be diagnosed with borderline personality disorder than native-born Americans. While the effects were small, this finding is noteworthy given the role of adverse life events in the etiology of borderline personality disorder in the general population (Leichsenring et al., 2011). This finding also raises questions as to intergenerational changes in exposure to childhood adversity and trauma among immigrant families.

This study examines several protective factors and, of these, immigrant status seems to be the constant determinant of mental health differences between first and second-generation immigrants and native-born respondents. It is of interest to note, however, that the effects of immigrant status seem to diminish over time. As such, present study results align with previous investigations that indicate a similar pattern and suggest a link between exposure to U.S. society and mental illness (e.g., Harker, 2001; Alegría et al., 2007; Takeuchi et al.,

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2007; Williams et al., 2007). It should also be noted that the relationship between immigrant status and mental health appears to be distinct among racial/ethnic minorities and non-minorities. Specifically, we found that, while both racial/ethnic minority and nonminority first-generation immigrants were found to be less likely to have a mood/anxiety disorder, the magnitude of this relationship was greater among minority immigrants than among non-minorities (i.e., non-Hispanic whites). Along the same lines, immigrant status was protective for mood/anxiety disorders among minority second-generation immigrants, but no relationship was observed among non-minority second-generation immigrants. Moreover, the relationship between immigrant status and personality disorders held for minority first and second-generation immigrants, but no link was observed among non-minority first or second-generation immigrants.

That there are generational and racial/ethnic differences provides a clear indication that post-migration factors do matter. Notwithstanding well-documented pre-migration stressors, the resettlement process seems to play a bigger role in informing mental health outcomes. Studies that have examined the resettlement phase point to adjustment stressors as potential triggers for mental illness (Aycan and Berry, 1996; Porter and Haslam, 2005). The loss of lifelong relationships, altered family roles, and economic incongruencies prior to and after migration are posited to adversely affect mental well-being. Additionally, the differential findings among minority and non-minority immigrants raise questions about segmented assimilation (Portes and Zhou, 1993) and the possible impact of racism and discrimination on the health of immigrants (Viruell-Fuentes, 2007; Chung et al., 2008; Pascoe and Richman, 2009). These findings also may be interpreted in the context of research examining the disruption of family and social support networks, particularly studies pointing to the importance of these factors among immigrants from non-western collectivist cultures (Garcia Coll et al., 1996; Oppedal et al., 2004; Alegria et al., 2007). A more robust body of research that examines the dynamics of resettlement in relation to mental health is needed to disentangle the etiology of generational and discriminatory factors.

4.1. Study limitations

The current study results should be interpreted within the context of several limitations. One limitation is that the data are less than ideal with respect to temporal ordering. Although we have follow-up data, the NESARC is not a true longitudinal investigation. We rely on retrospective lifetime measures and are unable to clarify the causal mechanisms that are associated with mental health among immigrants. Further, we do not know what the longer-term prevalence of mental disorders will be. This will require life-course study designs. Although the NESARC is a nationally representative sample, it is uncertain how the association between immigrant status and mental health would be similar or different if correctional or clinical samples were sampled. It seems likely that this would bias study findings and the difference between immigrants and native-born Americans would be magnified. We also don't know if findings from the present investigation would replicate for immigrants to other nations. An additional limitation is that the NESARC did not include information on schizophrenia or other psychotic disorders. This omission is noteworthy given that such disorders have been found to be positively related to first- and second-generation immigrant status in previous research (Cantor-Graae and Selten, 2005; Bourque

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et al., 2011). The NESARC also is lacking variables tapping the reasons why individuals elected to immigrate to the United States (e.g., economic scarcity, professional re-location). This is noteworthy as, for example, the mental health of someone who immigrated to the United States due to economic scarcity may be different than someone who was recruited for a high-paying professional position. Finally, although respondents had the option of completing the NESARC in one of several different languages, it is possible that potential respondents who did not speak one of these languages were excluded. It is possible that these "linguistically isolated" individuals may be at greater risk of mental problems and thus the results may underestimate the true prevalence of morbidity among immigrants. Future studies on immigration and mental health will benefit from addressing these study features.

4.2. Conclusions

Findings of the present study provide evidence in support of a multigenerational immigrant paradox for mood, anxiety, and personality disorders. First-generation immigrants and, to a lesser extent, second-generation immigrants were found to be significantly less likely to be diagnosed with a mood, anxiety, or personality disorder compared to native-born Americans. Notably, however, these effects were distinct among racial/ethnic minority and non-minority immigrants. These effects held for immigrants from major regions across the globe, including Africa, Asia, Europe, and Latin America. In all, while caution should be exercised in interpreting study findings due to the potential effects of selection and treatment bias, results suggest that immigrant status and generation are important factors related to mental health and wellbeing among first and second-generation immigrants in the United States.

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Highlights

- First-generation immigrants at lower risk for mood, anxiety, or personality disorders
- Similar results observed for first-generation immigrants from major world regions
- Protective effects for morbidity more robust among racial/ethnic minority immigrants
- Protective effect of immigrant status was somewhat attenuated by second-generation

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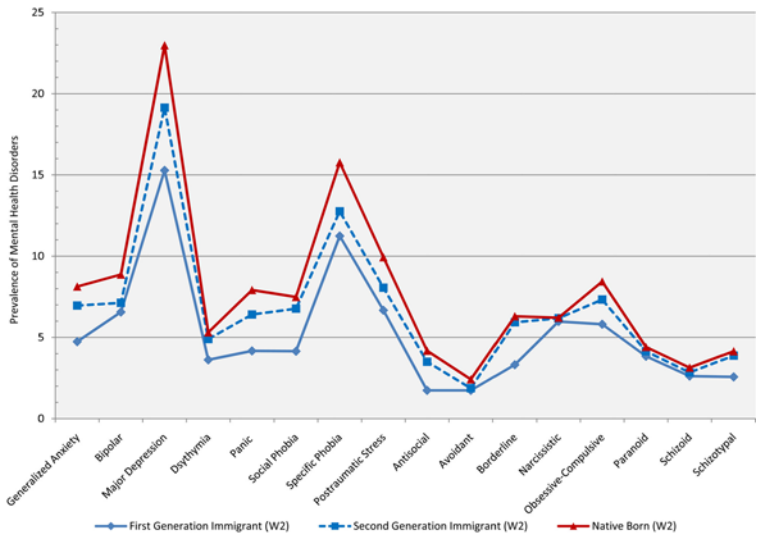

Figure 1.
Prevalence of lifetime mood/anxiety and personality disorders among native-born and first- and second-generation immigrant adults in the United States

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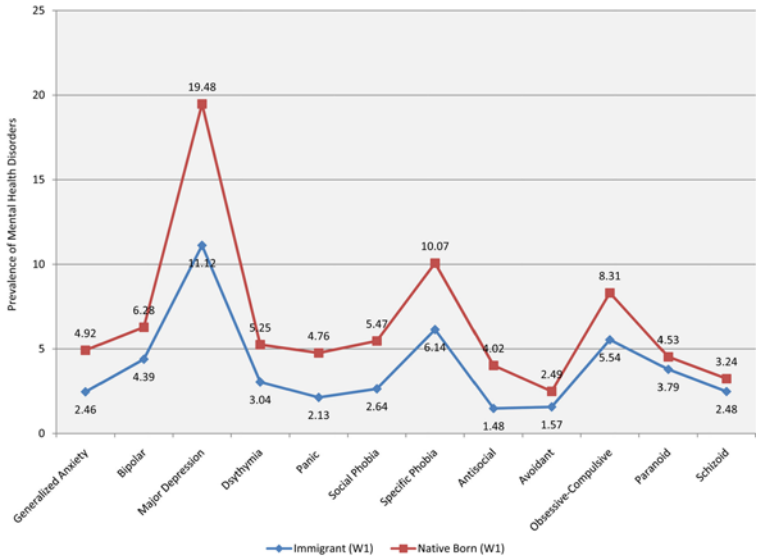

Figure 2.
Sensitivity analysis of the prevalence of lifetime mood/anxiety and personality among native-born and immigrant adults (Wave I).

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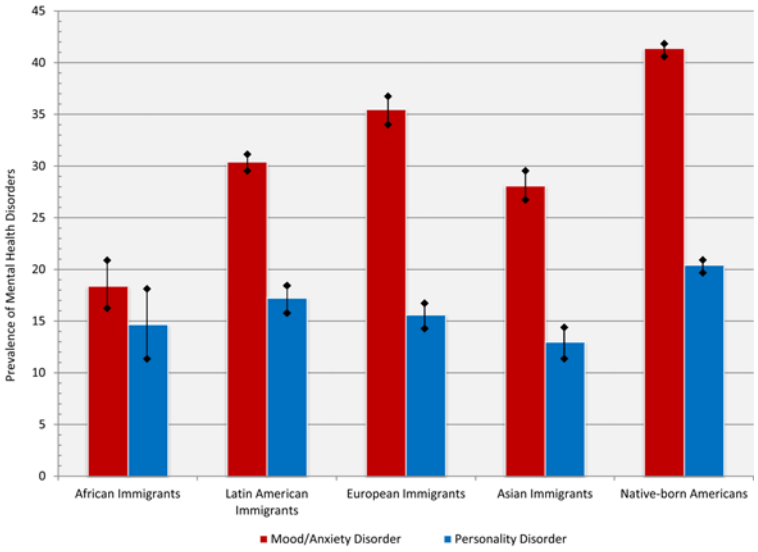

Figure 3.
Prevalence of lifetime mood/anxiety and personality disorders among native-born and immigrant adults by immigrant region of origin

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Table 1

Sociodemographic characteristics of native-born and first-generation immigrant adults in the United States

	Immigrated to the United States				Unadjusted		Adjusted	
	No (*n* = 24,461; 84.10%)		Yes (*n* = 5363; 15.90%)					
	%	95% CI	%	95% CI	OR	(95% CI)	OR	(95% CI)
Sociodemographic Factors								
Age								
18–34 years	24.43	(23.9–24.9)	29.38	(28.9–29.9)	1.00		1.00	
35–49 years	31.66	(31.2–32.1)	34.92	(34.4–35.4)	**0.92**	**(0.88–0.95)**	1.00	(0.95–1.05)
50–64 years	25.45	(25.0–25.9)	22.26	(21.9–22.6)	**0.73**	**(0.70–0.76)**	0.98	(0.92–1.04)
65+	18.46	(18.1–18.8)	13.44	(13.1–13.8)	**0.60**	**(0.58–0.63)**	0.98	(0.90–1.07)
Gender								
Female	52.33	(51.9–52.7)	50.48	(50.0–51.0)	1.00		1.00	
Male	47.67	(47.3–48.1)	49.52	(49.0–50.0)	**1.08**	**(1.05–1.10)**	0.99	(0.96–1.03)
Race/Ethnicity								
Non-Hispanic White	80.97	(80.3–81.6)	22.21	(21.8–22.8)	1.00		1.00	
Non-Hispanic Black	12.40	(11.9–12.9)	7.78	(7.5–8.0)	**2.28**	**(2.13–2.43)**	**2.39**	**(2.24–2.55)**
Hispanic	3.42	(3.3–3.6)	46.18	(45.6–46.9)	**48.95**	**(46.2–51.8)**	**45.53**	**(43.0–48.2)**
Other	3.21	(2.9–3.5)	23.74	(23.2–24.3)	**26.86**	**(24.3–29.6)**	**26.09**	**(23.6–28.8)**
Household Income								
< $20,000	18.77	(18.4–19.1)	22.65	(22.2–23.1)	**1.58**	**(1.51–1.64)**	**1.12**	**(1.02–1.22)**
$20,000–$34,999	18.03	(17.7–18.4)	22.23	(21.8–22.7)	**1.61**	**(1.55–1.67)**	**1.21**	**(1.14–1.29)**
$35,000–69,999	32.89	(32.5–33.3)	31.91	(31.4–32.4)	**1.27**	**(1.23–1.31)**	**1.10**	**(1.05–1.16)**
> $70,000	30.30	(29.9–30.6)	23.20	(22.8–23.6)	1.00		1.00	
Education Level								
Less than H.S.	11.16	(10.8–11.5)	29.25	(28.7–29.8)	**2.86**	**(2.73–3.00)**	**1.29**	**(1.20–1.39)**
H.S. Graduate	28.89	(28.4–29.4)	20.95	(20.6–21.3)	**0.79**	**(0.76–0.82)**	**0.60**	**(0.57–0.64)**
Some College	22.57	(22.2–22.9)	15.61	(15.4–15.9)	**0.75**	**(0.73–0.78)**	**0.59**	**(0.56–0.63)**
Completed AA, BA, or Technical Degree	37.37	(36.9–37.8)	34.20	(33.5–34.8)	1.00		1.00	

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	Immigrated to the United States				Unadjusted		Adjusted	
	No (n = 24,461; 84.10%)		Yes (n = 5363; 15.90%)					
	%	95% CI	%	95% CI	OR	(95% CI)	OR	(95% CI)
Marital Status								
Married/ Cohabitating	63.57	(63.1–64.0)	629.74	(69.4–70.1)	1.00		1.00	
Separated/Divorced	12.46	(12.2–12.7)	8.79	(8.5–9.1)	**0.64**	**(0.61–0.67)**	**0.60**	**(0.56–0.65)**
Widowed	6.90	(6.7–7.1)	5.33	(5.2–5.5)	**0.70**	**(0.67–0.74)**	**0.82**	**(0.75–0.90)**
Never Married	17.08	(16.7–17.5)	16.13	(15.8–16.4)	**0.86**	**(0.83–0.89)**	**0.76**	**(0.72–0.80)**
Region of U.S.A.								
Northeast	18.67	(18.2–19.2)	15.15	(14.7–15.6)	**0.79**	**(0.75–0.83)**	**0.85**	**(0.78–0.93)**
Midwest	17.65	(17.3–18.0)	22.31	(21.7–22.9)	**1.23**	**(1.18–1.28)**	**1.09**	**(1.03–1.16)**
South	38.76	(38.2–39.3)	36.96	(36.5–37.4)	**0.93**	**(0.90–0.96)**	**0.91**	**(0.86–0.96)**
West	24.93	(24.5–25.4)	25.57	(25.1–26.1)	1.00		1.00	
Urbanicity								
Urban	67.50	(66.9–68.1)	66.71	(66.1–67.3)	**1.04**	**(1.00–1.07)**	1.05	(0.99–1.11)
Rural	32.50	(31.9–33.1)	33.29	(32.6–33.9)	1.00		1.00	

Note: Adjusted odds ratios adjusted for age, gender, race/ethnicity, household income, education level, marital status, region of the United States, and urbanicity. Odds ratios in bold are significant at p <. 05 or lower

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Table 2

Odds ratios for psychiatric disorders among first- and second-generation immigrant adults in the U.S.A.

	Immigrated to the United States				Second Generation Immigrant			
	OR	(95% CI)	AOR	(95% CI)	OR	(95% CI)	AOR	(95% CI)
Mood/Anxiety Disorders								
Any Mood/Anxiety Disorder	**0.60**	**(0.58–0.62)**	**0.70**	**(0.67–0.73)**	**0.82**	**(0.79–0.85)**	**0.92**	**(0.88–0.96)**
Generalized Anxiety	**0.55**	**(0.52–0.57)**	**0.74**	**(0.70–0.79)**	**0.83**	**(0.78–0.88)**	1.00	(0.93–1.06)
Bipolar Disorder	**0.69**	**(0.66–0.73)**	**0.83**	**(0.77–0.89)**	**0.76**	**(0.73–0.80)**	**0.86**	**(0.81–0.90)**
Major Depression	**0.58**	**(0.55–0.61)**	**0.73**	**(0.68–0.77)**	**0.77**	**(0.73–0.80)**	**0.86**	**(0.82–0.90)**
Dysthymia	**0.66**	**(0.61–0.71)**	0.87	(0.76–1.00)	**0.91**	**(0.85–0.97)**	1.07	(0.99–1.16)
Panic disorder	**0.49**	**(0.46–0.51)**	**0.62**	**(0.58–0.67)**	**0.77**	**(0.73–0.82)**	**0.88**	**(0.83–0.93)**
Social phobia	**0.53**	**(0.47–0.58)**	**0.69**	**(0.60–0.80)**	**0.89**	**(0.82–0.96)**	1.04	(0.96–1.13)
Specific Phobia	**0.65**	**(0.62–0.68)**	**0.73**	**(0.69–0.78)**	**0.75**	**(0.72–0.79)**	**0.84**	**(0.80–0.88)**
Posttraumatic Stress	**0.62**	**(0.60–0.65)**	**0.66**	**(0.62–0.71)**	**0.77**	**(0.72–0.82)**	**0.84**	**(0.79–0.91)**
Personality Disorders								
Any Personality Disorder	**0.75**	**(0.73–0.77)**	**0.78**	**(0.74–0.82)**	**0.90**	**(0.87–0.94)**	0.97	(0.92–1.01)
Antisocial	**0.39**	**(0.32–0.48)**	**0.51**	**(0.37–0.71)**	**0.81**	**(0.74–0.89)**	**0.86**	**(0.77–0.96)**
Avoidant	**0.68**	**(0.62–0.75)**	**0.74**	**(0.59–0.91)**	**0.75**	**(0.66–0.84)**	**0.82**	**(0.71–0.95)**
Borderline	**0.50**	**(0.48–0.53)**	**0.64**	**(0.58–0.71)**	**0.93**	**(0.87–0.99)**	**1.11**	**(1.03–1.20)**
Narcissistic	0.96	(0.90–1.02)	1.00	(0.92–1.08)	0.99	(0.93–1.06)	1.06	(0.99–1.14)
Obsessive-Compulsive	**0.65**	**(0.61–0.69)**	0.92	(0.82–1.03)	**0.84**	**(0.79–0.89)**	0.96	(0.89–1.02)
Paranoid	**0.86**	**(0.77–0.95)**	0.88	(0.73–1.06)	0.94	(0.85–1.03)	1.01	(0.90–1.13)
Schizoid	**0.82**	**(0.76–0.88)**	0.90	(0.80–1.01)	**0.90**	**(0.81–1.00)**	0.99	(0.89–1.12)
Schizotypal	**0.60**	**(0.56–0.64)**	**0.69**	**(0.62–0.76)**	0.93	(0.84–1.02)	1.10	(1.00–1.21)

Note: Adjusted odds ratios adjusted for age, gender, race/ethnicity, household income, education level, marital status, region of the United States, urbanicity, and lifetime diagnosis of alcohol and any drug abuse or dependence. Odds ratios in bold are significant at $p < .05$ or lower.

rethinking crime and immigration

by robert j. sampson

The summer of 2007 witnessed a perfect storm of controversy over immigration to the United States. After building for months with angry debate, a widely touted immigration reform bill supported by President George W. Bush and many leaders in Congress failed decisively. Recriminations soon followed across the political spectrum.

Just when it seemed media attention couldn't be greater, a human tragedy unfolded with the horrifying execution-style murders of three teenagers in Newark, N.J., attributed by authorities to illegal aliens.

Presidential candidate Rep. Tom Tancredo (R–Colorado) descended on Newark to blame city leaders for encouraging illegal immigration, while Newt Gingrich declared the "war at home" against illegal immigrants was more deadly than the battlefields of Iraq. National headlines and outrage reached a feverish pitch, with Newark offering politicians a potent new symbol and a brown face to replace the infamous Willie Horton, who committed armed robbery and rape while on a weekend furlough from his life sentence to a Massachusetts prison. Another presidential candidate, former Tennessee senator Fred Thompson, seemed to capture the mood of the times at the Prescott Bush Awards Dinner: "Twelve million illegal immigrants later, we are now living in a nation that is beset by people who are suicidal maniacs and want to kill countless innocent men, women, and children around the world."

Now imagine a nearly opposite, fact-based scenario. Consider that immigration—even if illegal—is associated with *lower* crime rates in most disadvantaged urban neighborhoods. Or that increasing immigration tracks with the broad *reduction* in crime the United States has witnessed since the 1990s.

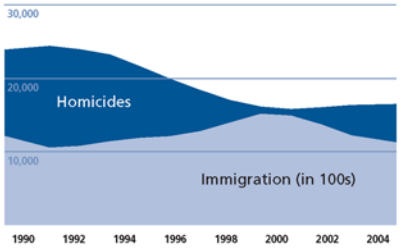
Well before the 2007 Summer of Discontent over immigration, I proposed we take such ideas seriously. Based on hindsight I shouldn't have been surprised by the intense reaction to what I thought at the time was a rather logical reflection. From the right came loud guffaws, expletive-filled insults, angry web postings, and not-so-thinly veiled threats. But the left wasn't so happy either, because my argument assumes racial and ethnic differences in crime not tidily attributable to material deprivation or discrimination—the canonical explanations.

Although Americans hold polarizing and conflicting views about its value, immigration is a major social force that will continue for some time. It thus pays to reconsider the role of immigration in shaping crime, cities, culture, and societal change writ large, especially in this era of social anxiety and vitriolic claims about immigration's reign of terror.

some facts

Consider first the "Latino Paradox." Hispanic Americans do better on a wide range of social indicators—including propensity to violence—than one would expect given their socioeconomic disadvantages. To assess this paradox in more depth, my colleagues and I examined violent acts committed by nearly 3,000 males and females in Chicago ranging in age from 8 to 25 between 1995 and 2003. The study selected whites, blacks, and Hispanics (primarily Mexican-Americans) from 180 neighborhoods ranging from highly segregated to very integrated. We also analyzed data from police records, the U.S. Census, and a separate survey of more than 8,000

Immigration flows and homicide trends: U.S. total, 1990-2004 (three-year averages)



Chicago residents who were asked about the characteristics of their neighborhoods.

Notably, we found a significantly lower rate of violence among Mexican-Americans compared to blacks and whites. A major reason is that more than a quarter of those of Mexican descent were born abroad and more than half lived in neighborhoods where the majority of residents were also Mexican. In particular, first-generation immigrants (those born outside the United States) were 45 percent less likely to commit violence than third-generation Americans, adjusting for individual, family, and neighborhood background. Second-generation immigrants were 22 percent less likely to commit violence than the third generation. This pattern held true for non-Hispanic whites and blacks as well. Our study further showed living in a neighborhood of concentrated immigration was directly associated with lower violence (again, after taking into account a host of correlated factors, including poverty and an individual's immigrant status). Immigration thus appeared "protective" against violence.

Consider next the implications of these findings when set against the backdrop of one of the most profound social changes to visit the United States in recent decades. Foreign immigration to the United States rose sharply in the 1990s, especially from Mexico and especially to immigrant enclaves in large cities. Overall, the foreign-born population increased by more than 50 percent in 10 years, to 31 million in 2000. A report by the Pew Hispanic Center found immigration grew most significantly in the mid-1990s and hit its peak at the end of the decade, when the national homicide rate plunged to levels not seen since the 1960s. Immigrant flows have receded since 2001 but remain high, while the national homicide rate leveled off and seems now to be creeping up. Both trends are compared over time at left.

The pattern upends popular stereotypes. Among the public, policy makers, and even many academics, a common expectation is that the concentration of immigrants and the influx of foreigners drive up crime rates because of the assumed propen-

Contexts, Vol. 7, No. 1, pp. 28–33. ISSN 1536-5042, electronic ISSN 1537-6052. © 2008 American Sociological Association. All rights reserved. For permission to photocopy or reproduce see http://www.ucpressjournals.com/reprintinfo.asp. DOI: 10.1525/ctx.2008.7.1.28.

sities of these groups to commit crimes and settle in poor, presumably disorganized communities. This belief is so pervasive that in our Chicago study the concentration of Latinos in a neighborhood strongly predicted perceptions of disorder no matter the actual amount of disorder or rate of reported crimes. And yet immigrants appear in general to be less violent than people born in America, particularly when they live in neighborhoods with high numbers of other immigrants.

We are thus witnessing a different pattern from early 20th century America, when growth in immigration from Europe, along with ethnic diversity more generally, was linked with increasing crime and formed a building block for what became known as "social disorganization" theory. New York today is a leading magnet for immigration, yet it has for a decade ranked as one of America's safest cities. Crime in Los Angeles dropped considerably in the late 1990s (45 percent overall) as did other Hispanic influenced cities such as San Jose, Dallas, and Phoenix. The same can be said for cities smack on the border like El Paso and San Diego, which have long ranked as low-crime areas. Cities of concentrated immigration are some of the safest places around.

counterpoint

There are criticisms of these arguments, of course. To begin, the previous figure juxtaposes two trends and nothing more—correlation doesn't equal causation. But it does demonstrate the trends are opposite of what's commonly assumed, which is surely not irrelevant to the many, and strongly causal, claims that immigration increases crime. Descriptive facts are at the heart of sound social science, a first step in any causal inquiry.

Perhaps a bigger concern is that we need to distinguish illegal from legal immigration and focus on the many illegal aliens who allegedly are accounting for crime waves across the country—the "Newark phenomenon." By one argument, because of deportation risk illegal immigrants are afraid to report crimes against them to the police, resulting in artificially low official estimates in the Hispanic community. But no evidence exists that reporting biases seriously affect estimates of the homicide victimization rate—unlike other crimes there is a body. At the national level, then, the homicides committed by illegal aliens in the United States are reflected in the data just like for everyone else. The bottom line is that as immigrants poured into the country, homicides plummeted. One could claim crime would decrease faster absent immigration inflows, but that's a different argument and concedes my basic point.

There is also little disputing that in areas and times of high legal immigration we find accompanying surges of illegal entrants. It would be odd indeed if illegal aliens descended on areas with no other immigrants or where they had no pre-existing networks. And so it is that areas of concentrated immigration are magnets for illegal concentration. Because crime tends

U.S. Rep. Tom Tancredo speaks during a press conference on immigration reform at the U.S. Capitol March 30, 2006. Photo by Win McNamee Getty Images

to be negatively associated with undifferentiated immigration measures, it follows that we can disconfirm the idea that increasing illegal immigration is associated with increasing crime.

Furthermore, our Chicago study did include both legal and illegal immigrants. I would estimate the illegal status at roughly a quarter—but in any case no group was excluded from the analysis. The other important point is that the violence estimates were based on confidential self-reports and not police statistics or other official sources of crime. Therefore, police arrest biases or undercounts can't explain the fact that first generation immigrants self-report lower violence than the second generation, which in turn reports less than the third generation.

So let us proceed on the assumption of a substantial negative association across individuals, places, and time with respect to immigration and violence. What potential mechanisms might explain the connections and are they causal? Thinking about these questions requires attention be paid to confounding factors and competing explanations.

Social scientists worry a lot about selection bias because individuals differ in preferences and can, within means, select their environments. It has been widely hypothesized that immigrants, and Mexicans in particular, selectively migrate to the United States on characteristics that predispose them to low crime, such as motivation to work, ambition, and a desire not to be deported. Immigrants may also come from cultures where violence isn't rewarded as a strategy for establishing reputation (to which I return below).

This scenario is undoubtedly the case and central to the argument—social selection is a causal mechanism. Namely, to the extent that more people predisposed to lower crime immigrate to the United States (we now have some 35 million people of foreign-born status), they will sharply increase the denominator of the crime rate while rarely appearing in the numerator. And in the neighborhoods of U.S. cities with high concentrations of immigrants, one would expect on selection grounds alone to find lower crime rates. Selection thus favors

the argument that immigration may be causally linked to lower crime.

Another concern of social scientists is common sources of causation, or "competing" explanations. One candidate is economic trends. After all, potential immigrants respond to incentives and presumably choose to relocate when times are better in their destinations. Although a legitimate concern, economics can't easily explain the story. Depending on the measure, economic trends aren't isomorphic with either immigration or crime at either the beginning or end of the time series. Real wages were declining and inequality increasing in the 1990s by most accounts, which should have produced increases in crime by the logic of relative deprivation theory, which says that income gaps, not absolute poverty, are what matters. Broad economic indicators like stock market values did skyrocket but collapsed sharply while immigration didn't.

Scholars in criminology have long searched for a sturdy link between national economic trends and violence, to little avail. The patterns just don't match up well, and often they're in the opposite direction of deprivation-based expectations. The best example is the 1960s when the economy markedly improved yet crime shot up. Don't forget, too, the concentrated immigration and crime link remains when controlling for economic indicators.

Finally, the "Latino Paradox" in itself should put to rest the idea that economics is the go-to answer: Immigrant Latinos are poor and disadvantaged but at low risk for crime. Poor immigrant neighborhoods and immigrant-tinged cities like El Paso have similarly lower crime than their economic profile would suggest.

Competing explanations also can't explain the Chicago findings. Immigrant youths committed less violence than natives after adjustment for a rich set of individual, family, and neighborhood confounders. Moreover, there's an influence of immigrant concentration beyond the effects of individual immigrant status and other individual factors, and beyond neighborhood socioeconomic status and legal cynicism—previously shown to significantly predict violence. We estimated male violence by age for three types of neighborhoods (below):

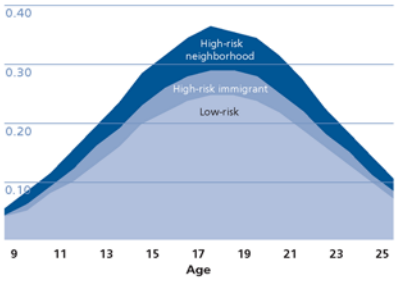
- "Low-risk," where a very high percentage of people work in professional and managerial occupations (90th percentile), few people hold cynical attitudes about the law and morality (10th percentile), and there are no immigrants;
- "High-risk," where professional/managerial jobs are scarce, cynicism is pervasive, and there are also no immigrants;
- "High-risk, immigrant neighborhoods," defined by similarly low shares of professional/managerial workers and high legal cynicism, but where about one-half of the people are immigrants.

> Cities of concentrated immigration are some of the safest places around.

The estimated probability an average male living in a high-risk neighborhood without immigrants will engage in violence is almost 25 percent higher than in the high-risk, immigrant neighborhood, a pattern again suggesting the protective, rather than crime-generating, influence of immigrant concentration.

Finally, we examined violence in Chicago neighborhoods by a foreign-born diversity index capturing 100 countries of birth from around the world (page 32). In both high- and low-poverty communities, foreign-born diversity is clearly and strongly linked to lower violence. Concentrated poverty predicts more violence (note the high poverty areas above the prediction line) but violence is lower as diversity goes up for low- and high-poverty neighborhoods alike. Interestingly, the link between lower violence and diversity is strongest in the most disadvantaged neighborhoods.

Estimated probability of violence by third-generation males in Chicago neighborhoods

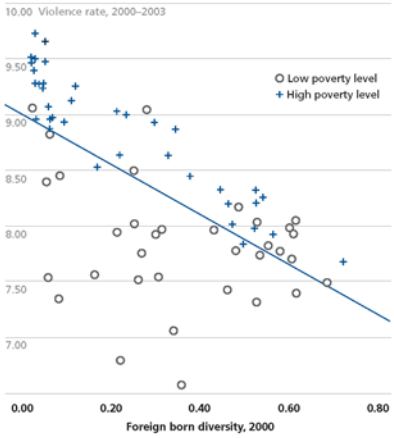


crime declines among non-hispanics

A puzzle apparently remains in how immigration explains the crime decline among whites and blacks in the 1990s. One agitated critic, for example, charged that my thesis implies that for every Mexican entering America a black person would have to commit fewer crimes. But immigration isn't the only cause of the crime decline. There are many causes of crime—that declines ensued for blacks and whites doesn't in itself invalidate the immigration argument.

This critique also exposes a misconception about immigrant diversity. Immigration isn't just about Mexicans, it's about the influx of a wide range of different groups. The previous figure, for example, represents 100 countries, a conservative template for many places. In cities such as Los Angeles and New York, immigrant flows are erasing simple black-white-brown scenarios and replacing them with a complex mixture

Violence and diversity in Chicago neighborhoods



of immigrant diversity.

Even the traditionally black-white city of Chicago reflects evidence of immigration's broad reach. When we looked at whites and blacks we still found surprising variation in generational status, with immigration protective for all racial/ethnic groups except Puerto Ricans/other Latinos. In fact, controlling for immigrant generation reduced the gap between African Americans and whites by 14 percent, implying one reason whites have lower levels of violence than African Americans is that whites are more likely to be recent immigrants. The pattern of immigrant generational status and lower crime is thus not just restricted to Latinos, and it extends to helping explain white-black differences as well.

Added to this is substantial non-Latino immigration into the United States from around the world, including Russia, Poland, India, and the Caribbean, to name just a few countries. Black and white populations are increasingly characterized by immigrants (Poles and Russians among whites in Chicago, for example, and Caribbeans and West Africans among blacks in New York). According to Census 2000, the Chicago area has more than 130,000 Polish immigrants, so we aren't talking about trivial numbers.

Perhaps more important, focusing on the "what about whites and blacks" question misses the non-selection-based component of a broader immigration argument. We're so used to thinking about immigrant adaptation (or assimilation) to the host society we've failed to fully appreciate how immigrants themselves shape the host society. Take economic revitalization and urban growth. A growing consensus argues immigration revitalizes cities around the country. Many decaying inner-city areas gained population in the 1990s and became more vital, in large part through immigration. One of the most thriving scenes of economic activity in the entire Chicagoland area, for example, second only to the famed "Miracle Mile" of Michigan Avenue, is the 26th Street corridor in Little Village. A recent analysis of New York City showed that for the first time ever, blacks' incomes in Queens have surpassed whites', with the surge in the black middle class driven largely by the successes of black immigrants from the West Indies. Segregation and the concentration of poverty have also decreased in many cities for the first time in decades.

Such changes are a major social force and immigrants aren't the only beneficiaries—native born blacks, whites, and other traditional groups in the United States have been exposed to the gains associated with lower crime (decreases in segregation, decreases in concentrated poverty, increases in the economic and civic health of central cities, to name just a few). There are many examples of inner-city neighborhoods rejuvenated by immigration that go well beyond Queens and the Lower West Side of Chicago. From Bushwick in Brooklyn to Miami, and from large swaths of south central Los Angeles to the rural South, immigration is reshaping America. It follows that the "externalities" associated with immigration are multiple in character and constitute a plausible mechanism explaining some of the variation in crime rates of all groups in the host society.

There are important implications for this line of argument. If it is correct, then simply adjusting for things like economic revitalization, urban change, and other seemingly confounding explanations is illegitimate from a causal explanation standpoint because they would instead be mediators or conduits of immigration effects—themselves part of the pathway of explanation. Put differently, to the extent immigration is causally bound up with major social changes that in turn are part of the explanatory process of reduced crime, estimating only the net effects of immigration will give us the wrong answer.

> The pattern of immigrant generational status and lower crime isn't just restricted to Latinos, it helps explain white-black differences, too.

cultural penetration and societal renewal

A related cultural implication, while speculative and perhaps provocative, is worth considering. If immigration leads to

the penetration into America of diverse and formerly external cultures, then this diffusion may contribute to less crime if these cultures don't carry the same meanings with respect to violence and crime.

It's no secret the United States has long been a high-violence society, with many scholars positing a subculture or code of the streets as its main cause. In one influential version, shared expectations for demanding respect and "saving face" lead participants in the "street culture" of poor inner cities to react violently to perceived slights, insults, and otherwise petty encounters that make up the rounds of daily life. But according to the logic of this theory, if one doesn't share the cultural attribution or perceived meaning of the event, violence is less likely. Outsiders to the culture, that is, are unlikely to be caught in the vicious cycles of interaction (and reaction) that promote violence.

The massive penetration of immigrant (particularly, but not only, Mexican) populations throughout the United States, including rural areas and the South, can properly be thought of as a diffusion-like process. One possible result is that over time American culture is being diluted. Some of the most voracious critiques of immigration have embraced this very line of argument. Samuel Huntington, in one well-known example, claims the very essence of American identity is at stake because of increasing diversity and immigration, especially from Mexico. He may well be right, but the diagnosis might not be so bad if a frontier mentality that endorses and perpetuates codes of violence is a defining feature of American culture.

A profound irony in the immigration debate concedes another point to Huntington. If immigration can be said to have brought violence to America, it most likely came with (white) Irish and Scottish immigrants whose cultural traditions emphasizing honor and respect were defended with violent means when they settled in the South in the 1700s and 1800s. Robert Nisbett and Dov Cohen have presented provocative evidence in favor of this thesis, emphasizing cultural transmission in the form of Scotch-Irish immigrants, descendants of Celtic herdsman, who developed rural herding communities in the frontier South. In areas with little state power to command compliance with the law, a tradition of frontier justice carried over from rural Europe took hold, with a heavy emphasis on retaliation and the use of violence to settle disputes, represented most clearly in the culture of dueling.

In today's society, then, I would hypothesize that immigration and the increasing cultural diversity that accompanies it generate the sort of conflicts of culture that lead not to increased crime but nearly the opposite. In other words, selective immigration in the current era may be leading to the greater visibility of competing non-violent mores that affect not just immigrant communities but diffuse and concatenate through social interactions to tamp down violent conflict in general. Recent findings showing the spread of immigration to all parts of America, including rural areas of the Midwest and South, give credence to this argument. The Willie Hortinization of illegal aliens notwithstanding, diversity and cultural conflict wrought by immigration may well prove healthy, rather than destructive, as traditionally believed.

> We're so used to thinking about immigrant adaptation we've failed to fully appreciate how immigrants themselves shape the host society.

recommended resources

Richard Nisbett and Dov Cohen. *Culture of Honor: The Psychology of Violence in the South* (Westview, 1996). A fascinating take on the cultural roots of violence in the United States, including the culture of honor posited to afflict the South disproportionately and traced to European immigration.

Eyal Press. "Do Immigrants Make Us Safer?" *New York Times Magazine* December 3, 2006. A *New York Times* writer considers the questions raised in this article, taking to the streets of Chicago.

Rubén G. Rumbaut and Walter A. Ewing. "The Myth of Immigrant Criminality and the Paradox of Assimilation: Incarceration Rates among Native and Foreign-Born Men," (Immigration Policy Center, 2007). A recent synthesis of the empirical facts on immigration and crime, with a special focus on incarceration.

Thorsten Sellin. *Culture Conflict and Crime* (Social Science Research Council, 1938). Widely considered the classical account of immigration, culture, and crime in the early part of the 20th century.

Robert J. Sampson is chair of the sociology department at Harvard University. His research centers on crime, deviance, and stigma; the life course; neighborhood effects; and the social organization of cities.

CATO INSTITUTE BLOG

MARCH 29, 2019 12:02PM

Sanctuary Jurisdictions in Florida Do Not Have Higher Crime Rates

By **Alex Nowrasteh** and **Andrew C. Forrester**

Florida state Senator Joe Gruters (R-Sarasota) introduced a bill (SB 168) earlier this year to ban so-called sanctuary jurisdictions in Florida and require local governments to cooperate fully with Immigration and Customs Enforcement (ICE). A sanctuary jurisdiction is any state or local government that has a policy to comply with fewer than 100 percent of ICE detainers, which are ICE requests for the local government to release an arrested or imprisoned person into ICE custody for deportation. Local and state governments still prosecute illegal immigrants for crimes in sanctuary jurisdictions, but they only turn some illegal immigrants over to ICE and uniformly if they are charged with or convicted of serious crimes.

The complaint over sanctuary jurisdictions is that they result in increased crime, but the limited research on the topic finds no increase in crime in sanctuary jurisdictions relative to non-sanctuary jurisdictions. Regardless, the methods employed in that paper, the potential for sample selection bias, and the poor quality of national crime data have impeded research into how sanctuary jurisdictions impact crime.

Regardless, we decided to do our best in looking at how sanctuary jurisdiction policies affect crime in Florida. According to the Center for

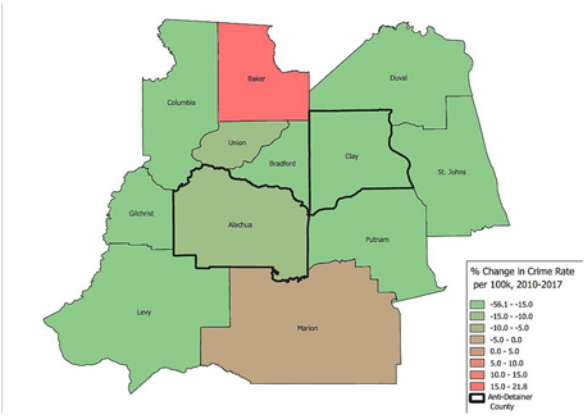
Immigration Studies, Clay and Alachua counties in Florida will not honor ICE detainers without a judicial order or a criminal warrant and their policies were enacted in December 2014 and September 2015, respectively.

To compare whether the adoption of anti-detainer sanctuary policies had an impact on crime in Alachua and Clay Counties, we draw on crime data from the FBI's Uniform Crime Reports (UCR) Return A file. The Return A is the gold standard in crime data in the economics and criminal justice literature. Since these data are provided at the reporting agency level, we aggregate the crime counts up to the county-year level to reflect the extent of geographic coverage for each anti-detainer policy. As a basis for comparison, we identify counties neighboring Alachua and Clay as counterfactual counties using the county adjacency file from the National Bureau for Economic Research. We then compute county crime rates per 100,000 to compare crime rates across counties. For illustrative purposes, we compute an "adjacent counties" counterfactual crime rate as the sum of all crimes in surrounding counties normalized by their combined population.

Figure 1 shows that the crime rates in Clay and Alachua counties have fallen just like in their neighboring counties, except for Baker County, from 2010 through 2017. If sanctuary policies in Clay and Alachua counties affected crime rates, there is no obvious indication of that in Figure 1.

Figure 1

Change in Crime Rates in Sanctuary Counties and Neighboring Counties

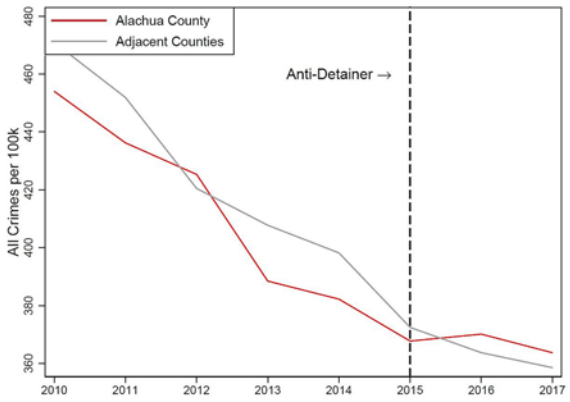


Sources: FBI, Census Bureau TIGER/Line, and Center for Immigration Studies.

Figure 2 displays the crime rates in Alachua County relative to its neighboring counties before after the sanctuary policy was enacted. The crime rates were roughly parallel before the enactment of the sanctuary policy and stayed parallel afterward, meaning that the change in policy likely had no effect on crime rates. The results look nearly identical if trends in property or violent crime rates are compared separately.

Figure 2

Alachua and Neighboring Counties Crime Rates

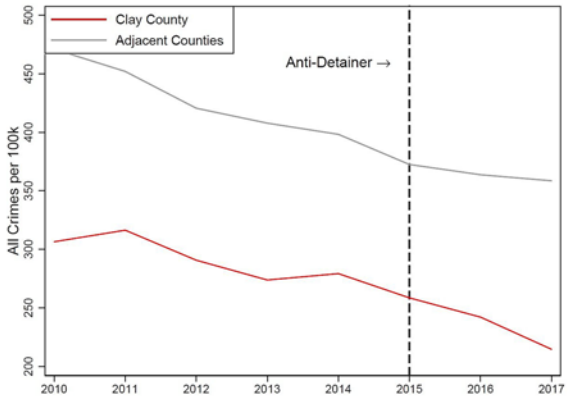


Sources: FBI and Center for Immigration Studies.

Figure 3 displays the crime rates in Clay County relative to neighboring counties. The crime rates were roughly parallel before Clay County enacted its sanctuary policy and remained roughly parallel afterward. Again, it looks as if the enactment of a sanctuary policy in Clay County had no effect on crime. More time after the enactment of the sanctuary policies and more rigorous statistical methods are required to fully analyze these effects for both Clay and Alachua Counties, but Figures 2 and 3 are convincing on their own. The results look nearly identical if trends in property or violent crime rates are compared separately.

Figure 3

Clay and Neighboring Counties Crime Rates



Sources: FBI and Center for Immigration Studies.

The small numbers of non-citizens in Alachua and Clay counties could explain why there was no effect on crime. In 2017, only 5.2 percent of Alachua County's population were non-citizens and 2.5 percent of Clay County's population were non-citizens. In different jurisdictions like Miami-Dade County, where 23.3 percent of the population were non-citizens in 2017, the effect of sanctuary city policies might be different although there is no evidence of that during the brief period when it had a sanctuary policy.

SB 168 was originally paired with a bill that would have mandated E-Verify on the state level. E-Verify is a government electronic eligibility for employment verification system where employers run the identity information of new hires against government databases to see if they are legally able to work. The goal of E-Verify is to exclude illegal immigrants from the workforce. **E-Verify doesn't work well**, but it looks to have **increased crime in Arizona** when that state government mandated it for all

new hire. Although SB 168 will have no effect on crime in Florida, at least the legislature ditched its effort to mandate E-Verify as that may well have increased crime.

Florida currently has only two sanctuary jurisdictions according to the Center for Immigration Studies and is unlikely to have many more in coming years. Furthermore, crime rates in those counties did not rise relative to neighboring counties after they adopted their sanctuary policies. The effect of sanctuary policies on local crime rates is a subject screaming for more research, but the evidence so far shows that sanctuary policies don't affect crime in Florida.

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The immigrant paradox: immigrants are less antisocial than native-born Americans

Michael G. Vaughn,
School of Social Work, College for Public Health and Social Justice, Saint Louis University, Tegeler Hall, 3550 Lindell Boulevard, St. Louis 63103, MO, USA

Christopher P. Salas-Wright,
School of Social Work, University of Texas at Austin, Austin, TX, USA

Matt DeLisi, and
Department of Sociology, Iowa State University, Ames, IA, USA

Brandy R. Maynard
School of Social Work, College for Public Health and Social Justice, Saint Louis University, Tegeler Hall, 3550 Lindell Boulevard, St. Louis 63103, MO, USA

Michael G. Vaughn: mvaughn9@slu.edu

Abstract

Purpose—Although recent research on crime and violence among immigrants suggests a paradox—where immigrants are more socially disadvantaged yet less likely to commit crime—previous research is limited by issues of generalizability and assessment of the full depth of antisocial behavior.

Methods—We surmount these limitations using data from waves I and II of the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) and compare immigrants (N = 7,320) from Asia, Africa, Europe, and Latin America to native-born Americans (N = 34,622) with respect to violent and nonviolent forms of antisocial behavior.

Results—After controlling for an extensive array of confounds, results indicate that immigrants are significantly less antisocial despite being more likely to have lower levels of income, less education, and reside in urban areas. These findings hold for immigrants from major regions of the world including Africa, Asia, Europe, and Latin America.

Conclusions—This study confirms and extends prior research on crime and antisocial behavior, but suggests that it is premature however to think of immigrants as a policy intervention for treating high crime areas.

Keywords

Immigrant; Crime; Immigration; Antisocial behavior; Race and ethnicity

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Correspondence to: Michael G. Vaughn, mvaughn9@slu.edu.
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Introduction

Popular culture depictions of immigrants have tended toward characterizing immigrants as criminogenic and dangerous. These depictions are partly fueled by media images of "foreign born" drug lords and mafia kingpins and also by the socioeconomically disadvantaged state of new immigrants [1–4]. While it is true that immigrants to the United States are more socially disadvantaged than most citizens, this does not necessarily mean that they are more likely to resort to crime and violence [5]. This belief, to some extent, is partly nourished by traditions of sociological theorizing on crime that places the sources of crime squarely on the shoulders of inequality, concentrated disadvantage, and neighborhood social disorder [6–12].

Recent research on immigrants in the US, however, casts doubt on the universality of these sources of crime and violence leading to what can be termed the *immigrant paradox*, where immigrants are more socially disadvantaged yet also less likely to commit crime and evince other forms of social pathology [5, 13–16]. The immigrant paradox finding has emerged from studies using various data sources, research designs, and geographic areas.

Prior research

Concerns about immigrants and crime are longstanding. Despite popular assumptions about immigrants, there is relatively little empirical research in support of the notions that immigrants are disproportionately likely to contribute to crime, violence, and correlated social problems [5, 14, 17]. For instance, Hagan and Palloni [18] studied numerous historical sources and found little evidence of an aggregate-level association between immigration and crime. In their study of recent immigrants to Miami, Martinez and Lee revealed that these newcomers had little to do with increased rates of violence [19]. Additional research along new immigrant destinations and border towns indicates a negative relationship between immigrant status and homicide [12, 15, 20]. In addition, these findings seem to hold across different contexts such as traditional and non-traditional residential destinations [21] and school behavioral problems [22].

At the macro level, rapid neighborhood change such as brought about by the influx of immigrants may theoretically erode neighborhood social norms that maintain informal behavioral controls and thus increase the probability of crime; studies have largely refuted these claims [23, 24]. In fact, recent evidence suggests that immigrants can actually lower neighborhood crime rates [13, 23, 25] and cities that experienced the greatest number of immigrants also experienced the largest decreases in robbery and homicide [26]. Moreover, it has been suggested that immigrants may be a major driver in the crime drop in the US [16, 27]. However, in a study of the unintended consequences of US immigration policy, Shihadeh and Barranco [28, 29] found that increased Latino immigration and concentration in US cities resulted in increased black unemployment and increased black violence.

While it appears that immigrants commit less crime, there is also evidence that the intergenerational continuity in the reductions in criminal behavior at the individual level is transitory. In a unique study of migration from Mexico to the United States and increasing exposure across generations on the prevalence of conduct disorder (CD), Breslau and

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colleagues [30] found that CD was lowest in immigrant families relative to the general population, higher in children of Mexican-born parents who were raised in the US, but highest in Mexican–American children of US born parents. This suggests that the mechanisms that serve to protect or decrease the odds of antisocial behaviors are attenuated as immigrants to the US begin to be distanced from their culture of origin and increase their assimilation to the host culture. Alternatively, the underlying propensity for crime and antisocial behavior may become expressed as a result of exposure to unfavorable environmental conditions that might occur due to acculturation processes and associated strains.

There are several explanations for the immigrant paradox [4–7, 16, 17, 31, 32]. One simple explanation is that immigrants have a lot to lose, including deportation, and avoiding law enforcement is an especially good idea when you are in a new and strange land. This is particularly true for illegal immigrants where contact with the criminal justice system will reveal their status. Another reason why immigrants are less crime prone may be found in selection processes. Immigrants are motivated to come to the US, work hard, and play by the rules. Thus, the antisocial liability of immigrants is low because immigration selects for persons who are motivated and ambitious to achieve.

Another explanation for the immigrant paradox that has received less attention is what can be termed the cultural armamentarium hypothesis. Essentially, this explanation is based on the premise that immigrants come to the US and bring with them their cultural practices (e.g., shared normative structures, similar ways of living, tendency to congregate around other immigrants) thus providing a social network and form of "herd immunity" from many of the challenges of moving to a new nation [33]. Although it is important to theorize why immigrants are less likely to commit crimes, explanations are difficult to test directly.

The present study

While there is an emerging literature showing that immigrants to the US may be less likely to commit crimes than their native-born counterparts, the emphasis in these investigations has been on arrests, official crime rates and other aggregate-level indices. The main weakness of this type of measurement strategy is that most people who commit transgressions are not arrested. Thus, the full depth of specific antisocial behaviors that are not detected by official sources has been understudied among immigrants. Further, to our knowledge, there have been no studies derived from nationally representative samples, hampering the generalizability of previous investigations. Another limitation of the literature is the over-emphasis on Latino immigrants and under-emphasis on immigrants from other regions of the world. These are major gaps that are addressed in the present study. First, we hypothesize that immigrants will report fewer violent and nonviolent antisocial behaviors than native-born Americans while controlling for a notable array of confounds including age, gender, race/ethnicity, education, household income, urbanicity, region of the country, any alcohol, drug, or mental health disorder, and family history of antisocial behavior. Second, we also hypothesize that the immigrant paradox will be invariant with lower prevalence of antisocial behavior found for immigrants from Asia, Latin America, Africa, and Europe.

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Methods

Participants

Study findings are based on data from waves I and II of the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC). The NESARC is a nationally representative sample of 43,093 non-institutionalized US residents aged 18 years and older [34]. The survey gathered background data and extensive information about substance use and comorbid psychiatric disorders from individuals living in households and group settings such as shelters, college dormitories, and group homes in all 50 states and the District of Columbia. The NESARC utilized a multistage cluster sampling design, oversampling young adults, Hispanics, and African–Americans in the interest of obtaining reliable statistical estimation in these subpopulations, and to ensure appropriate representation of racial/ethnic subgroups. Multistage cluster sampling design is a commonly used design when attempting to provide nationally representative estimates. This is because interviewing all participants is not feasible, so larger units (i.e., clusters) are identified and randomly selected from. With respect to the NESARC, 709 primary sampling units (PSUs) provided by the Census Supplementary Survey were selected (stage one). Within the sample PSUs, households were systematically selected (stage two). An individual aged 18 years or older was randomly selected from each household. The response rate for wave I data was 81 % and for wave II was 86.7 % (N = 34,653) with a cumulative response rate of 70.0 %for both waves. Data were weighted at the individual and household levels to adjust for oversampling and nonresponse on demographic variables (i.e., age, race/ethnicity, sex, region, and place of residence). Data were also adjusted to be representative (based on region, age, race, and ethnicity) of the US adult population as assessed during the 2000 Census. Study participants provided fully informed consent. The US Census Bureau and the US Office of Management and Budget approved the research protocol and informed consent procedures.

Diagnostic assessment

Data were collected through face-to-face structured psychiatric interviews conducted by US Census workers trained by the National Institute on Alcohol Abuse and Alcoholism and US Census Bureau. Interviewers administered the Alcohol Use Disorder and Associated Disabilities Interview Schedule–DSM-IV version (AUDADIS-IV), which provides diagnoses for mood, anxiety, personality, and substance use disorders. The AUDADIS-IV has shown to have good-to-excellent reliability in assessing alcohol and drug use in the general population [35, 36].

Measures

Immigrant status—Respondents were queried as to whether they were born in the United States of America. Respondents who reported having been born outside the US (N = 7,320, 14.60 %) were coded as 1 and those born in the US (N = 35,622, 85.40 %) were coded as 0.

Violent and nonviolent antisocial behavior—Violent and nonviolent antisocial behaviors were measured using 31 items from the antisocial personality disorder module of the AUDADIS-IV. Subjects self-reported whether they exhibited any of the behaviors in their lifetime (summarized in Tables 1 and 2). Each item was dichotomously scored (0 = no,

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1 = yes). In addition, for purposes of post hoc analyses, the violent and nonviolent items were also used to form two dichotomous measures representing any violent or nonviolent antisocial behavior, respectively. Psychometric tests of internal consistency reliability for violent and nonviolent antisocial behaviors indicated adequate reliability for both immigrants ($\alpha = 0.67$ and 0.74) and non-immigrants ($\alpha = 0.79$ and 0.80). Several studies have found evidence for concurrent validity of these items [37–39].

Mental and behavioral health—Numerous mental and behavioral health factors were used to reduce confounding, including: lifetime DSM-IV mood (major depression, dysthymia, and bipolar disorder) and anxiety disorder (social phobia, generalized anxiety disorder, panic disorder, and specific phobia); lifetime personality disorder (avoidant, dependent, obsessive compulsive, paranoid, schizoid, or histrionic personality disorder); lifetime alcohol use disorder (abuse/dependence), lifetime drug use disorder (abuse/dependence on heroin, hallucinogens, cocaine/crack, marijuana, stimulants, painkillers, tranquilizers, or sedatives), and parental antisocial influence.

Sociodemographic controls—The following demographic variables were included as control variables: age, gender, race/ethnicity, household income, education level, marital status, region of the US, and urbanicity.

Statistical analysis

Logistic regression analyses were conducted that compared non-immigrants with immigrants to the US in terms of a variety of violent and nonviolent antisocial behavioral outcomes. Adjusted odds ratios (AORs) were considered to be statistically significant, if the associated confidence intervals did not cross the 1.0 threshold. For all statistical analyses, weighted prevalence estimates and standard errors were computed using Stata 12.1SE [40]. This system implements a Taylor series linearization to adjust standard errors of estimates for complex survey sampling design effects including clustered data.

Results

Figure 1 displays the prevalence of violent and nonviolent antisocial behavior among native-born and immigrant adults aged 18 years and older in US. Across the board, the prevalence of antisocial behavior among native-born Americans was greater than that of immigrants. Particularly large differences were observed in terms of involvement in behaviors that could easily hurt oneself/others, truancy, staying out late without permission from one's parents, quitting a job without having other options, shoplifting, and taking part in illegal behaviors that could get one arrested.

Table 1 compares the prevalence of violent antisocial behaviors among native-born Americans and immigrants in the US. With only two non-significant associations (setting a fire on purpose, hurting an animal on purpose), immigrants were significantly less likely to take part in violent antisocial behaviors as compared to native-born Americans. When controlling for sociodemographic and mental/behavioral health factors, the strongest associations between immigrant status and antisocial behavior were found in terms of using

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a weapon in a fight, getting into a fight with a romantic partner, hitting someone so hard that you injure them, and doing things that could easily hurt oneself/others.

Table 2 compares the prevalence of nonviolent antisocial behaviors among native-born Americans and immigrants to the US. With the exception of non-significant associations between immigrant status and traveling around for more than 1 month without plans, lying a lot, using a false name or alias, failing to pay off one's debts, and making money illegally, immigrants were significantly less likely to take part in the majority of nonviolent antisocial behaviors. When controlling for sociodemographic and mental/behavioral health factors, the strongest associations identified between immigrant status and nonviolent antisocial behavior were in reference to being absent from work/school a lot, doing something you could have been arrested for, and shoplifting.

Figure 2 displays the prevalence of violent and nonviolent antisocial behavior among African, Latin American, European, and Asian immigrants and native-born Americans. Across the board, for both violent and nonviolent behaviors, native-born Americans reported the highest levels of antisociality. In terms of comparing immigrants from various regions of the world, European immigrants reported the highest levels of violent and nonviolent antisocial behavior. After European immigrants, the second highest level of both violent and nonviolent antisocial behavior was reported by Latin American immigrants. Asian immigrants reported the lowest levels of violent antisociality and Africans reported the lowest levels of nonviolent antisociality.

Supplemental analyses

Three-year follow-up—We assessed the stability of the lifetime measures in wave I results using wave II data that was based on the past 3 years of self-reported antisocial behaviors (see Fig. 3). Although as expected the prevalence of antisocial behaviors at three-year follow-up was substantially lower than that of wave I, results largely mirrored the overall pattern found for wave I with immigrants reporting lower levels of violent and nonviolent acts even after controlling for the same confounds as in the main analyses.

Age and length of time in the U.S—In these analyses, we wanted to examine whether the age of immigration altered the relationship between immigrant status and nonviolent and violent antisocial behavior. We contrasted these relationships among individuals who had immigrated before the age of 13 and after the age of 13. Controlling for all the same factors presented in our main findings, immigrants who came to the United States at the age of 12 or younger were significantly more likely to report involvement in at least one of the violent (AOR = 2.01, 95 % CI = 1.87–2.15) or nonviolent (AOR = 1.80, 95 % CI = 1.71–1.89) antisocial behaviors as compared with immigrants who arrived at the age of 13 or older. However, when compared to native-born Americans, immigrants who arrived as children or arrived at the age of 13 or older are still significantly less likely to take part in violent and nonviolent antisocial behavior than Native-born Americans, though the latter group begins to somewhat resemble the native-born behaviorally.

Finally, for all immigrants regardless of age, we estimated what each year in the US translates to with respect to an increased probability of reporting an antisocial act. Results

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showed that each additional year an immigrant has lived in the United States is associated with a 1.9 % increase in the likelihood of violence and a 0.9 % increase in the likelihood of nonviolent antisociality.

Discussion

Findings from the present study not only contradict popular culture depictions of immigrants but also provide several unique contributions. Consistent with prior studies that rely on arrests [20, 26], we find immigrants are also less likely to be antisocial. However, perhaps more substantially, no matter the region of the world, immigrants to the US are less likely to engage in violent or nonviolent antisocial behaviors than native-born Americans. Notably, nativeborn Americans were approximately four times more likely to report violent behavior than Asian and African immigrants and three times more likely than immigrants from Latin America. European immigrants were closest to native-born Americans in their antisociality. To our knowledge, this is the first study to examine the prevalence of antisocial behavior among immigrant to the US from all major regions of the world.

Study results also are paradoxical to conventional social scientific reasoning with respect to the social sources of crime and antisocial behavior. Immigrants in the present investigation were more likely to be male, poor, possess less than a high school education, and reside in urban areas. These results run counter to studies of crime among native-born Americans. As such, the immigrant paradox serves a valuable lesson by perhaps forcing us to question our assumptions about crime and antisociality. In this study, even after controlling for the effects of sociodemographic variables and potential confounds such as lifetime alcohol and drug use disorders and mental health conditions, immigrants were still about half less likely than native-born Americans to endorse committing a violent (e.g., using a weapon) or nonviolent antisocial act (e.g., shoplifting).

The immigration paradox has important implications and raises several questions. If increased immigration lowers the crime rate, then can immigration be thought of as a crime prevention strategy? In this scenario, incentives for immigrants to move into high crime areas would be a major component of such a strategy. Recent studies support this notion as the increased neighborhood concentration of immigrants has been associated with substantial reductions in the rates of serious crime [26, 27]. Still another tantalizing speculation is that the crime drop that has fascinated scholars for over a decade could be due to increased immigration [17]. Although provocative, this hypothesis has not been tested.

Despite the promise of immigration as an antidote for crime, an important caveat is that this effect may be fleeting in that the progeny of immigrants over time begins to resemble their adopted culture. We found that each year spent in the US was associated with an increased probability of engaging in antisocial behavior. In other words, while immigrants are less likely to engage in antisocial behavior, the positive benefit lies with the immigrant and as previous research indicates, this pattern may not be intergenerationally stable [30]. In the present study, we found that each additional year an immigrant spends in the US is associated with a 1.9 and 0.9 % increase in nonviolent and violent crime, respectively. Another factor is jobs. Employment opportunities attract immigrants and it is difficult to

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promote immigration to a city that has limited gainful work. Further, increased immigration has been shown to impact employment among urban blacks which in turn leads to increases in the crime rate [28, 29].

Future studies should attempt to test some of the explanations that lie behind the immigrant paradox. While it may seem obvious that immigrants have a lot to lose such as deportation, especially illegal immigrants, if they engage in antisocial behavior, little research has accrued on this explanation. Deeper mechanisms involving selection processes and cultural armamentarium theories also have received little attention. Selection processes can be tested by examining the levels of motivation and perseverance of immigrants compared to a matched sample of non-immigrants from the home nation using counterfactual designs. Although it is observable that existing social networks are an important reason as to where immigrants locate themselves in a new country, examining the shielding effects of these networks and communities in relation to crime and antisocial behavior can be undertaken using ethnographic and social network analysis. This is important not only to better understand the processes behind the genesis of antisocial behavior and crime but also because it may illuminate strategies by which national polices and local initiatives can better prevent crime.

Strengths and limitations

The present study has three major assets. First, we extend previous research on immigrants and crime by examining self-reported acts of antisocial behavior, which are likely to go undetected. Our findings add validity to previous investigations on arrest and official crime rates among immigrants by showing that self-report data converge with these studies indicating that immigrants are indeed less criminogenic and antisocial. Second, prior studies have relied on data from geographically circumscribed areas and the present study is nationally representative. Thus, our findings suggest that previous work is likely generalizable. Third, the present study is novel in that we are able to provide results on antisocial acts from immigrant groups from around the world. This extends the scope of previous work and, to our knowledge, represents the broadest study of immigrants and antisocial behavior or crime ever conducted.

However, despite these strengths, the current study findings require interpretation within the context of several limitations. One limitation is the data are cross-sectional. We rely on retrospective lifetime measures of antisocial behavior and are unable to clarify the causal mechanisms that are associated with antisocial behavior among immigrants. Further, we do not know what the longer term prevalence of antisocial behaviors will be. However, it seems unlikely that immigrant antisocial behavior will make up the difference to that of native-born Americans. This will require life-course study designs. Although the NESARC is a nationally representative sample, it is uncertain how the association between immigrant status and antisocial behavior would be similar or different, if enriched correctional or clinical samples were employed. An additional limitation is that the data did not include important contextual information (e.g., neighborhood) which could be useful to understand the immigrant paradox. Future studies on immigration and antisocial behavior will benefit from including these study features.

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In summary, the immigrant paradox is a novel empirical finding that challenges classic criminological theory and both classic and contemporary public opinion. That persons with multiple risk factors for antisocial behavior engage in less antisocial behavior is an important insight into resilience and should serve as a primary focus for both researchers and policy makers.

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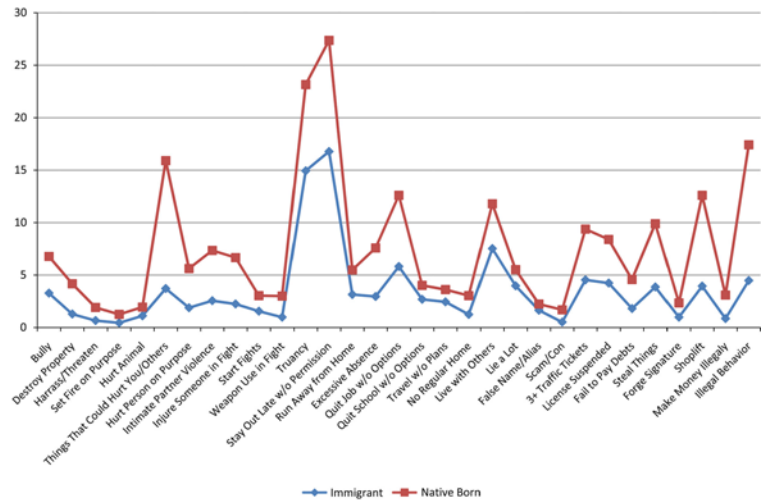

Fig. 1.
Lifetime prevalence of violent and nonviolent antisocial behavior among native-born and immigrants aged 18 and older in the United States (wave I)

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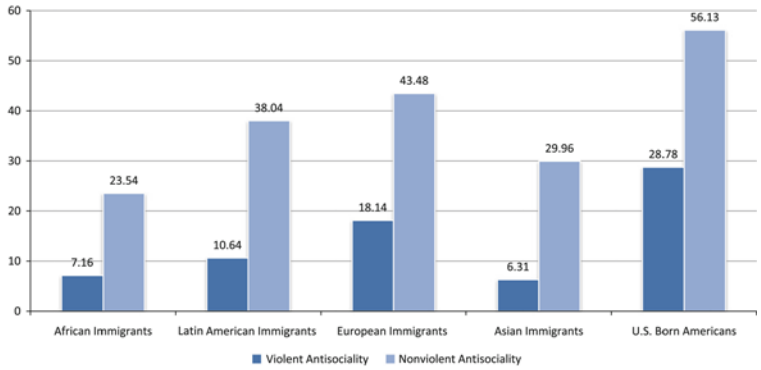

Fig. 2.
Prevalence of violent and nonviolent antisocial behavior among native-born and immigrants aged 18 and older by immigrant country of origin (wave I)

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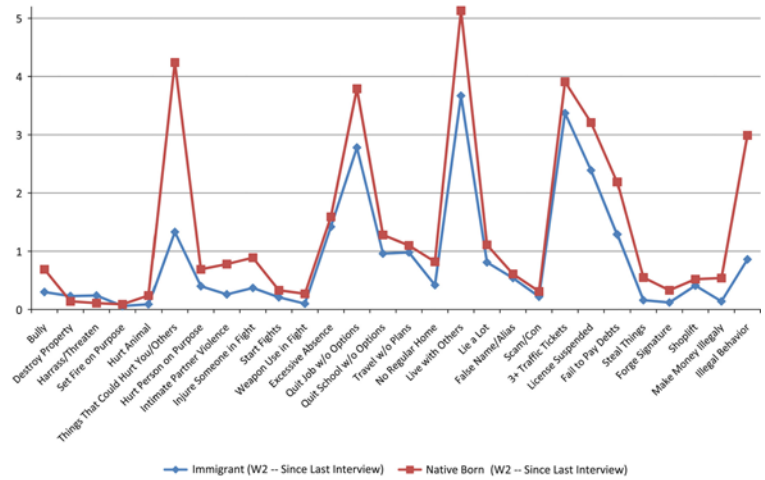

Fig. 3.

Three year follow-up of the prevalence of violent and nonviolent antisocial behavior among native-born and immigrants aged 18 and older in the United States (Wave II)

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Table 1

Violent antisocial behaviors of native-born and immigrants aged 18 and older in the United States

Violent antisocial behaviors	Immigrated to the United States				Unadjusted		Adjusted	
	No (*N* = 35,622, 85.40 %)		Yes (*N* = 7,320, 14.60 %)					
	%	95 % CI	%	95 % CI	OR	(95 % CI)	OR	(95 % CI)
Bullied people	6.78	(6.6–7.0)	3.28	(3.1–3.5)	0.47	(0.44–0.50)	0.63	(0.57–0.69)
Destroy others' property	4.17	(4.0–4.3)	1.29	(1.0–1.6)	0.30	(0.24–0.37)	0.66	(0.50–0.86)
Harass/threatened/blackmail someone	1.92	(1.8–2.0)	0.67	(0.6–0.8)	0.35	(0.29–0.42)	0.69	(0.54–0.88)
Set a fire on purpose	1.25	(1.2–1.3)	0.45	(0.2–0.8)	0.36	(0.20–0.64)	0.99	(0.45–2.19)
Hurt an animal on purpose	1.95	(1.8–2.0)	1.12	(1.0–1.2)	0.57	(0.51–0.63)	0.92	(0.78–1.08)
Do things that could have easily hurt you/others	15.91	(15.6–16.2)	3.72	(3.5–3.9)	0.20	(0.19–0.22)	0.54	(0.48–0.60)
Hurt another person on purpose	5.62	(5.4–5.8)	1.89	(1.7–2.1)	0.32	(0.29–0.36)	0.67	(0.59–0.77)
Get into a fight that came to swapping blows with romantic partner	7.35	(7.1–7.6)	2.56	(2.4–2.7)	0.33	(0.31–0.35)	0.47	(0.43–0.51)
Hit someone so hard that you injure them	6.67	(6.5–6.8)	2.25	(2.1–2.4)	0.32	(0.30–0.35)	0.52	(0.47–0.58)
Get into lots of fights that you started	3.04	(2.9–3.2)	1.56	(1.4–1.7)	0.50	(0.45–0.57)	0.82	(0.71–0.96)
Use a weapon in a fight	3.00	(2.9–3.1)	0.99	(0.9–1.1)	0.32	(0.29–0.37)	0.46	(0.38–0.56)

Adjusted odds ratios adjusted for age, gender, race/ethnicity, household income, education level, marital status, region of the United States, urbanicity, and anxiety and mood disorders, personality disorders, alcohol use disorders, drug use disorders, and parental antisocial influence Odds ratios and confidence intervals in bold are statistically significant

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Table 2

Nonviolent antisocial behaviors of native-born and immigrants aged 18 and older in the United States

Nonviolent antisocial behaviors	Immigrated to the United States				Unadjusted		Adjusted	
	No (*N* = 35,622, 85.40 %)		Yes (*N* = 7,320, 14.60 %)					
	%	95 % CI	%	95 % CI	OR	(95 % CI)	OR	(95 % CI)
Cut class and leave without permission	23.16	(22.8–23.5)	14.94	(14.6–15.3)	0.58	(0.56–0.60)	0.63	(0.60–0.65)
Stay out late at night	27.37	(27.1–27.6)	16.78	(16.4–17.2)	0.53	(0.52–0.55)	0.74	(0.71–0.78)
Run away from home	5.49	(5.3–5.7)	3.15	(3.0–3.3)	0.56	(0.53–0.59)	0.61	(0.55–0.67)
Be absent from work/school a lot	7.59	(7.4–7.8)	2.96	(2.8–3.1)	0.37	(0.35–0.40)	0.43	(0.39–0.48)
Quit a job without knowing where to find another	12.61	(12.4–12.8)	5.82	(5.5–6.1)	0.43	(0.40–0.45)	0.57	(0.52–0.61)
With a school program without knowing what to do next	4.02	(3.9–4.1)	2.69	(2.5–2.8)	0.66	(0.62–0.70)	0.67	(0.61–0.73)
Travel around more than 1 month without plans	3.62	(3.5–3.8)	2.45	(2.3–2.6)	0.67	(0.62–0.72)	1.00	(0.88–1.14)
Have no regular place to live at least 1 month	3.04	(2.9–3.2)	1.25	(1.1–1.4)	0.40	(0.37–0.44)	0.56	(0.49–0.65)
Live with others at least 1 month	11.80	(11.5–12.1)	7.53	(7.2–7.8)	0.61	(0.58–0.64)	0.79	(0.74–0.84)
Lie a lot	5.52	(5.4–5.7)	3.98	(3.7–4.3)	0.71	(0.66–0.76)	1.06	(0.96–1.18)
Use a false or made-up name/alias	2.24	(2.1–2.3)	1.65	(1.5–1.8)	0.73	(0.66–0.81)	0.95	(0.82–1.10)
Scam/con someone for money	1.69	(1.6–1.8)	0.52	(0.4–0.6)	0.30	(0.26–0.36)	0.55	(0.43–0.71)
Get three or more traffic tickets for reckless driving/causing accidents	9.39	(9.2–9.6)	4.54	(4.4–4.7)	0.46	(0.44–0.48)	0.81	(0.77–0.85)
Have a driver's license suspended/revoked	8.40	(8.2–8.6)	4.24	(4.0–4.5)	0.48	(0.45–0.51)	0.71	(0.66–0.76)
Fail to pay off your debts	4.58	(4.4–4.7)	1.81	(1.6–2.1)	0.52	(0.41–0.66)	0.86	(0.70–1.05)
Steal anything from others	9.90	(9.7–10.1)	3.87	(3.7–4.1)	0.37	(0.35–0.39)	0.67	(0.61–0.73)
Forge someone's signature	2.35	(2.2–2.5)	0.99	(0.9–1.0)	0.41	(0.38–0.45)	0.69	(0.63–0.76)
Shoplift	12.61	(12.3–12.9)	3.95	(3.7–4.2)	0.28	(0.26–0.31)	0.48	(0.44–0.52)
Make money illegally	3.10	(3.0–3.2)	0.86	(0.6–1.2)	0.27	(0.20–0.37)	0.86	(0.59–1.24)
Do something you could have been arrested for	17.42	(17.1–17.8)	4.48	(4.2–4.8)	0.22	(0.21–0.24)	0.46	(0.41–0.52)

Adjusted odds ratios adjusted for age, gender, race/ethnicity, household income, education level, marital status, region of the United States, urbanicity, anxiety and mood disorders, personality disorders, alcohol use disorders, drug use disorders, and parental antisocial influence Odds ratios and confidence intervals in bold are statistically significant

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What Accounts for Differences in Substance Use Among U.S.-Born and Immigrant Hispanic Adolescents?: Results from a Longitudinal Prospective Cohort Study

Guillermo Prado, Ph.D.[a,*], **Shi Huang, Ph.D.**[a], **Seth J. Schwartz, Ph.D.**[a], **Mildred M. Maldonado-Molina, Ph.D.**[b], **Frank C. Bandiera, M.P.H.**[a], **Mario de la Rosa, Ph.D.**[c], and **Hilda Pantin, Ph.D.**[a]

[a]Department of Epidemiology and Public Health, University of Miami Miller School of Medicine, Miami, Florida

[b]Department of Epidemiology and Health Policy, College of Medicine and Institute for Child Health Policy, University of Florida, Gainesville, Florida

[c]School of Social Work, College of Public Health and Social Work, Florida International University, Miami, Florida

Abstract

Purpose—The current study was conducted to ascertain whether the effects of nativity (i.e., U.S. born vs. immigrant) on Hispanic adolescent substance use is mediated by ecological processes such as family functioning, school connectedness, and perceived peer substance use.

Methods—The effects of family, peer, and school processes on adolescent substance use were examined in a nationally representative sample of 742 (358 male, 384 female) Hispanic youth (mean age = 15.9; SD = 1.8).

Results—Results from a structural equation model indicated that the higher rates of substance use among U.S.-born Hispanics (compared with foreign-born Hispanics) are partially mediated by perceived peer substance use (as measured by the adolescent). The results also showed that perceived peer substance use and school connectedness mediate the relationship between family processes and substance use, suggesting that family processes may offset some of the deleterious effects of negative peer selection on adolescent substance use.

Conclusion—These findings imply that public health behavioral interventions to prevent substance use among both U.S.-born and foreign-born Hispanics may need to attend to multiple ecological processes, including family, school, and peers.

Keywords

Substance use; Hispanic; Adolescents; Nativity; Immigrant

Cigarette smoking, alcohol, and illicit drug use represent important public health problems facing American youth [1]. However, these problems are not evenly distributed across ethnic groups. According to national population-based studies, Hispanic adolescents report higher rates of cigarette use, alcohol use, and use of most categories of illicit drug use than non-Hispanic white and African American adolescents [1]. This health disparity is

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*Address correspondence to: Guillermo Prado, Ph.D., Department of Epidemiology and Public Health, University of Miami, 1425 N.W. 10th Avenue, Third floor, Miami, FL 33136. gprado@med.miami.edu.

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especially alarming given that Hispanics represent the largest and fastest growing minority population in the United States [2]. It is expected that by 2050, Hispanics will comprise 24% of all the U.S. population [3]. Furthermore, Hispanics are a young population, with nearly 40% under age 20 [4]. Because of the large and growing population of Hispanic youth, decreasing behavioral health risks such as cigarette, alcohol, and drug use in this population is of considerable public health importance. Efforts to understand the prevalence and etiology of cigarette, alcohol, and drug use, among Hispanic adolescents are challenged by the heterogeneity within the Hispanic population. Studies attempting to identify risk and protective processes for substance use in Hispanics need to consider the heterogeneity of this population [5,6].

Nativity status (U.S. born vs. immigrant) [7] is a primary demographic source of variation in the Hispanic population. Previous studies have shown that U.S.-born Hispanics report higher rates of cigarette, alcohol, and drug use than their immigrant counterparts [8-11]. Some studies have examined reasons explaining why U.S.-born Hispanics are more likely to report substance use than their foreign-born counterparts. For example, there is a growing body of literature describing the Immigrant Paradox [11]: the tendency for foreign-born Hispanics, who would be expected to show poorer signs of health because of immigration and life disruptions, to report more favorable health indicators than U.S.-born Hispanics. Potential explanations for the Immigrant Paradox include the deterioration of cultural and Hispanic family values, attitudes, and behaviors [6], and the increased exposure to substances in the United States than in the immigrant host country [12]. Social–ecological processes such as school, peer, and family processes may also explain, at least in part, the Immigrant Paradox [5,7,13].

It should be noted, however, that these studies conducted to examine the Immigrant Paradox have been sampled only from specific areas of the United States. Because different segments of the Hispanic population have settled in different parts of the country [14], a nationally representative sample would be most likely to provide authoritative evidence related to the Hispanic Immigrant Paradox. To our knowledge, however, no studies have used nationally representative samples to examine whether ecological processes such as family functioning, school connectedness, and peers partially explain the Immigrant Paradox in a prospective cohort design. A number of studies, however, have documented the direct relationships of family functioning, school connectedness, and peer drug use to substance use in adolescents in general, and in Hispanic adolescents in particular (see [5] for a review). Positive family functioning, bonding to and success in school, and avoidance of substance using peers have been identified as protective against substance use in both general population and Hispanic adolescents [5,15-17]. Furthermore, a recent study by Galaif and colleagues [18] found that U.S.-born Hispanics had more deviant peers, poorer family functioning, and less school connectedness than immigrant Hispanics. Thus, these processes might help explain why U.S.-born Hispanics report higher rates of substance use than their immigrant counterparts. Therefore, the purpose of the present study was to determine whether, and to what extent, family functioning (defined as parental involvement and family connectedness), school connectedness, and peer substance may partially mediate the effects of nativity and Hispanic adolescent substance use (see Figure 1).

These ecological processes (i.e., family functioning, school connectedness, and peer substance use) were chosen as potential mediators for the association between nativity status and adolescent substance use, because they are central to ecodevelopmental theory [13,19]. Ecodevelopmental theory is a conceptual model that describes the interconnections among various sources of risk and protection in adolescents' lives. Microsystems, which represent contexts in which the adolescent participates directly, such as the family, school, and peers, have by far the strongest effects on adolescent development (e.g., [20]), and hence, are the

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focus of the present study. Ecodevelopmental processes such as parental involvement, family connectedness, peer substance use, and school connectedness have been found to be related to adolescent substance use [13,17].

Method

Study design

The present study analyzed data from the National Longitudinal Study of Adolescent Health, commonly referred to as Add Health. Add Health is a school-based, prospective, nationally representative cohort study of adolescents in grades 7 through 12. It was designed to examine the effects of social influences on health-related behaviors such as alcohol, cigarette, and drug use. The Add Health dataset consists of three waves. Wave 1 data collection occurred in 1994 and 1995 and included in-school interviews of 90,118 adolescents in grades 7 through 12. Of those adolescents, 13,143 completed home-based interviews. Substance use data were collected during these in-home interviews. Wave 2 consisted of follow up in-home interviews 1 year later. Wave 3, conducted in 2001 and 2002 (i.e., 5 to 6 years after the Wave 2 data), followed the adolescent participants into emerging adulthood. Because the focus of the present study is on adolescents (and not on emerging adulthood), the Wave 3 data were not used in the present study. With regard to the sampling frame, all high schools in the U.S. that had an 11th grade and a minimum enrollment of 30 students were eligible for participation. Of the 26,666 eligible schools, 132 were randomly selected with unequal probability. Implicit stratification and systematic sampling methods ensured that the schools selected were representative of all U.S. high schools with respect to school size, school type, U.S. region, location, and percentage of white students. The response rates for the Wave 1 and Wave 2 surveys were 78.9% and 88.2%, respectively. Additional details regarding the study design, including sampling frame, for the Add Health study can be found at http://www.cpc.unc.edu/projects/addhealth/design.

Participants

The present study used the Wave 1 and Wave 2 Add Health public use data set, distributed by Sociometrics Corporation (http://www.socio.com). A total of 742 participants (358 boys, 384 girls) who identified themselves as Hispanic or Latino during the Wave 1 survey and completed the nativity status (i.e., U.S. born vs. foreign born) item were included in the analyses. At Wave 1, the majority of the participants solely self-identified as Mexican (55.6%), Puerto Rican (13.0%), or Central/South American (12.0%). Of the 742 participants, 73.7% were U.S. born, whereas 26.3% were foreign born. Participants' average age was 15.9 years (SD = 1.8 years). A total of 17.9% of adolescents' families were on some sort of public assistance.

Measures

Family functioning at Wave 1 was assessed using two indicators: family connectedness and parental involvement. The 11-item family connectedness subscale (Cronbach's α = .88) assesses the extent to which adolescents perceive their mother and father as warm, loving, and caring, as well as adolescents' overall satisfaction with their relationships with their mother, father, and other family members. Each item is rated on a five-point Likert scale ranging from "strongly agree" to "strongly disagree" or from "not at all" to "very much" (the labels for the endpoints vary as a function of item content). Consistent with prior research [21,22], the sum of the 11 items is used as a single subscale in the analyses. The 20-item parent–adolescent activities subscale assesses the extent of parental involvement in various activities with the adolescent during the 4 weeks prior to assessment. The activities listed include going shopping, playing a sport, and attending a religious service or churchrelated event. Each item is binary, where 0 indicates that the parent did not participate

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in the indicated activity with their adolescent and 1 indicates that the parent did participate in the activity with their adolescent. A single scale consisting of the sum of the counts was used in the analyses.

School connectedness at Wave 1 was assessed using six items. These items asses whether adolescents feel they are connected/bonded to their school and to their teachers, whether adolescents feel safe at school, and whether adolescents feel their teachers are treating them fairly. Adolescents responded on a five-point Likert scale ranging from "strongly disagree" to "strongly agree" or "not at all" to "very much." Cronbach's α in the present sample is .77.

Adolescent's report of their peers' substance use at Wave 1 was assessed using three items. Adolescents were asked whether any of their three best friends (a) smoked at least 1 cigarette a day (perceived peer cigarette use), (b) drank alcohol at least once a month (perceived peer alcohol use), and (c) used marijuana at least once a month (perceived peer drug use). Details on how these items were used in the analyses can be found in the "Adolescent Substance Use and Perceived Peer Substance Use Measurement Models" section below.

Adolescent's own substance use at Wave 1 was measured using three items: (a) ever tried a cigarette; (b) ever drank beer, wine, or liquor (not just a sip or a taste of someone else's drink) more than two or three times; and (c) ever used marijuana. During their Wave 2 interview (i.e., 1 year after the Wave 1 interview), adolescents responded whether they had (a) tried a cigarette since their Wave 1 interview; (b) drank beer, wine, or liquor (not just a sip or a taste of someone else's drink) more than two or three times since their Wave 1 interview; or (c) used marijuana since their Wave 1 interview. Because data on illicit drugs (other than marijuana use) were not collected for perceived peer substance use and to have parallel measures for perceived peer substance use and adolescent substance use, we only included marijuana use (in addition to alcohol and cigarette use) for the adolescent substance use measure. Details on how these items were used in the analyses can be found in the "Adolescent Substance Use and Perceived Peer Substance Use Measurement Models" section below.

Results

Descriptive statistics and bivariate correlations

Descriptive statistics and bivariate correlations for the ecological processes and substance use variables can be found in Table 1 and Table 2, respectively.

Differences between U.S.-born and foreign-born adolescents

Substance use—STATA version 9.0 was first used to calculate the weighted cigarette, alcohol, and marijuana use prevalence rates for both the U.S.- and foreign-born Hispanics at Wave 1 and Wave 2. Rao-Scott chi-square tests were then used to compare whether rates of lifetime (measured during Wave 1) and past year (measured during Wave 2) cigarette, alcohol, marijuana, and substance use differed between immigrant and U.S.-born Hispanic youth.

Results indicated that both lifetime, Wave 1: $\chi^2(1) = 319.4$, $p < .001$, and past year, Wave 2: $\chi^2(1) = 142.7$, $p < .01$, overall substance use rates (i.e., alcohol, cigarette, or marijuana use) were significantly higher among U.S.-born than immigrant Hispanic youth (Figure 2). For example, at the time of the Wave 1 assessment, 75.7% of U.S.-born youth reported having ever used cigarettes, alcohol, or marijuana, compared to 52.4% of immigrant youth. Furthermore, compared to foreign born youth, U.S.-born youth reported higher rates of lifetime cigarette use, $\chi^2(1) = 228.1$, $p < .001$, lifetime alcohol use, $\chi^2(1) = 340.4$, $p < .001$,

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and lifetime marijuana use, $\chi^2(1) = 265.2$, $p < .001$. As noted in Figure 2, the prevalence estimates for lifetime cigarette use, alcohol use, and marijuana use for U.S.-born youth were 58.5%, 63.4%, and 33.3%, respectively, compared to 37.2%, 37.5%, and 12.5% for foreign-born youth. Similarly at Wave 2, U.S.-born youth reported higher rates of past year cigarette use, $\chi^2(1) = 98.9$, $p < .01$, alcohol use, $\chi^2(1) = 109.8$, $p < .01$, and marijuana use, $\chi^2(1) = 202.7$, $p < .001$, than did immigrant youth. As shown in Figure 2, the prevalence estimates for past year cigarette use, alcohol use, and marijuana use for U.S.-born youth (at Wave 2) were 43.3%, 54.3%, and 33.6%, respectively, compared to 27.1%, 36.8%, and 12.2% for immigrant youth.

Social–contextual functioning—Weighted mean comparisons were conducted to determine whether family connectedness, parental involvement, and school connectedness at Wave 1 differed between U.S.-born and immigrant Hispanic youth. Rao-Scott chi-square tests were conducted to determine whether perceived peer substance use differed between U.S.-born and foreign-born Hispanic youth.

Significant differences emerged for both school connectedness and perceived peer substance use, with U.S. born reporting less school connectedness and more peer using substances. No significant differences between immigrant and U.S.-born youth emerged for family connectedness and parental involvement (Table 1).

Adolescent substance use and perceived peer substance use measurement models

The next step of the analysis was to test the hypothesized ecological model depicted in Figure 1. First, measurement models were estimated to ascertain the feasibility of collapsing multiple binary indicators of adolescent substance use and perceived peer substance use into two separate measurement models (one for substance use and a second for perceived peer substance use) by conducting confirmatory factor analyses using Mplus version 5.1. Although both the adolescent substance use and perceived substance use measurement models were just identified (and hence model fit statistics were not available), results indicated that all three adolescent substance use and all three perceived substance use indicators loaded significantly onto their respective latent constructs. The standardized loadings were .62, .80, and .93 for adolescent alcohol use, adolescent cigarette use, and adolescent marijuana use. The standardized loadings were .85, .72, and .79 for perceived peer alcohol use, perceived peer cigarette use, and perceived peer marijuana use. The perceived peer substance use and adolescent substance use latent variables are represented by circles in Figures 1 and 3.

Structural equation models

We estimated the hypothesized structural equation model (Figure 1), controlling for gender and adolescent lifetime substance use at Wave 1, to ascertain the hypothesized relationships between (a) nativity, family functioning (i.e., family connectedness and parental involvement), perceived peer substance use, and school connectedness (measured at Wave 1) and past-year adolescent substance use at Wave 2. The fit of this model to the data was evaluated primarily in terms of the comparative fit index (CFI) and the root-meansquare error of approximation (RMSEA). The chi-square statistic is reported but is not used in interpretation, given that it often indicates significant deviations even when these deviations are quite small [23]. CFI values of .95 or greater, and RMSEA values of .06 or less, are indicative of good model fit [24].

The model provided an adequate fit to the data, $\chi^2(9) = 33.5$, $p < .01$; CFI = .94; RMSEA = .02. To explore the effects of social–ecological variables within the overall model, we examined the path coefficients for each set of direct and indirect relationships. The results

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indicated (Figure 3) that perceived peer substance use ($B = .68$, $p < .001$) was positively related to adolescent substance use at Wave 2, whereas school connectedness was negatively related to adolescent substance use ($B = -.15$, $p < .05$). Neither nativity ($B = 0.07$, ns), family connectedness ($B = 0.13$, ns), nor parental involvement ($B = 0.10$, ns) were significantly related to adolescent substance use.

It is worthy of note that nativity was significantly predictive of substance use at the bivariate level (Figure 2) but was no longer significant when the social–ecological variables were included in the model. This suggests that the effects of nativity on substance use (i.e., the Immigrant Paradox) may have been mediated by one or more of the social–contextual variables in the model. As a result, as a final step of analysis, we tested this possibility empirically. We used the asymmetric distribution of products tests [25] to evaluate whether the effects of nativity status and substance use were mediated by (a) perceived peer substance use, (b) school connectedness, (c) family connectedness, and (d) parental involvement. This test constructs a 95% confidence interval around the product of the two unstandardized path coefficients that comprise each mediating pathway. If the confidence interval for a given product does not include zero, then partial mediation is assumed. This method is more accurate and statistically powerful [26] than traditional methods of testing for mediation.

The indirect path between nativity and substance use through perceived peer substance use was significant (unstandardized point estimate: 0.446; 95% confidence interval [CI] = 0.250 to 0.642). None of the other indirect paths involving nativity and substance use reached significance: (a) family connectedness, unstandardized point estimate = −0.011; 95% CI = −0.014 to 0.035; (b) parental involvement, unstandardized point estimate = 0.006; 95% CI = −0.014 to 0.027; and (c) school connectedness, unstandardized point estimate = 0.042; 95% CI = −0.006 to 0.089). As a result, we concluded that only perceived peer substance use mediated the effect of nativity on substance use.

Post hoc mediation analyses

Although family connectedness and parental involvement were not directly related to adolescent substance use, we sought to examine whether family connectedness and parental involvement may have been indirectly related to substance use through perceived peer substance use and school connectedness. Post hoc mediation analyses were conducted using the asymmetric distribution of products test. Results indicated two significant mediators of the effect of family connectedness on substance use: perceived peer substance use (unstandardized point estimate = −0.033; 95% CI = −0.045 to −0.021) and school connectedness (unstandardized point estimate = −0.011; 95% CI = −0.020 to −0.002). Results also indicated two significant mediators of the effect of parental involvement on substance use: perceived peer substance use (unstandardized point estimate = −0.037; 95% CI = −0.058 to −0.015) and school connectedness (unstandardized point estimate = −0.011; 95% CI = −0.020 to −0.001).

Discussion

The objective of the current study was to determine whether, and to what extent, ecological processes such as family connectedness, parental involvement, perceived peer substance use, and school connectedness can partially explain the differences in substance use between U.S.-born and immigrant Hispanic adolescents. Such differences represent the “Immigrant Paradox,” whereby immigrants evidence more favorable health outcomes than their U.S.-born counterparts.

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As anticipated, the Immigrant Paradox emerged in the present study: immigrant adolescents were significantly less likely to report using cigarettes, alcohol, or marijuana compared to U.S.-born adolescents. This effect of nativity was mediated by perceived peer substance use. Specifically, we found that U.S.-born Hispanic youth were more likely to associate with substance using peers than were foreign-born youth, and consequently, U.S.-born Hispanic youth were more likely to report using substances themselves 1 year later. We also found that perceived peer substance use and school connectedness were directly associated with substance use. This relationship suggests that school bonding and affiliation is protective against adolescent substance use beyond simply discouraging adolescents from affiliating with substance-using peers [17]. The finding that perceived peer substance use was directly related to substance use is consistent with much of the existing literature on non-Hispanic white [27] and Hispanic [17,28] samples, which suggests that the peer context is most proximally related to substance use, whereas other contexts such as the family and peer are less proximal at this time of development. At least two other studies have reported similar findings [29,30]. However, these two other studies have been limited by sampling a nonrepresentative sample of Hispanics in the United States.

Although we did not find a direct relationship between parental connectedness and substance use or between parental involvement and substance use, we did find that perceived peer substance use and school connectedness mediated the relationship between family connectedness and substance use as well as between parental involvement and substance use. This finding is consistent with prior work [31], and extends past research by examining the role of multiple contexts (peer and school) as potential mediators of the relationship between family processes and substance use and by modeling relationships to substance use over time. Coupled with our finding that the Immigrant Paradox, at least with regard to substance use, is explained (to some extent) by family processes (i.e., parental involvement, family connectedness) and affiliation with peers whom the adolescent perceives as using substances, the protective effects of family processes suggest that family processes such as parental involvement and family connectedness may help to counteract the Immigrant Paradox. Specifically, improving family connectedness and parental involvement may help to offset the effects of nativity on affiliation with perceived substance-using peers, and may therefore help to reduce risks for substance use in U.S.-born Hispanic adolescents. However, given that all of the mediators were assessed at the same point in time, future research should replicate these findings before definitive conclusions can be made. Nonetheless, the present findings suggest that family strengthening programs may represent one way to intervene to prevent substance use in U.S.-born Hispanic adolescents [5]. Indeed, previous research has found that behavioral interventions designed to improve family processes in Hispanic families may lead to reductions in substance use, both among immigrant and U.S.-born Hispanic adolescents [32].

Study limitations and strengths

The present results should be interpreted in light of several important limitations. First, this study, and most of the Add Health data as a whole, relies exclusively on self-report measures. Independent reports of family connectedness and parental involvement (e.g., observational tasks) and of substance use (e.g., biological assays) were not available. A second limitation is that both peer substance use and adolescent substance use were reported solely by the adolescent. Consequently, it is possible that the association between (perceived) peer substance use and adolescent substance use may have been inflated by the tendency for adolescents to assume that they and their peers are engaging in similar degrees of substance use (e.g., [33]). Third, the current study examined the prospective association of ecological processes and substance use among a nationally representative sample of Hispanic youth of data collected between 1994 and 1996. There is evidence that

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demonstrates that the prevalence of substance use among all adolescents (including Hispanics) has declined in the past decade. Additionally, patterns of drug use may have changed in the past 15 years, with drugs such as ecstasy and methamphetamines becoming popular in the late 1990s [1]. Furthermore, there has been massive migration from Latin America to the United States, and this wave of newer immigrants may not necessarily be reflective of the older immigrants. However, more recent cohorts of nationally representative Hispanic samples (with data on family, school, and peer processes) are not available. Nonetheless, the ecological processes associated with substance use progression are still relevant. A final limitation is that, given the moderate sample size, we were not able to stratify the analyses by country of origin or gender. Such research is important to continue exploring the effects of nativity on adolescent substance use. The present study is also characterized by several strengths, including the prospective cohort design, the wide range of social–ecological processes measured, and the nationally representative sample. Another strength of the present study is that we examine both the direct and indirect effects of nativity on Hispanic adolescent substance use.

Conclusions and Implications for Public Health

Despite these limitations, the present study has identified a potential mechanism of ecological processes (although it is important to note that there may be others) that may help to explain the Immigrant Paradox. It appears that immigrant adolescents may be less likely to affiliate with substanceusing peers, and that elements of family functioning may help to offset the effects of nativity on affiliation with substance-using peers, and may therefore help to reduce substance use rates in U.S.-born Hispanic adolescents. In the recent past a call has been made to increase research efforts to understand the etiology of substance use in U.S.- born Hispanic youth and to develop and evaluate prevention interventions for this population based on basic epidemiological research [6]. At the minimum, the findings presented in this study suggest that behavioral interventions to prevent substance use in U.S.-born Hispanic youth should attend to multiple ecological processes, including family and peers. Such interventions have already shown promise in preventing substance use among U.S.-born and foreign-born youth [32-34].

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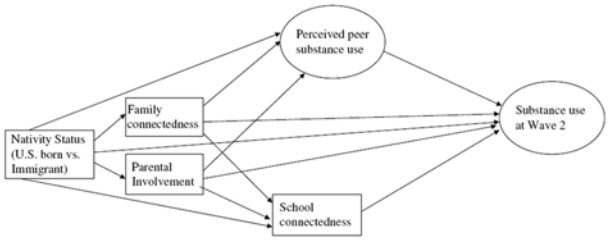

Figure 1.
Hypothesized ecological predictors of hispanic adolescent substance use.

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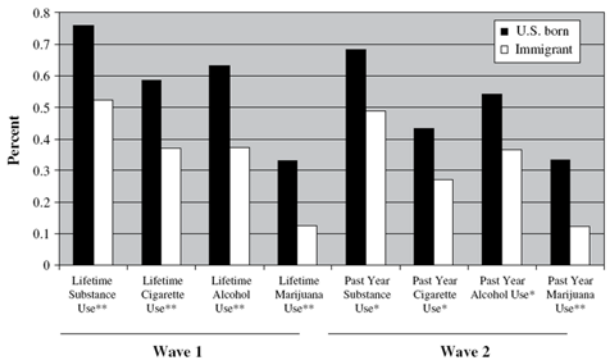

Figure 2.
Substance use prevalence estimates for U.S.-born and immigrant Hispanic youth.
* p<0.01; **p<0.001.

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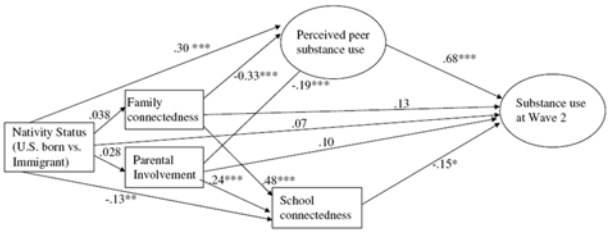

Figure 3.

Model of the Wave 1 ecological predictors of Hispanic adolescent substance use at Wave 2. Note. ***$p < .001$; **$p < .01$; *$p < .05$. $\chi^2(9) = 33.5$, $p < 0.01$; CFI = .94; RMSEA = 0.02. Note: We controlled for both gender and lifetime substance use at Wave 1. These control variables were not significantly related to the outcome and hence were not included in the figure.

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Table 1

Comparisons of demographic characteristics and ecological predictors by nativity status

	U.S. born (n = 546)	Immigrant (n = 196)			Total
	Percent*		$\chi^2(1)$	*p*	
Gender			6.04	.50	
Male	50.9%	47.4%			50.0%
Female	49.1%	52.6%			50.0%
Public assistance	18.7%	15.8%	6.84	.37	17.9%
Perceived peer substance use	72.1%	49.9%	268.3	<.001	66.3%
	Mean (SD)*		$F(1, 131)$	*p*	
Age	15.7 (1.8)	16.4 (1.7)	16.6	.0001	15.9 (1.8)
Family connectedness	46.8 (6.6)	46.1 (6.9)	1.8	.18	46.6 (6.6)
Parental involvement	5.4 (3.4)	5.2 (3.2)	0.5	.48	5.3 (3.3)
School connectedness	21.8 (4.4)	22.8 (4.0)	7.2	.0081	22.1 (4.3)

* Analyses used weighted data to take account for the clustered sampling design.

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Table 2

Means (or percentages), standard deviations, and correlations for the ecological processes and substance use variables

Variable	Mean or percentage	SD[a]	1	2	3	4	5	6	7	8	9
1. Family connectedness	46.6	6.6	1.00								
2. Parental involvement	5.3	3.3	0.29*	1.00							
3. School connectedness	22.1	4.3	0.42*	0.18*	1.00						
4. Peer marijuana use	36.0%		−0.19*	−0.09*	−0.29*	1.00					
5. Peer alcohol use	52.8%		−0.23*	−0.07*	−0.24*	0.67*	1.00				
6. Peer cigarette use	39.7%		−0.14*	−0.15*	−0.34*	0.57*	0.61*	1.00			
7. Adolescent marijuana use at Wave 2	28.2%		−0.13*	−0.06*	−0.23*	0.61*	0.40*	0.38*	1.00		
8. Adolescent cigarette use at Wave 2	39.2%		−0.14*	−0.06*	−0.18*	0.48*	0.35*	0.38*	0.74*	1.00	
9. Adolescent alcohol use at Wave 2	49.9%		−0.04*	−0.001	−0.13*	0.40*	0.43*	0.27*	0.58*	0.49*	1.00

[a]We do not report *SD* for percentages.

*$p < .001$.

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WHY ARE IMMIGRANTS' INCARCERATION RATES SO LOW? EVIDENCE ON SELECTIVE IMMIGRATION, DETERRENCE, AND DEPORTATION

Kristin F. Butcher
Anne Morrison Piehl

Working Paper 13229
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Why are Immigrants' Incarceration Rates so Low? Evidence on Selective Immigration, Deterrence, and Deportation
Kristin F. Butcher and Anne Morrison Piehl
NBER Working Paper No. 13229
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JEL No. J1,J2,K4

ABSTRACT

The perception that immigration adversely affects crime rates led to legislation in the 1990s that particularly increased punishment of criminal aliens. In fact, immigrants have much lower institutionalization (incarceration) rates than the native born - on the order of one-fifth the rate of natives. More recently arrived immigrants have the lowest relative incarceration rates, and this difference increased from 1980 to 2000. We examine whether the improvement in immigrants' relative incarceration rates over the last three decades is linked to increased deportation, immigrant self-selection, or deterrence. Our evidence suggests that deportation does not drive the results. Rather, the process of migration selects individuals who either have lower criminal propensities or are more responsive to deterrent effects than the average native. Immigrants who were already in the country reduced their relative institutionalization probability over the decades; and the newly arrived immigrants in the 1980s and 1990s seem to be particularly unlikely to be involved in criminal activity, consistent with increasingly positive selection along this dimension.

Kristin F. Butcher
Department of Economics
Wellesley College
106 Central Street
Wellesley, MA 02481-8203
kbutcher@wellesley.edu

Anne Morrison Piehl
Department of Economics
Rutgers, The State University of New Jersey
New Jersey Hall
75 Hamilton Street
New Brunswick, NJ 08901-1248
and NBER
apiehl@economics.rutgers.edu

Much of the concern that immigration to the United States adversely affects crime derives from the fact that immigrants tend to have characteristics in common with native-born populations that are disproportionately incarcerated. That is, immigrants have low average levels of education and very low average wages, and many are young, male, and Hispanic. For similar reasons, there are general concerns that immigration adds to the "underclass" in the United States by increasing dependence on cash assistance and subsidized medical care, decreasing homeownership, and creating pockets of entrenched poverty with adverse social outcomes.[1] During the 1990s, when immigration rates were high and crime rates were high and rising, observers feared a link between immigration and crime, and several significant pieces of federal legislation increased criminal penalties for noncitizens.

Economic theories tend to support the concern about a link between immigration and crime. The economic model of crime (Becker 1968), for example, posits that those who have poor labor market outcomes—and thus low opportunity costs from giving up activities in the legal sector—will be more likely to engage in criminal activity. Many studies have documented immigrants' poor labor market outcomes (see, for example, Borjas 2004). These outcomes are due in part to the low skills that many immigrants bring with them, and in part to immigrants' loss of other elements of human capital (such as language and social networks) that enable individuals to make full use of their skills. A one-dimensional model of skills would lead one to

[1] Research on the criminal justice outcomes of immigrants is limited (Mears 2002). However, research on other outcomes shows that immigrants are less likely to use welfare than similar natives (Butcher and Hu 2000). Immigrants are less likely to own homes than the native-born, and this gap widened between 1980 and 2000. However, this gap is mainly driven by location choice and immigrants' country of origin. Future increases in immigrant enclaves may be expected to increase demand for owner-occupied housing (Borjas 2002). Other research has examined the participation of immigrants in mainstream financial institutions. Recent evidence suggests that immigrants are less likely to participate in financial markets, that these differences tend to persist, and that they may be driven by immigrants' experience with financial institutions in their countries of origin (Osili and Paulson 2004a, b).

expect that a population with poor labor market outcomes would also have poor outcomes in other arenas such as crime, health, and family life.

In this paper, we examine immigrants' institutionalization rates as a proxy for incarceration, and thus their involvement in criminal activity. Contrary to what one might expect from labor market studies, immigrants have very low rates of institutionalization compared with the native born. What's more, immigrants' relative rates of institutionalization have fallen over the last three decades. More recent cohorts also have better criminal-justice outcomes than earlier cohorts, and synthetic cohort analyses show that immigrants' relative rates of institutionalization tend to decline with time in the country. If one assumed that the relationship between "skills" and outcomes is the same among immigrants and the native born, this is precisely the opposite of what one would have predicted from most synthetic cohort analyses of immigrants' labor market outcomes.

Important laws enacted in the 1990s increased penalties for criminal noncitizens by broadening the crimes for which they could be deported. Did this mechanically lower immigrants' institutionalization rates by ensuring that criminal aliens were deported? Or did the greater punishment change immigrants' criminal activity in the U.S.? Beyond laws specifically increasing punishments for criminal aliens, the 1980s and 1990s saw increased punishment for crimes more generally. Did these changes affect immigrants' behavior more than the native born? Or did increased punishment for criminal activity combined with welfare reform—which decreased immigrants' access to social welfare programs—change the type of person who self-selects to immigrate to the United States?

We present a variety of tests of these potential explanations. We rule out deportation as an important mechanical factor for the observed differences in institutionalization. Instead, the

evidence is consistent with a model of multiple dimensions regarding who self-selects to immigrate to the United States. That is, the type of person who chose to immigrate to the United States in the 1990s shifted toward those who are less criminally active. The results also suggest that immigrants are more responsive to criminal punishment than the average native, and decreased their criminal activity in response to harsher criminal penalties imposed during the 1980s and 1990s.

Immigrant Skill Levels and Self-Selection

The last four decades have seen striking changes in immigration to the United States. Borjas (2004) provides a thorough accounting of the experience of immigrants in the U.S. labor market. Male immigrants have slightly lower employment rates but wage rates that are substantially below those of the native born. While in 1960 immigrants' wages were 6.5% above those of natives, by 2000 they were 19% lower. Those who have been in the U.S. for the shortest period have larger deficits: in 1960 those who had arrived recently earned 9% less than natives, but that gap expanded to 38% in 1990. Because they begin at lower earnings rates, immigrant cohorts arriving after 1970 are not expected to fully assimilate to higher native earnings rates.

Borjas (1987, 1994) provides a framework for understanding these changes in immigrants' labor market outcomes over time. He adapts a version of the Roy (1951) model to immigrant self-selection. Under the assumption that migration costs are the same across individuals, he shows that low-skilled individuals will have an incentive to migrate to countries where the distribution of earnings is more equal than in their own country. As U.S. immigration policy changed, allowing more individuals from countries with very unequal earnings

distributions, the type of individuals who find it in their best interest to migrate also changed.[2] While immigrants from countries with relatively compressed earnings distributions would tend to come from the upper end of the skills distribution, immigrants from Mexico, for example, would tend to come from the lower end of the wage distribution, as those with high levels of skill would prefer the high wages from the relatively unequal wage distribution in Mexico.

The economic model of crime (Becker 1968), or other one-dimensional models of skill, would predict that more recent immigrants would be increasingly likely to have poor social outcomes as the converse of their low wages. As we will show, more recent immigrants actually have better realizations of our measure of criminal activity than other groups. There are different ways that this may be interpreted in the Roy model context. First, as the U.S. wage distribution became more unequal in the 1980s and 1990s, it may have led to a change in the type of immigrant who selects to migrate. Interestingly, Borjas and Friedberg (2006) show that the cohort of immigrants who came between 1995 and 2000 halted the trend of declining relative wages for new immigrant cohorts. Some of this turnaround may be attributed to an influx of highly-skilled engineers and computer scientists attracted by the labor market in the 1990s. Thus, we might expect a change in the social outcomes of these new immigrants that mirrors this improvement in their labor market outcomes.

Second, however, small changes in the model of self-selection alter the predictions about immigrant self-selection. Chiquiar and Hanson (2005) show that if one allows migration costs to decline with skills, then the Roy model can predict "intermediate" selection. They find evidence

[2] See Borjas (1985, 1995) for empirical evidence on changes in immigrants' earnings over time.

that Mexicans who come to the U.S. are from the middle to upper part of the observable skill distribution compared with Mexicans who remain in Mexico. [3]

If we assume that migration costs vary with the quality of one's social network, such that migration costs are lower when one has strong connections to those who are succeeding in the new country (Chiquiar and Hanson 2005; Hanson 2007), then those with more productive social networks will require a lower wage premium to induce migration. This may create a situation such that immigrants with low-wages and observable skills, may nonetheless have good social outcomes. Policy changes in the 1980s and 1990s reducing access to welfare for immigrants, increasing criminal penalties in general, and increasing penalties for criminal noncitizens in particular, would act to reinforce a process that selected immigrants with relatively good social outcomes. Because the process of immigration may peel apart different dimensions of "skill," how immigrants compare to natives in social domains cannot be simply deduced from evidence on wages.

Comparison of Immigrant and Native born Institutionalization Rates across Three Decades

We use data from the 5% Public Use Microsamples of the U.S. Census in 1980, 1990, and 2000 to examine institutionalization rates for men ages 18–40. The 1980 Census identifies the incarcerated among the institutionalized, but later Censuses do not. For men aged 18–40, in 1980 70% of the institutionalized are incarcerated, with the remainder in mental institutions, hospitals, drug treatment centers, and long-term care facilities. Butcher and Piehl (1998b)

[3] Chiquiar and Hanson (2005) examine the question of immigrant self-selection from Mexico using Mexican and U.S. Census data from 1990 and 2000. Contrary to what one might expect from the Borjas-Roy model, they find that Mexican immigrants in the U.S. tend to be selected from the middle to upper part of the observable skill distribution compared with Mexicans who remain in Mexico. Ibarraran and Lubotsky (2007), on the other hand, find that households that report having members who have emigrated to the U.S. tend to be selected from the lower part of the observable skill distribution. The difference between these likely arises because Ibarraran and Lubotsky's methodology should pick up those individuals who are undercounted in the U.S. Census—young, low-skilled, single men. We will address the undercount issue in the context of institutionalization below. Ibarraran and Lubotsky also suggest that education among Mexican immigrants in the U.S. is likely to be misreported.

demonstrates that limiting the 1980 analysis to only those who are incarcerated does not substantively change the results.[4]

Before we proceed, a note about our definition of "immigrant." In many contexts, the law distinguishes between the foreign born who intend to become permanent residents and those who are more transient. For example, permanent resident aliens typically have the right to work in the U.S., while those on a student visa do not. In these data, however, we do not have information on visa status, and "immigrant" is equivalent to "foreign born."[5] The important distinction in terms of the legal treatment of criminal aliens is between immigrants who have naturalized and those who have not, since the latter are subject to deportation. Later in the study we will pay close attention to citizenship status and how this has changed over time.

The immigrant population in the United States rose substantially between 1980 and 2000. Figure 1 shows the fraction immigrant inside and outside institutions for 18–40-year-old men for all three censuses. The fraction immigrant outside institutions nearly tripled over this period, rising from around 6% to 17% of the population. The fraction immigrant inside institutions, on the other hand, actually fell between 1990 and 2000, despite the increase in the overall immigrant population.

Table 1 presents summary statistics for demographic characteristics by nativity.[6] As well-documented elsewhere, immigrant and native-born populations differ greatly in education and race/ethnicity distributions. Between 1980 and 2000, the fraction of the native-born population with some college education or more rose 25 percent, while among immigrants this

[4] Expansions of the prison population since 1980 likely increased the proportion in our sample of the institutionalized who are incarcerated.

[5] We omit those born in outlying areas of the United States and those born abroad to U.S. citizens.

[6] Throughout the paper, we reported estimates using the person weight reported by the Census (there are no weights in 1980).

group held steady. Similarly, the fraction of the immigrant population that is Hispanic rose substantially over this period; by 2000, nearly 60 percent of all immigrants describe themselves as Hispanic.

Table 2 reports the institutionalization rates by nativity and other demographic characteristics. Overall, institutionalization rates more than doubled over the period of study, from 1.3% to 3.0% of young men at a point in time. These unadjusted rates are much lower for the foreign born than for the native born in all three Census years. In addition, institutionalization is strongly correlated with educational attainment, race, and ethnicity.

Because among the native born, those with low levels of education and Hispanics are more likely to be institutionalized than the average person, one might expect high institutionalization rates among immigrants. Figure 2 reports the actual institutionalization rates of the native and foreign born, and then those we expect to see among immigrants based on the institutionalization propensities of the native born.[7] Simply predicting institutionalization for immigrants based on their ages and native-born institutionalization propensities in 1980 gives an average predicted institutionalization rate of 0.013—up from their actual rate of 0.004, and equal to the native born. The effect for 1990 and 2000 is similar: predictions based on age-institutionalization relationship give immigrants institutionalization rates similar to those of natives. This shows that differences in the age distribution are not responsible for the large gap in observed institutionalization rates. The final bar in Figure 2 for each year represents predictions based on age, education, race, and ethnicity. In 2000, this model predicts an institutionalization rate for immigrants of 0.073 -- double that of the native born, and 10 times the actual rate for immigrants. Clearly, immigrants have characteristics that in the native-born

[7] These calculations come from running logits on a sample of the native born only and then using the estimated coefficients to predict institutionalization for immigrants.

population are highly correlated with institutionalization. This exercise reveals just how low the observed rates of institutionalization for immigrants are, considering their lower educational attainment and other characteristics.

Figure 3 shows the relationship between age and institutionalization for the native born and the most recent immigrants for each of the three Census years. For native-born Americans, the age-institutionalization curve peaks in the early twenties and gradually falls off (in a pattern well-known to criminologists). Institutionalization rates rose each decade for all age groups. In contrast, a dramatic feature of the graph is the relatively low rate for recent immigrants (those who arrived within the last five years in each Census). One possible explanation is that immigrants are not likely to be institutionalized before several years of exposure to the U.S. criminal justice system, and recent immigrants have not accumulated enough experience (to begin criminal enterprises, to be caught by law enforcement, or to have cases processed through the system). This explanation may also explain the relatively linear relationship between age and institutionalization among immigrants. If we set aside this "exposure time" hypothesis (which we explore in a later section), it is clear that recent immigrants have not seen increases in institutionalization comparable to those of natives, and, in fact, appear to have reduced institutionalization from 1990 to 2000.

The main result from these tables and figures is that immigrants have very low institutionalization rates, despite characteristics that in the native population are highly correlated with institutionalization. Furthermore, their institutionalization rates relative to the native born fell between 1990 and 2000. In the next section, we discuss the reliability of using these results to draw inferences about how institutionalization and criminal propensities have changed over time.

Enumeration

Our institutionalization rates are measures of the number of individuals in institutions divided by the number of individuals overall. Mismeasurement of either the numerator or the denominator would result in poor estimates of institutionalization rates. For the analyses we will conduct, we are particularly concerned with whether any mismeasurement differs systematically between the native-born and immigrant populations, and whether mismeasurement of the institutionalized population changes over time.

A. The Numerator

The institutionalized population is a subset of the "special populations" category in the U.S. Census. The Census has separate questionnaires and procedures for those housed in group quarters, including institutions. Many of those living in institutions, including prisons and jails, are deemed unable to fill out their own questionnaires, in which case Census enumerators fill out these forms over several weeks using administrative data. (See the data appendix for a more detailed description of Census enumeration procedures in special populations.) Thus, the Census records for the incarcerated population should be as good as the administrative data on which they are based. Administrators of prisons and jails have large incentives to accurately count their inmates, as keeping track of inmates is their foremost responsibility. And administrative data are verified during the processing of criminal cases. Thus, we would expect this population to be accurately counted relative to the rest of the population.

The next question is whether the counting of immigrants and the native born differs systematically, and whether any differences changed over time. While all the foreign born may not be properly identified, criminal justice administrators have incentives to identify them, particularly those who are not citizens. The incentives to identify noncitizen aliens increased

over this period, as the federal government became more active in managing criminal aliens (Butcher and Piehl 2000; Legomsky 1999). Thus, if anything, we would expect any undercount of institutionalized immigrants to decrease relative to the native born over the time period. Such a change in mismeasurement would be expected to increase the measured institutionalization rate of immigrants relative to the native born.

We have no reason to believe that the mismeasurement in the institutionalized population changed over the period considered in this study. Differences in enumeration practice are more likely to have occurred for the noninstitutionalized than for the institutionalized. We turn to that population now.

B. The Denominator

A second source of mismeasurement comes from the "undercount" of the overall population. The undercount arises when the Census does not enumerate some individuals. This is thought to be more likely in certain populations, particularly those that are more likely to be transient. The 2000 Census is widely reputed to have improved the undercount problem relative to the 1990 Census. We are not necessarily concerned if the undercount improved for all populations in the same way. However, if immigrants are more likely to be undercounted than the general population, and the undercount improved for them, we could find a spurious decrease in the institutionalization rates of immigrants between 1990 and 2000 relative to the native born.

The data appendix provides more details on the potential impact of changes in the undercount on our estimates of immigrant institutionalization rates. Our simulations show that our estimates are unlikely to be driven purely by changes in the undercount. Suppose that the institutionalization rate for immigrants in 1990 and 2000 was actually the same, but the 1990 Census differentially miscounted immigrants in the civilian population. For changes in the

undercount to generate the estimates of institutionalization observed in the data (see Table 2), the undercount of immigrants relative to the native born would have to be 37:1 (that is, the Census missed 37 immigrants for every 1 missed native) in the 1990 Census.

Incarceration and Its Relationship to Crime Commission

A second important question is whether we can use institutionalization rates to make inferences about immigrants' commission of crime in the United States. If Census measures of institutionalization are poor measures of true incarceration rates, then these measures would not tell us much about how immigration affects public safety. Alternatively, if immigrants are less likely than the native born to be caught for a given criminal act (for example, because they evade police by slipping across the border), immigrants will have lower institutionalization rates. However, areas with large immigrant populations will have higher crime rates.[8]

We can use information on Metropolitan Area (MA) crime rates and immigrant density to provide some evidence on the relationship between immigration and crime.[9] Figure 4 shows the change in MA crime rates graphed against the change in fraction immigrant for 1990 to 2000 for the 20 largest MAs.[10] Areas with the largest increases in their fraction immigrant had the largest decreases in their crime rates. This analysis corroborates the results from the individual-level

[8] Some might argue that immigrants are less likely to report crimes. The nationally representative study of criminal victimization does not allow analysis of reporting rates by immigrant status. However, there is no consistent pattern between Hispanics and non-Hispanics regarding the propensity to report crimes to police (Catalano 2005).

[9] There is a sizeable literature on the labor market impacts of immigration that uses cross-MA variation to examine the issue. Many of the concerns around interpretation there also pertain for the impact of immigration on crime rates. See Card (2001) and Borjas (2003) for examples and discussion.

[10] Data are from the Uniform Crime Reports (see table notes for details). Four of the 24 largest MAs had to be dropped because data were unavailable.

Census data reported here: immigrants' criminality improved relative to that of the native born between 1990 and 2000.[11]

What Explains the Low Institutionalization Rates of Immigrants?

Having established that immigrants have low institutionalization rates relative to the native born in the U.S., we now turn to the potential causes. We begin this section with a baseline multivariate model. Then we discuss the competing hypotheses and our empirical strategy for testing them.

A. Baseline Specification

In Table 3 we present the results of logit models to explain institutionalization, with all three Censuses stacked together.[12] Each specification includes a full set of age dummies, year dummies, and controls for the length of time in the U.S. for immigrants, as in equation (1) below:

$$(1) \quad \begin{aligned} I_i = {} & \alpha + \beta_1 (c_{96-00}) + \beta_2 (c_{91-95}) + \beta_3 (c_{85-90}) + \ldots + \beta_{10} (c_{40-50}) \\ & + \delta_1 (1980) + \delta_2 (1990) + \gamma_1 (yearsUS) + \gamma_2 (yearsUS^2) + \varphi(X) + \varepsilon \end{aligned}$$

The table reports the marginal effects evaluated at the sample means, not the logit coefficients.

The first model in Table 3 shows that immigrants have a 2.5-percentage-point lower probability of being institutionalized, controlling for year, age, and time in the U.S. As the length of time immigrants are in the U.S. grows, institutionalization rates fall—the opposite of assimilation. Column (1) shows that the immigrant effect varies by cohort, with more recent cohorts having lower institutionalization rates (relative to natives) than earlier cohorts. Note that some cohorts appeared in just one or two of the Census years, and that time in the U.S. is highly

[11] Other researchers have documented a non-relationship between immigration and crime: Butcher and Piehl (1998a), which analyzed changes in city crime rates and immigration patterns between 1980 and 1990, Hagan and Palloni (1999), which compared crime rates of border to non-border cities, and Martinez and Rosenfeld (2001), which compared crimes rates at the neighborhood level in Miami, El Paso, and San Diego.

correlated with cohort, especially because of the age limitations on our sample.[13] In later specifications, we free up the coefficients to vary by Census year. In those specifications, the meaning of a particular cohort is clearer. For now, it is enough to note these issues affect interpretation of the estimates.

Model (2) adds education, model (3) further adds race and ethnicity, and model (4) adds an indicator of U.S. citizenship (for the foreign born) to the earlier controls. All these variables are strongly statistically significant predictors of institutionalization. In particular, the addition of education attenuates all the measured effects in the earlier models, including the cohort indicators and year dummies. The main qualitative description of the cohort pattern persists, though with a more modest quantitative difference between immigrants and the native born.

Lubotsky (2000) points out that the Census may misclassify reentering immigrants as recent arrivals. Indeed, he finds that many of the studies focusing on immigrant wage assimilation overstate the secular decline in earnings across immigrant cohorts, because these mostly low-wage entrants are misclassified as "recent immigrants." It is less clear how this misclassification may affect our results. If some in the "recent immigrant" category are these reentrants with very low skills, then we might expect, as a corollary to the wage studies, to find this group more likely to be incarcerated. This would work against our finding that recent immigrants have particularly low incarceration rates. On the other hand, the fact that they are reentrants may suggest a certain fluidity of movement that allows them to escape detection, and thus to have lower institutionalization rates for a given level of criminal activity. However, if this

[12] To estimate this model, we randomly dropped 75% of the native born and adjusted the weights accordingly.

[13] Age-at-arrival will also systematically vary across cohorts since the people who arrived in earlier cohorts would on average have had to arrive younger in order to still be under 40 and in our sample. Friedberg (1992) shows the importance of age-at-arrival for labor market outcomes. Here, we find substantively similar results if we split the sample between those who arrived as young children (under 12) and those who arrived at older ages.

were the case, we would expect that an increase in the immigrant population would increase crime rates, and Figure 4 shows the opposite.

Another source of misclassification may come from efforts by the Census Bureau to fill in missing information. If the Bureau is more likely to allocate incorrect data to immigrants than to the native born, that might affect our results. There is evidence that immigrants are more likely to have allocated education data than are the native born, for example (Ibarraran and Lubotsky 2007). In our sample, immigrants are one and a half times as likely as natives to have any of our variables of interest allocated (22% versus 14%). Of particular concern is that 14% of our sample immigrants in 2000 have their year of arrival allocated (the rate is under 6% in 1990 and 1980). We have reestimated our model after dropping all observations with any allocated variables and find qualitatively similar results, though the estimated cohort effects are less negative.[14] From here forward we rely on the full sample for analysis. [15]

B. Three Hypotheses

We hypothesize three types of causes of the institutionalization patterns documented above: deportation, deterrence, and selection. Changes in the policies toward criminals during the 1980s and 1990s, toward criminal noncitizens in particular in the 1990s, and toward those who were likely to be less successful in the labor market in the 1980s and 1990s may have

[14] Results available upon request.

[15] We also examined how sensitive the results are to the choice of where to evaluate the marginal effects. The non-linear nature of the logit means that the marginal effects may differ, depending on where along the function they are calculated. Appendix Table 2 presents estimates analogous to those in Table 3 evaluated for a 25-year-old Hispanic with a high school degree. The estimates are qualitatively similar to those in Table 3—namely, that the more recent cohorts have lower institutionalization rates than earlier cohorts, and that there is no evidence that immigrants are assimilating toward the higher institutionalization rates of the native born with time in the country. However, the marginal effects are nearly twice as large, suggesting even lower institutionalization rates for immigrants. Note that when we evaluate at the mean of the sample, we are comparing immigrants to the average native, who has an ever-higher education level. When we evaluate the results for a 25-year-old Hispanic with a high school degree, we are comparing institutionalization rates for immigrants and natives with those characteristics. We continue with the more conservative estimate, evaluating the logits at the sample mean, but we note that our comparisons are much larger when we evaluate at the average characteristics for immigrants.

operated through all three mechanisms to dramatically reduce incarceration rates among immigrants by 2000. Penalties for criminal activity tightened in the 1980s and 1990s (Travis 2005). The Violent Crime Act of 1994 and the Anti-Terrorism and Effective Death Penalty Act of 1996 expanded the list of crimes for which noncitizen immigrants can be deported (Legomsky 1999), and applied this change retroactively to noncitizen immigrants. This greater emphasis on deportation may have mechanically decreased the probability that a criminal alien would be incarcerated. Because noncitizens must complete their sentences before being deported, the higher probability of deportation acts as an additional punishment. The increase in punishment may have deterred some immigrants from committing crimes in the U.S.

The Personal Responsibility and Work Reconciliation Act of 1996 (welfare reform) also made it more difficult for noncitizens to receive welfare benefits.[16] This, combined with the added punishment for criminal noncitizens, may have given immigrants with poor social outcomes an incentive to naturalize to obtain the protection citizenship provides.[17] Anecdotes at the time suggested that immigrants were lining up to apply for citizenship as the atmosphere changed in the mid-1990s.

Alternatively, these policy changes, coupled with economic conditions attractive to high-skilled immigrants, may have undercut the attraction of the U.S. to potential immigrants likely to interact with the criminal justice or welfare systems. Thus, these policy changes may have had a deterrent effect on the intensive margin, changing the behavior of immigrants already in the U.S.,

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[16] As originally passed, the welfare reform bill barred non-naturalized immigrants from receipt of most forms of welfare; as revised, only immigrants who arrived after the law are subject to the ban. States had the option to use state funds to extend benefits to immigrants left out of the federal statute. Many, especially those with large immigrant populations chose to do so.

[17] In general, the better-off immigrants become citizens. Fix et al. (2003) reports that immigrants with the least English language proficiency, lower education, and lower incomes are less likely to become naturalized citizens.

and on the extensive margin, changing the pool of self-selected immigrants to a less criminally active one.

Changes in policies toward criminals, immigrants, and criminal immigrants during the 1980s and 1990s may have operated through all three of these mechanisms: deportation, deterrence, and selection. Although the mechanisms are not mutually exclusive, they generate testable implications that allow distinctions among them.

Results

To test these hypotheses, we need estimates of institutionalization of cohorts of immigrants over time, allowing estimated cohort effects to vary by period. For the remainder of the analyses, therefore, we run separate logits for each Census year, as shown in equations (2) and (3) for 1990 and 2000, respectively:

(2) $$I_i = a^{90} + b_3^{90}\left(c_{85\text{-}90}\right)_i + \ldots + b_9^{90}\left(c_{50\text{-}59}\right)_i + \varphi^{90}\left(X\right)_i + \varepsilon_i^{90}$$

(3) $$I_i = a^{00} + b_1^{00}\left(c_{96\text{-}00}\right)_i + b_2^{00}\left(c_{91\text{-}95}\right)_i + b_3^{00}\left(c_{85\text{-}90}\right)_i + \ldots + b_8^{00}\left(c_{60\text{-}64}\right)_i + \varphi^{00}\left(X\right)_i + \varepsilon_i^{00}$$

Because each equation is limited to a single cross section, we can no longer identify the effect of time in the country separately from cohort of entry. Another difference from the stacked regressions shown earlier is that all coefficients vary freely by year. Appendix Table 3 reports marginal effects of the year-by-year results for 1990 and 2000 for easy comparison to the stacked regressions reported in Table 3. The dominant features from the earlier approach—that immigrants have lower institutionalization rates, and more recent cohorts have lower rates than earlier cohorts—carry through to the new modeling approach.

The top panel of Table 4 reports estimates of how the institutionalization of immigrant cohorts changed from one Census to the next. For example, column (1) shows the difference between the marginal effect of b_3^{00} and b_3^{90}, with a negative number indicating that the (negative)

gap between that immigrant cohort and the native born widened. That is, the first entry in the table means that the gap between the institutionalization of the cohort arriving between 1985 and 1990 and natives grew more negative by 0.0074 between 1990 and 2000. This represents an enormous shift compared with the base institutionalization rate among natives in 1990 (0.022), or among that immigrant cohort in 1990 (0.007).

The rest of the columns in the table contain the results of adding further control variables. As in the earlier table, column (1) controls only for age, column (2) adds education controls, column (3) adds race and ethnicity, and column (4) adds citizenship to all the previous controls. Between 1990 and 2000, all the cohorts decreased their relative institutionalization rates, regardless of which controls are included. For example, these estimates suggest a 0.36–0.86-percentage-point decline in relative institutionalization for the 1985–1990 and 1980–1984 cohorts between 1990 and 2000. Between 1980 and 1990, the 1975–1979 and 1970–1974 cohorts decreased their relative institutionalization rates once education is included in the controls. Immigrants appear to have improved relative to natives with time in the country, and this improvement was greater from 1990 to 2000 than it was from 1980 to 1990.

These results are consistent with a mechanical decrease in the institutionalized population of immigrants due to deportation, which would have had a bigger effect from 1990 to 2000 than from 1980 to 1990. However, the results are also consistent with a deterrent effect due to increased penalties for criminal activity that either selects immigrants with lower criminality or to which immigrants are more responsive than the average native. We examine each of these explanations for the cohort pattern in our results in turn.

A. Deportation

First, we consider the mechanical effect of deportation on incarceration. The number of immigrants deported (both voluntary departures and formal removals) rose over the three decades we examine. From 1971 to 1980, about 7.5 million immigrants were expelled (Yearbook of Immigration Statistics 2002); from 1981 to 1990, about 10.2 million immigrants were expelled; and from 1991 to 2000, about 14.5 million immigrants were expelled (Immigration Statistics Reports 2002).[18] Among those deported, not simply excluded, the most common administrative reasons given during the 1990s were "attempted entry without proper documents" (35%) and "criminal activity" (31%).

This increase in deportation might be expected to decrease immigrants' relative institutionalization rates.[19] However, the countervailing effects of this policy may increase the probability that an immigrant will be institutionalized. First, the Anti-Terrorism and Effective Death Penalty Act increased the list of criminal acts for which noncitizens must be detained. Legomsky (1999) reports that "mandatory detention now applies to almost all noncitizens who are inadmissible or deportable on crime-related grounds—not just to those convicted of aggravated felonies (p. 532)." Second, the speed of removal of deportable aliens may critically affect immigrants' institutionalization rates. Shuck and Williams (1999) estimate that the Immigration and Naturalization Service (INS) had removed "fewer than twenty percent of the nearly 300,000 criminal aliens estimated to be already under law enforcement supervision."

[18] The vast majority of these expulsions are voluntary departures. For example, from 1991 to 2000, only 939,749 of the expulsions were formal removals.

[19] Note that because immigrants serve their sentences and then are deported, deportation should not decrease incarceration probabilities for a given offence, but only for subsequent offences, since immigrants would no longer be in the country to commit these offences. However, even that may be in doubt. A recent *New York Times* investigation reported that city sanctuary policies, such as the one in Los Angeles that prohibits police from inquiring about immigration status unless there is a formal charge of a crime, mean that those who have been deported can frequently return to the U.S. and resume their lives (LeDuff 2005).

Butcher and Piehl (2000) showed that immigrants under a deportation order spent more time incarcerated for a given sentence than similar natives—perhaps because backlogs in the system meant that immigrants awaiting deportation waited in prison until the INS could clear their cases. These effects would tend to inflate immigrants' relative incarceration rates.

We would like to know the extent to which the added emphasis on deportation in the 1990s influenced estimated institutionalization rates of immigrants. Secondary sources on the number of immigrants deported are of little use because although deportation increased, so did the population of immigrants. Thus, we take a different approach, beginning with the fact that naturalized citizens are not subject to deportation. If growing deportation of criminal aliens represents an important risk to immigrants, we would expect to see differences in the institutionalization of citizen and noncitizen immigrants in the 2000 Census, as the mechanical impact of increasing deportation must occur for all the noncitizens. We use the sample of citizens (native born and naturalized) to examine how institutionalization patterns changed over time for immigrants who are not subject to the greater threat of deportation from legislation enacted in the 1990s.

The second panel in Table 4 shows the results of this exercise. Here we see that the decline in relative institutionalization rates is at least as large for immigrants who cannot be deported as for immigrants overall. Thus, the decline in the relative institutionalization rates in the top panel of Table 4 cannot all be driven by deportation.

This test is predicated on the assumption that a great shift in naturalization over this period did not occur. If immigrants who were criminally active became more likely to naturalize, then this could inflate the relative incarceration rates of citizen immigrants. Because the relative incarceration rates for naturalized citizens fell by more than for immigrants as a whole, this

seems unlikely. However, we examined rates of naturalization directly to see whether the policy changes in the 1990s that would seem to give immigrants an incentive to naturalize had a detectable effect. Table 1 shows the fraction of immigrants who are citizens in each cohort. Not surprisingly, the fraction citizen is higher in earlier cohorts. However, the probability of naturalizing for a cohort with a given number of years in the U.S. did not rise between 1990 and 2000. For example, 7% of immigrants who had been in the country for less than 5 years in 1990 were citizens; in 2000, 4% of recent arrivals were citizens. This pattern holds up when we run logit models that control for changes in immigrant characteristics as well.[20]

B. Deterrence

The 1980s and 1990s saw the adoption of many policies that increased criminal penalties and thus had the potential to deter criminal activity of both immigrants and the native born. If migration selects individuals who are particularly responsive to incentives, we might see more of a deterrent effect for this group than for the native born, on average. If immigrants are responding to this general deterrence, we would expect to see naturalized citizens reducing their incarceration probabilities as well, not simply noncitizens who face the increased threat of deportation. As we saw in the second panel of Table 4, naturalized citizens reduced their incarceration rates relative to the native born. The effect is larger than for immigrants overall, suggesting immigrants are responding to a deterrent effect from the general increase in punishment, rather than to the specific threat of deportation.

If migration selects individuals who are particularly responsive to incentives, we would further expect that, just like immigrants, native-born migrants -- defined as those who live

[20] Available upon request.

outside their state of birth[21] -- would have reduced their incarceration probabilities over time as well.[22] This is, in fact, what happened. In 1980, native-born movers were 0.04 percent less likely than nonmovers to be institutionalized. By 1990, this difference had risen to 0.2%, and by 2000 to 0.3%.[23]

Native-born movers and immigrants appear to have responded to the harsher criminal penalties in the 1980s and 1990s. The bottom panel of Table 4 compares immigrants to native-born movers, and shows that the decline in immigrants' relative institutionalization rates is much smaller here. This suggests a general deterrence effect, with both immigrants and native-born movers responding to the incentives of new policies toward criminals in similar ways.

C. Changes in Immigrant Selection

Changes in the legal, economic, and social environment during the 1980s and 1990s may have affected the type of immigrant who self-selects to come to the United States.[24] The laws increasing penalties for criminal aliens passed in 1994 and 1996, so we would expect the biggest impact for the cohort arriving between 1996 and 2000. To see whether selection has changed over time, we compare the cohorts who arrived just before the 2000 Census to the outcomes of cohorts who had been in the country a comparable amount of time in previous Censuses. With this strategy, we will see whether the most recent immigrants in 2000 appear more positively

[21] This measure is somewhat problematic since one reason a person may live outside his state of birth is if he is sent to a federal prison in another state. This would tend to increase the institutionalization rates of native born movers. This bias is likely to be small, however, as federal prisoners are a small fraction of all prisoners.

[22] Butcher (1994) compared labor market outcomes for immigrant and native-born blacks, finding that immigrant blacks had better labor market outcomes than the native-born. However, when the native-born who had moved from their state of birth were used as the comparison group, outcomes were very similar, suggesting that movers, whether native-born or immigrant, are similar.

[23] These results come from logits controlling for a full set of age dummies, available upon request. Once education is controlled, native movers have higher institutionalization rates than nonmovers.

[24] Welfare reform and changes in the labor market in the 1990s may also have contributed to the changes in the type of immigrant who came to the U.S. in the 1990s.

selected (conditional on various sets of controls) relative to natives than earlier cohorts, holding constant the time exposed to the U.S. criminal justice system.

Table 5 shows the change in relative institutionalization rates, holding time in the U.S. constant, for the two most recent arrival cohorts. The numbers in the top panel of Table 5 are calculated by subtracting the marginal effects in equations 2 and 3, $b_1^{00} - b_3^{90}$, for example.[25] This comparison limits the bias that may arise from selective return migration—the most recently arrived cohorts will not have lost as many return migrants as those who have been in the U.S. longer. The results show that the most recently arrived cohort in 2000 was 1.1 percentage points less likely to be institutionalized than the most recently arrived cohort was in 1990 (column 1, top panel). The fact that the results are similar when we limit the sample to citizens again suggests that increased deportation is not mechanically driving this result.

Although the evidence in Table 5 suggests that the type of immigrant who came to the U.S. was changing over these decades toward those who were less crime-prone, there is less evidence for a specific effect of the legislation passed in the mid 1990s. For example, the decrease in the relative incarceration rate is as big for the cohort arriving 5–10 years ago as for those with fewer than 5 years in the U.S.[26]

The comparison to native-born movers, as before, shows changes that are smaller in magnitude, because native-born movers have lower incarceration rates than the rest of the native born. Again, there does not appear to be a particularly large effect for the cohort of immigrants whose decision should have been most affected by the harsher environment toward noncitizens in the U.S. beginning in the mid-1990s.

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[25] See Appendix Table 3 for the marginal effects from which these numbers are calculated.

[26] The change in the gap is similar for those arriving 10 to 15 years before each Census. The comparison is not shown here, but may be calculated from Appendix Table 3.

It is quite possible that those who arrived most recently are most likely to be undocumented, giving them the greatest incentive to avoid contact with law enforcement for even minor offenses, as such contact is likely to increase the likelihood that their illegal status is revealed. It is not possible to distinguish the documented from undocumented in the Census in order to test for such an effect. Moreover, it is likely that the most recently arrived in 2000 are even more likely to be undocumented than the recent arrivals in 1990 were, given the large flows of immigrants in the 1990s and the possibility that the Immigration Reform and Control Act (IRCA) amnesty may have allowed those who arrived in the late 1980s to adjust their status. If this is the case, then it offers another potential explanation for the particularly good outcomes of the recent arrivals in 2000.

Robustness

Finally, we assess the sensitivity of the substantive conclusions to the choice of the reference group. Over the study period, the incarceration of natives greatly increased, but this increase was not shared evenly across demographic groups. Because the analysis emphasizes *changes*, it might be particularly important to have a comparison group that is comparable in levels in the pre period.

Limiting the reference native population to those with lower incarceration rates leads, naturally, to a reduction in the extent to which immigrants get relatively better over time. However, the magnitudes are not much smaller than those in the main models when the specifications include control variables. For example, restricting the native reference group to the racial/ethnic group with the lowest institutionalization rate, white non-Hispanics, yields no change in the qualitative conclusions.

Alternatively, it is possible that a better estimate would result from limiting both groups to provide a more homogeneous comparison. Limiting both groups to dropouts, again the original results carry through. In contrast to the above robustness check, in this case the differences in levels within Census are huge. But just as above, once we turn to the changes over time, shifting the base group does not change the qualitative conclusions of this paper.

Conclusion

Using the 1980, 1990, and 2000 Censuses, we show that 18-40 year-old male immigrants have lower institutionalization rates than the native born in each year. The gap in these institutionalization rates widens over the decades, and by 2000 immigrants have institutionalization rates that are one-fifth those of the native born.

The fact that immigrants have lower relative incarceration rates in 2000 than in earlier years may be due to several factors. Legislation passed in the 1980s and 1990s raised penalties for criminal activity; and legislation passed in the 1990s increased penalties for criminal noncitizens in particular. The 1990s legislation may have increased the probability that immigrants are deported for their crimes, thus mechanically lowering their incarceration rates. Alternatively, immigrants' criminal behavior may have changed in response to the general deterrence from increased criminal penalties. Or, the type of person choosing to immigrate to the U.S. may have changed in response to these general increases in penalties for criminal activity, or in response to the specific penalties (deportation after serving one's sentence) that apply to noncitizens. We exploit the fact that some of these effects will pertain for some groups and not others to distinguish among these explanations.

We find that deportation is not driving the decline in relative institutionalization rates of immigrants, because naturalized citizens, who are not subject to deportation, also reduced their

relative institutionalization rates. There is evidence that the process of migration selects individuals who have lower criminal propensity or are more responsive to deterrent effects than the average native. Similar to the foreign born, the native born who live outside their state of birth also reduced their relative institutionalization rates over time. Immigrants who were already in the country reduced their relative institutionalization probability over the decades; and the newly arrived immigrants in the 1980s and 1990s seem to be particularly unlikely to be involved in criminal activity, consistent with increasingly positive selection along this dimension. However, we find little evidence of a dramatic change in the 1996–2000 cohort, the only cohort for which the decision to migrate may have been affected by the increased penalties specific to criminal aliens.

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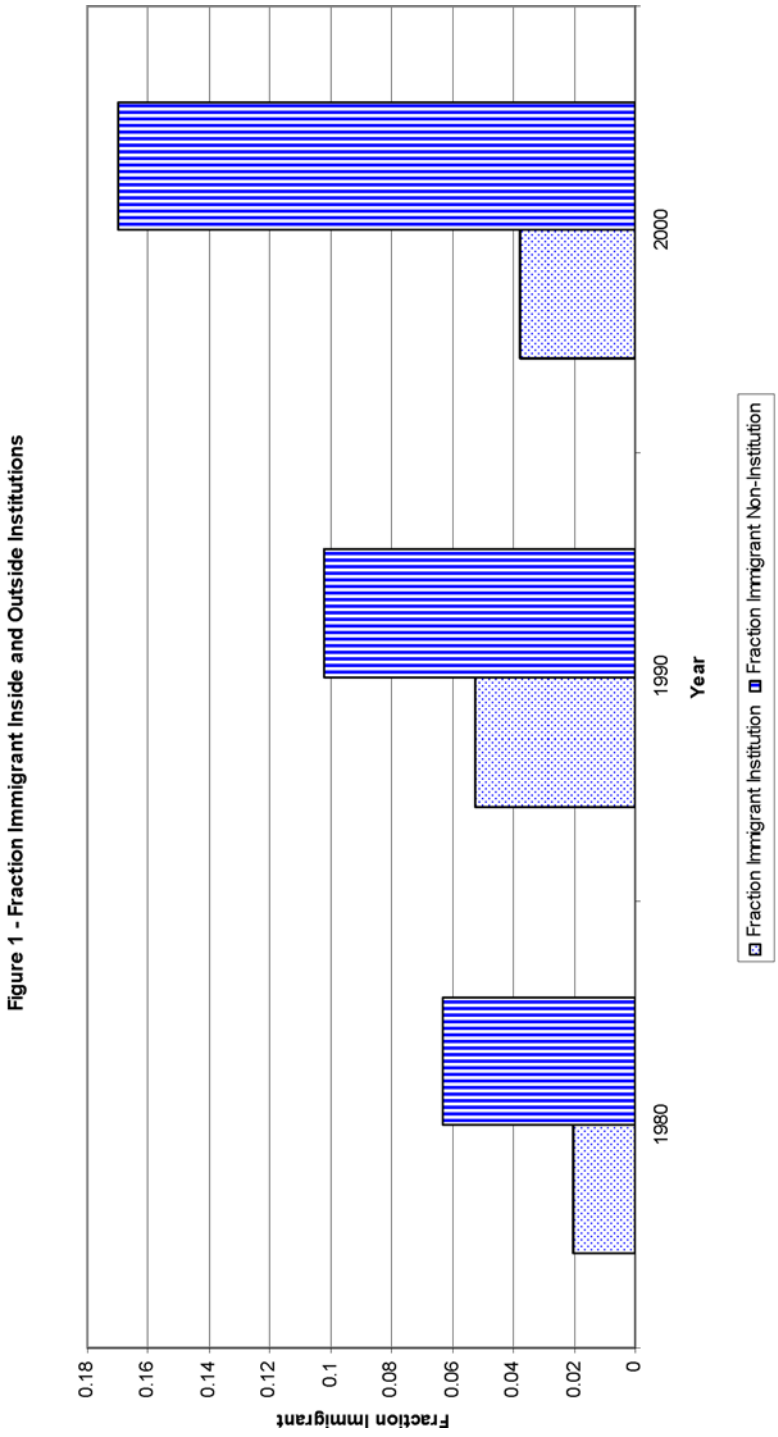
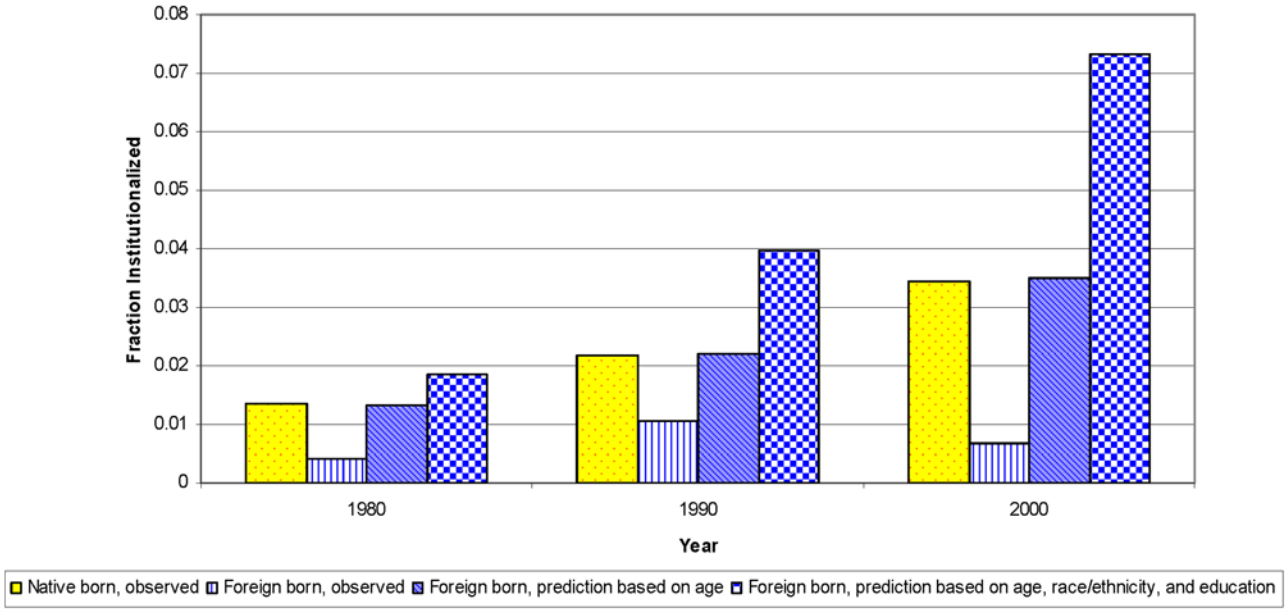


Figure 1 - Fraction Immigrant Inside and Outside Institutions
Fraction Immigrant
0.18
0.16
0.14
0.12
0.1
0.08
0.06
0.04
0.02
0
1980
1990
2000
Year
Fraction Immigrant Institution
Fraction Immigrant Non-Institution

Figure 2: Actual and Predicted Institutionalization Rates



Notes: These numbers are calculated from logit regressions using the 5% Public Use Microdata Samples of the U.S. Census. Predictions are based on the institutionalization probabilities of natives. They are calculated by running the logits for natives alone and predicting immigrant institutionalization rates using these coefficients and the characteristics of immigrants. Controls include a full set of age dummies for the first prediction, and add dichotomous variables for black, Asian, other race, Hispanic origin, high school dropout, high school degree, and some college for the second prediction.

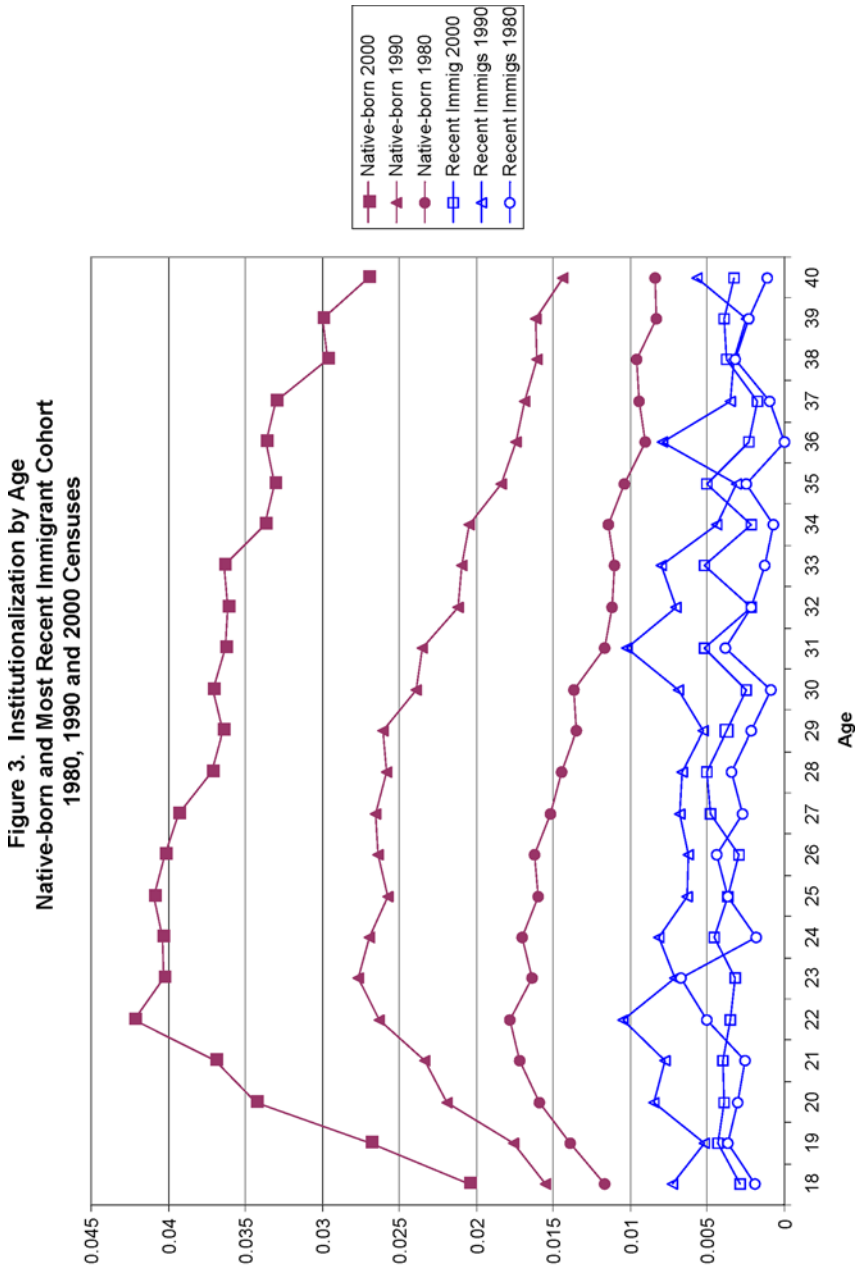


Figure 3. Institutionalization by Age
Native-born and Most Recent Immigrant Cohort
1980, 1990 and 2000 Censuses

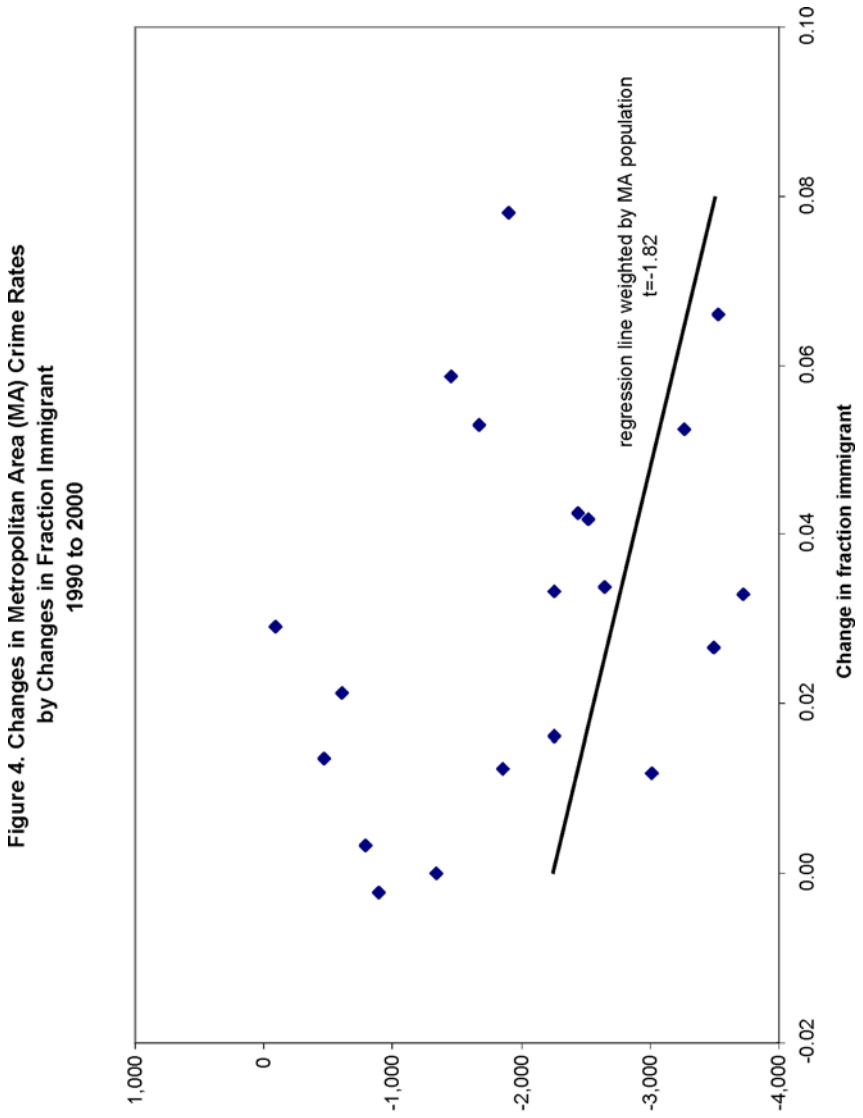


Figure 4. Changes in Metropolitan Area (MA) Crime Rates
by Changes in Fraction Immigrant
1990 to 2000
regression line weighted by MA population
t=-1.82
Change in fraction immigrant
1,000
0
-1,000
-2,000
-3,000
-4,000
-0.02
0.00
0.02
0.04
0.06
0.08
0.10

Table 1. **Summary Statistics:**
Characteristics of Immigrants and Natives in 1980, 1990 and 2000
(Standard Errors in Parentheses)

	1980		*1990*		*2000*	
Characteristic	*Native born*	*Immigrants*	*Native born*	*Immigrants*	*Native born*	*Immigrants*
Age	27.834	28.793	29.085	29.280	29.321	29.671
	(0.0047)	(0.0178)	(0.0046)	(0.0137)	(0.0050)	(0.0107)
< H.S.Degree	0.1925	0.3449	0.1268	0.3258	0.1241	0.3396
	(0.0003)	(0.0013)	(0.0002)	(0.0010)	(0.0002)	(0.0008)
H.S.Degree	0.3909	0.2365	0.3545	0.2470	0.3506	0.2693
	(0.0004)	(0.0012)	(0.0003)	(0.0009)	(0.0003)	(0.0007)
Some College	0.2285	0.2029	0.3222	0.2228	0.3256	0.1889
	(0.0003)	(0.0011)	(0.0003)	(0.0009)	(0.0003)	(0.0007)
College Degree	0.1880	0.2157	0.1964	0.2043	0.1997	0.2023
	(0.0003)	(0.0011)	(0.0003)	(0.0009)	(0.0003)	(0.0007)
Black	0.1143	0.0682	0.1243	0.0807	0.1401	0.0719
	(0.0002)	(0.0007)	(0.0002)	(0.0006)	(0.0003)	(0.0004)
White Non-Hispanic	0.8330	0.3421	0.8084	0.1994	0.7631	0.1547
	(0.0003)	(0.0013)	(0.0003)	(0.0009)	(0.0003)	(0.0006)
Asian or Pacific	0.0060	0.1957	0.0082	0.2347	0.0169	0.2198
	(0.0001)	(0.0011)	(0.0001)	(0.0009)	(0.0001)	(0.0007)
Other Race	0.0024	0.0270	0.0005	0.0034	0.0405	0.3400
	(0.0000)	(0.0005)	(0.0000)	(0.0001)	(0.0001)	(0.0008)
Hispanic	0.0405	0.3975	0.0519	0.4977	0.0784	0.5671
	(0.0001)	(0.0014)	(0.0002)	(0.0011)	(0.0002)	(0.0008)
U.S. Citizen	1	0.3306	1	0.2903	1	0.2667
		(0.0013)		(0.0010)		(0.0007)
Citizen: 96-00						0.0445
						(0.0007)
Citizen: 91-95						0.1392
						(0.0012)
Citizen: 85-90				0.0674		0.2991
				(0.0010)		(0.0015)
Citizen: 80-84				0.2388		0.4863
				(0.0018)		(0.0022)
Citizen: 75-79		0.0730		0.3973		0.5874
		(0.0012)		(0.0025)		(0.0031)
Citizen: 70-74		0.2604		0.4771		0.6671
		(0.0025)		(0.0032)		(0.0043)
Citizen: 65-69		0.4345		0.5839		0.7292
		(0.0034)		(0.0044)		(0.0057)
Citizen: 60-64		0.5875		0.6809		0.7667
		(0.0041)		(0.0054)		(0.0100)
Citizen: 50-59		0.7890		0.7699		
		(0.0034)		(0.0057)		
Citizen: 40-49		0.8965				
		(0.0057)				
No. Obs	1,900,112	127,392	1,984,069	209,878	1,875,961	352,534

Notes: These data are from the 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS) of the U.S. Census. The data include men aged 18–40 inclusive. Those born in U.S. outlying areas, born abroad of American parents, or born at sea are excluded from the sample. All means are weighted to reflect sampling.

Table 2. **Fraction of the Population Institutionalized in 1980, 1990 and 2000**
(Standard Errors in Parentheses; Sample Size in Square Brackets)

Group	*1980*		*1990*		*2000*	
			All			
Full Sample	0.0129		0.0206		0.0299	
	(0.00008)		(0.00010)		(0.00011)	
	[2,027,504]		[2,193,947]		[2,228,495]	
			By Immigrant Status			
	1980		*1990*		*2000*	
	Native born	*Immigrants*	*Native born*	*Immigrants*	*Native born*	*Immigrants*
All	0.0135	0.0042	0.0217	0.0107	0.0345	0.0068
	(0.00008)	(0.00018)	(0.00010)	(0.00022)	(0.00013)	(0.00014)
	[1,900,111]	[127,392]	[1,984,069]	[209,878]	[1,875,961]	[352,534]
< H.S Degree	0.0389	0.0076	0.0673	0.0167	0.1104	0.0101
	(0.00032)	(0.0041)	(0.00049)	(0.0048)	(0.00064)	(0.0028)
H.S. Degree	0.0101	0.0041	0.0229	0.0119	0.0412	0.0082
	(0.00011)	(0.00037)	(0.00018)	(0.00048)	(0.00024)	(0.00024)
Some College	0.0069	0.0024	0.0143	0.0082	0.0171	0.0047
	(0.00013)	(0.00030)	(0.00015)	(0.00042)	(0.00017)	(0.00027)
Black	0.0445	0.0078	0.0811	0.0289	0.1132	0.0179
	(0.0004)	(0.00095)	(0.00060)	(0.00142)	(0.00065)	(0.00087)
White Non-Hispanic	0.0088	0.0040	0.0116	0.0052	0.0170	0.0039
	(0.00007)	(0.00030)	(0.00008)	(0.00035)	(0.00011)	(0.00027)
Asian or Pacific	0.0086	0.0011	0.0130	0.0024	0.0253	0.0037
	(0.00087)	(0.00021)	(0.00090)	(0.00022)	(0.00090)	(0.00022)
Hispanic	0.0210	0.0054	0.0396	0.0152	0.0659	0.0079
	(0.00052)	(0.00032)	(0.00062)	(0.00037)	(0.00066)	(0.00020)
U.S. Citizen		0.0055		0.0097		0.0051
		(0.00036)		(0.00040)		(0.00023)
Immigrant Cohorts						
1996-2000						0.0037
						(0.00020)
1991-1995						0.0050
						(0.00025)
1985-1990				0.0068		0.0072
				(0.00032)		(0.00028)
1980-1984				0.0117		0.0106
				(0.00046)		(0.00046)
1975-1979		0.0029		0.0117		0.0096
		(0.00025)		(0.00055)		(0.00061)
1970-1974		0.0036		0.0128		0.0141
		(0.00034)		(0.00072)		(0.00108)
1965-1969		0.0039		0.0172		0.0098
		(0.00043)		(0.00115)		(0.00127)
1960-1964		0.0067		0.0163		0.0183
		(0.00068)		(0.00147)		(0.00309)
1950-1959		0.0065		0.0090		
		(0.00068)		(0.00128)		
1940-1949		0.0089				
		(0.0018)				

Notes: These data are from the 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS) of the U.S. Census. The data include men aged 18–40 inclusive. All means are weighted to reflect sampling.

Table 3. **Marginal Effects for Logit Estimates of Institutionalization**
(Evaluated at Sample Mean, Robust Standard Errors in Parentheses)

	(0)	(1)	(2)	(3)	(4)
Immigrant	-0.0251				
	(0.0005)				
1996-2000		-0.0208	-0.0137	-0.0116	-0.0117
		(0.0003)	(0.0002)	(0.0002)	(0.0002)
1991-1995		-0.0192	-0.0130	-0.0111	-0.0113
		(0.0004)	(0.0002)	(0.0002)	(0.0002)
1985-1990		-0.0162	-0.0118	-0.0105	-0.0108
		(0.0006)	(0.0003)	(0.0002)	(0.0002)
1980-1984		-0.0094	-0.0083	-0.0083	-0.0089
		(0.0011)	(0.0005)	(0.0004)	(0.0004)
1975-1979		-0.0082	-0.0073	-0.0075	-0.0082
		(0.0013)	(0.0007)	(0.0005)	(0.0004)
1970-1974		-0.0012	-0.0031	-0.0052	-0.0062
		(0.0022)	(0.0011)	(0.0007)	(0.0007)
1965-1969		0.0047	0.0022	-0.0018	-0.0032
		(0.0003)	(0.0019)	(0.0012)	(0.0011)
1960-1964		0.0132	0.0102	0.0040	0.0019
		(0.0044)	(0.0031)	(0.0022)	(0.0020)
1950-1959		0.0054	0.0055	0.0028	0.0009
		(0.0050)	(0.0036)	(0.0028)	(0.0025)
1940-1950		0.0229	0.0151	0.0089	0.0067
		(0.0098)	(0.0065)	(0.0048)	(0.0044)
Less than H.S.			0.2227	0.1428	0.1424
			(0.0068)	(0.0050)	(0.0050)
H.S. Degree			0.0594	0.0398	0.0397
			(0.0019)	(0.0014)	(0.0014)
Some College			0.0354	0.0246	0.0246
			(0.0016)	(0.0012)	(0.0012)
Black				0.0397	0.0396
				(0.0008)	(0.0008)
American Indian				0.0088	0.0088
				(0.0011)	(0.0011)
Asian or Pacific				0.0065	0.0066
				(0.0011)	(0.0011)
Other Race				-0.0008	-0.0008
				(0.0005)	(0.0005)
Hispanic				0.0135	0.0134
				(0.0007)	(0.0007)
U.S. Citizen					-0.0023
					(0.0006)
Years in US	-0.0016	-0.0004	-0.0007	-0.0005	-0.0005
	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Years in US^2	-0.00002	-0.000006	-0.000009	0.000008	0.000008
	(0.00001)	(0.00001)	(0.00001)	(0.00000)	(0.00000)
1980	-0.0143	-0.0146	-0.0103	-0.0081	-0.0081
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
1990	-0.0101	-0.0108	-0.0075	-0.0055	-0.0055
	(0.0004)	(0.0004)	(0.0003)	(0.0002)	(0.0002)
Age Dummies	Yes	Yes	Yes	Yes	Yes
Psuedo R-square	0.0193	0.0204	0.1039	0.1614	0.1614

Notes: The marginal effects are calculated at the sample means. Number of observations for 1980 is 2,027,504. Number of observations for 1990 is 2,193,947. Number of observations for 2000 is 2,228,495. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen.

Table 4. **Change in the Relative Institutionalization Rate for Immigrant Arrival Cohorts Compared with the Native Born in 1980, 1990, and 2000 Between Census Estimates[a]**

(Standard Errors in Parentheses)

	1990 to 2000				1980 to 1990			
	(1)	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
				All				
1985-1990 Cohort	-0.0074	-0.0043	-0.0039	-0.0043				
	(0.0005)	(0.0003)	(0.0002)	(0.0003)				
1980-1984 Cohort	-0.0086	-0.0042	-0.0036	-0.0041				
	(0.0006)	(0.0003)	(0.0002)	(0.0003)				
1975-1979 Cohort	-0.0098	-0.0042	-0.0036	-0.0040	0.0009	-0.0016	-0.0012	-0.0018
	(0.0009)	(0.0004)	(0.0003)	(0.0003)	(0.0008)	(0.0003)	(0.0003)	(0.0004)
1970-1974 Cohort	-0.0075	-0.0025	-0.0025	-0.0031	0.0014	-0.0011	-0.0009	-0.0014
	(0.0014)	(0.0006)	(0.0004)	(0.0005)	(0.0011)	(0.0005)	(0.0004)	(0.0004)
				Native Born and Naturalized Citizens Only				
1985-1990 Cohort	-0.0137	-0.0065	-0.0054					
	(0.0014)	(0.0006)	(0.0004)					
1980-1984 Cohort	-0.0140	-0.0072	-0.0060					
	(0.0012)	(0.0006)	(0.0004)					
1975-1979 Cohort	-0.0137	-0.0072	-0.0060		-0.0055	-0.0030	-0.0022	
	(0.0012)	(0.0007)	(0.0005)		(0.0016)	(0.0008)	(0.0006)	
1970-1974 Cohort	-0.0144	-0.0072	-0.0061		-0.0021	-0.0015	-0.0012	
	(0.0017)	(0.0010)	(0.0007)		(0.0017)	(0.0009)	(0.0007)	
				Immigrants Compared with the Native Born Who Live Outside Their State of Birth				
1985-1990 Cohort	-0.0030	-0.0013	-0.0015	-0.0019				
	(0.0005)	(0.0003)	(0.0003)	(0.0003)				
1980-1984 Cohort	-0.0044	-0.0011	-0.0011	-0.0014				
	(0.0006)	(0.0003)	(0.0002)	(0.0003)				
1975-1979 Cohort	-0.0051	-0.0010	-0.0010	-0.0012	0.0017	-0.0010	-0.0008	-0.0013
	(0.0008)	(0.0003)	(0.0003)	(0.0003)	(0.0007)	(0.0003)	(0.0003)	(0.0004)
1970-1974 Cohort	-0.0034	0.0000	-0.0003	-0.0007	0.0019	-0.0007	-0.0006	-0.0010
	(0.0012)	(0.0005)	(0.0004)	(0.0042)	(0.0011)	(0.0004)	(0.0004)	(0.0042)

Notes: These numbers are calculated using the marginal effects calculated from logit estimates run separately for each year; see Appendix Table 3 for the marginal effects from which the top panel numbers are calculated. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen. Standard errors are calculated as for the difference between two means.

[a]Between-Census estimates are calculated by subtracting the probability for a given cohort in the two different Censuses (probability in later census minus probability in earlier census) from logit regressions run separately for each year.

Table 5. **Change in the Relative Institutionalization Rate for Immigrant Arrival Cohorts Compared with the Native Born in 1980, 1990, and 2000**
Constant Exposure Time Estimates[a]
(Standard Errors in Parentheses)

	1990 to 2000				1980 to 1990			
	(1)	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
All								
Fewer than 5	-0.0110	-0.0054	-0.0047	-0.0051	-0.0044	-0.0039	-0.0029	-0.0034
	(0.0005)	(0.0003)	(0.0002)	(0.0003)	(0.0005)	(0.0002)	(0.0002)	(0.0003)
Between 5 and 10	-0.0142	-0.0067	-0.0054	-0.0058	-0.0007	-0.0024	-0.0019	-0.0024
	(0.0006)	(0.0003)	(0.0002)	(0.0003)	(0.0007)	(0.0003)	(0.0002)	(0.0003)
Native Born and Naturalized Citizens Only								
Fewer than 5	-0.0124	-0.0059	-0.0049		-0.0066	-0.0048	-0.0037	
	(0.0016)	(0.0007)	(0.0004)		(0.0018)	(0.0007)	(0.0005)	
Between 5 and 10	-0.0163	-0.0082	-0.0065		-0.0040	-0.0034	-0.0027	
	(0.0011)	(0.0006)	(0.0004)		(0.0013)	(0.0006)	(0.0005)	
Immigrants Compared with the Native Born Who Live Outside Their State of Birth								
Fewer than 5	-0.0065	-0.0025	-0.0025	-0.0029	-0.0036	-0.0032	-0.0026	-0.0031
	(0.0005)	(0.0003)	(0.0003)	(0.0003)	(0.0005)	(0.0003)	(0.0003)	(0.0003)
Between 5 and 10	-0.0095	0.0069	-0.0031	-0.0034	0.0001	-0.0019	-0.0016	-0.0021
	(0.0026)	(0.0003)	(0.0003)	(0.0003)	(0.0006)	(0.0003)	(0.0003)	(0.0004)

Notes: These numbers are calculated using the marginal effects calculated from logit estimates run separately for each year; see Appendix Table 3 for the marginal effects from which the top panel numbers are calculated. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen. Standard errors are calculated as for the difference between two means.

[a] These numbers are calculated by subtracting the relative institutionalization rate in the earlier Census from the relative institutionalization rate in the later Census for cohorts that had been in the U.S. for the same length of time (e.g., marginal effect for 1996–2000 cohort in 2000 minus marginal effect for 1985–1990 cohort in 1990).

Appendix

Two potential problems with Census data could affect our results. The first is the "undercount"—that is, failing to enumerate individuals, a problem typically considered more serious in poor and minority communities. The second potential problem concerns how the Census counts special populations, such as those in institutions. For example, if a high fraction of those under correctional supervision are in transition (being transferred from one place to another), the population count may miss them. Thus, it is worth understanding how the Census collects data for special populations.

1) Data Collection in Special Populations

Data collection in special populations—like that in the institutionalized population—may present particular challenges. For example, many people in institutions may be unable or unwilling to fill out Census forms. In the case of prisoners, people may also frequently be moved between institutions, creating difficulty in counting them.

The Census has developed a different form for people living in group quarters, and additional forms for those in military quarters and onboard ships. As mentioned in the data section, some types of group quarters are designated as "institutions." Jails and prisons fall into this category. Although the PUMS data does not separately identify this population, we can infer that a very high fraction of the institutionalized population is in correctional facilities by limiting our sample to men aged 18-40 (confirmed by comparisons to the 1980 Census, where type of institution is identifiable).

In the 2000 Census, about half of those living in group quarters were unable to fill out Census forms (personal correspondence with Karen Humes, Special Populations Division, U.S. Census Bureau). A disproportionate share of these individuals is in an institution (jail, prison, or mental institution, for example). In this case, Census enumerators fill out the forms using the institutions' administrative records.

The enumeration procedure for group quarters occurs over several weeks. Census enumerators ask where an inmate was on April 1. If inmates who are in transit on April 1 reach their final destination that day, they are counted at the final destination. If they are in transit, they are counted at their originating location.

Under these circumstances, then, the institutionalized population is likely to be well-counted, as the institutions are likely to keep accurate administrative records that document the number of inmates. Thus, the "undercount" of the institutionalized population is likely much less severe than that of the overall population. The demographic information on inmates of correctional institutions is also likely to be about as good as the administrative records themselves. Since there was more pressure and more incentive for correctional institutions to identify (non-naturalized) immigrants in their inmate populations in 2000 than in 1990, we would expect that a higher fraction of immigrants would be identified in 2000 than in 1990. Thus, any "undercount" of institutionalized immigrants would likely be higher in 1990 than in 2000.

2) The Undercount

The 1990 Census is widely viewed to have missed a substantial number of people. This problem is thought to be particularly severe in the case of poor and minority communities. The undercount does not present a problem for our analysis per se, if all populations are miscounted to the same degree in all years. There may be a problem for our analysis of changes in institutionalization between years, however, if the undercount changes across the years, or is different for different populations.

Consider, for example, the change in the institutionalization rates of immigrants between 1990 and 2000. Table 2 shows that the fraction institutionalized for immigrants in 1990 was 0.0107 and fell to 0.0068 in 2000. Our interpretation is that immigrants were less likely to be institutionalized in 2000 than in 1990. However, this change could occur mechanically if the undercount of minority communities was less severe in 2000 than in 1990. Our interpretation of this decline in institutionalization as signaling something about the behavior of immigrants in the U.S. would be flawed, if the decline really occurs because of better data collection.

Robinson et al. (2002) uses demographic analysis to estimate the undercount in 1990 and 2000. They estimate that the net undercount in 1990 was 1.65%, and in 2000 was a much smaller 0.12%. We can use these estimates to do some "back-of-the-envelope" calculations as to how the undercount of the immigrant population might affect our estimate of the fraction of immigrants who are institutionalized. Appendix Table 1 shows how our estimate of the fraction of immigrants institutionalized would change under different assumptions about the undercount of immigrants in 1990 and 2000.

Appendix Table 1: Estimates of How the Undercount Might Affect Fraction of Immigrants Institutionalized

Undercount Ratio Immigrants : Native-Born	Fraction Institutionalized	
	1990	2000
1:1	0.0105	0.00679
2:1	0.0104	0.00678
3:1	0.0102	0.006776
37:1	0.0067	0.0065

These calculations are based on the numbers reported in Tables 1 and 2. For example, in 1990, our sample included 209,878 immigrants. The fraction institutionalized was 0.0107, implying 2245.7 institutionalized immigrants in 1990. If we assume that the undercount estimate applies to the non-institutionalized population, then we need to subtract the number of institutionalized immigrants from the full sample, multiply this number by the fraction "missing," and then add this number back onto the estimate of the total number of immigrants: (209878-2245.7) * 0.0165 = 3425.9. Thus, the fraction institutionalized among the immigrants, assuming a 1.65% undercount, would be: 2245.7 / (209878 + 3425.9) = 0.0105.

Using this formulation, we can examine the impact on the estimate of the fraction of immigrants institutionalized, given different assumptions about the severity of the undercount in the non-institutionalized immigrant population.

Assuming that the undercount is three times larger for immigrants than for the overall population (that is, there are three "missing" immigrants for every "missing" person overall), we would still find that the fraction institutionalized among immigrants was over 1.5 times higher in 1990 than in 2000. For the undercount to be the only reason for the decline in the fraction institutionalized among immigrants between 1990 and 2000, we would have to think that the undercount was about 37 times larger for immigrants than for the population overall.

In sum, neither the improvement in the undercount of the overall population between 1990 and 2000, nor undercount problems that pertain to the institutionalized population, would be likely to mechanically generate our finding that the fraction of immigrants institutionalized between 1990 and 2000 substantially declined.

Appendix Table 2. Marginal Effects for Logit Estimates of Institutionalization
(Evaluated at Constant Profile, Robust Standard Errors in Parentheses)

	(0)	(1)	(2)	(3)	(4)
Immigrant	-0.0348				
	(0.0014)				
1996-2000		-0.0363	-0.0455	-0.0552	-0.0665
		(0.0014)	(0.0018)	(0.0026)	(0.0044)
1991-1995		-0.0341	-0.0437	-0.0535	-0.0648
		(0.0014)	(0.0018)	(0.0026)	(0.0043)
1985-1990		-0.0284	-0.0390	-0.0493	-0.0606
		(0.0014)	(0.0017)	(0.0025)	(0.0043)
1980-1984		-0.0168	-0.0283	-0.0402	-0.0513
		(0.0021)	(0.0021)	(0.0027)	(0.0043)
1975-1979		-0.0148	-0.0248	-0.0369	-0.0478
		(0.0024)	(0.0024)	(0.0029)	(0.0044)
1970-1974		-0.0021	-0.0107	-0.0258	-0.0364
		(0.0039)	(0.0040)	(0.0040)	(0.0052)
1965-1969		0.0084	0.0074	-0.0089	-0.0188
		(0.0055)	(0.0064)	(0.0061)	(0.0070)
1960-1964		0.0235	0.0337	0.0191	0.0107
		(0.0078)	(0.0102)	(0.0102)	(0.0110)
1950-1959		0.0096	0.0182	0.0132	0.0050
		(0.0090)	(0.0121)	(0.0133)	(0.0141)
1940-1950		0.0405	0.0493	0.0415	0.0369
		(0.0172)	(0.0206)	(0.0216)	(0.0230)
Less than H.S.			0.6068	0.5473	0.5795
			(0.0144)	(0.0160)	(0.0166)
H.S. Degree			0.0446	0.0515	0.0613
			(0.0018)	(0.0025)	(0.0039)
Some College			0.1693	0.1588	0.1811
			(0.0109)	(0.0112)	(0.0135)
Black				0.1947	0.2206
				(0.0074)	(0.0102)
American Indian				0.0412	0.0481
				(0.0050)	(0.0061)
Asian or Pacific				0.0311	0.0372
				(0.0053)	(0.0065)
Other Race				-0.0039	-0.0046
				(0.0025)	(0.0029)
Hispanic				0.0326	0.0386
				(0.0020)	(0.0029)
U.S. Citizen					-0.0114
					(0.0032)
Years in U.S.	0.0028	-0.0008	-0.0024	-0.0025	-0.0028
	(0.0003)	(0.0003)	(0.0004)	(0.0005)	(0.0006)
Years in U.S.^{2}	-0.00004	-0.00001	0.00003	0.00004	0.00004
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00002)
1980	-0.0250	-0.0262	-0.0348	-0.0395	-0.0470
	(0.0010)	(0.0010)	(0.0014)	(0.0019)	(0.0030)
1990	-0.0145	-0.0156	-0.0200	-0.0216	-0.0256
	(0.0007)	(0.0008)	(0.0010)	(0.0013)	(0.0018)
Age Dummies	Yes	Yes	Yes	Yes	Yes
Pseudo R-square	0.0193	0.0204	0.1039	0.1614	0.1614

Notes: The marginal effects are calculated for a 25-year-old Hispanic male with a high school degree. Number of observations for 1980 is 2,027,504. Number of observations for 1990 is 2,193,947. Number of observations for 2000 is 2,228,495. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen.

Appendix Table 3. Marginal Effects for Logit Estimates of Institutionalization
(Evaluated at Sample Means, Robust Standard Errors in Parentheses)

	1990					2000				
Immigrant	-0.0110					-0.0276				
	(0.0003)					(0.0002)				
1996-2000							-0.0254	-0.0166	-0.0142	-0.0146
							(0.0002)	(0.0002)	(0.0001)	(0.0002)
1991-1995							-0.0239	-0.0160	-0.0137	-0.0141
							(0.0003)	(0.0002)	(0.0001)	(0.0001)
1985-1990		-0.0144	-0.0112	-0.0095	-0.0095		-0.0219	-0.0155	-0.0134	-0.0138
		(0.0004)	(0.0002)	(0.0002)	(0.0002)		(0.0003)	(0.0002)	(0.0001)	(0.0002)
1980-1984		-0.0097	-0.0094	-0.0083	-0.0083		-0.0183	-0.0136	-0.0119	-0.0123
		(0.0006)	(0.0003)	(0.0002)	(0.0003)		(0.0004)	(0.0002)	(0.0002)	(0.0002)
1975-1979		-0.0091	-0.0090	-0.0079	-0.0079		-0.0189	-0.0131	-0.0115	-0.0118
		(0.0007)	(0.0003)	(0.0002)	(0.0003)		(0.0005)	(0.0002)	(0.0002)	(0.0002)
1970-1974		-0.0076	-0.0081	-0.0073	-0.0073		-0.0151	-0.0106	-0.0098	-0.0104
		(0.0011)	(0.0005)	(0.0003)	(0.0004)		(0.0009)	(0.0004)	(0.0003)	(0.0003)
1965-1969		-0.0043	-0.0049	-0.0049	-0.0049		-0.0180	-0.0111	-0.0100	-0.0104
		(0.0014)	(0.0008)	(0.0006)	(0.0006)		(0.0011)	(0.0007)	(0.0005)	(0.0004)
1960-1964		-0.0044	-0.0031	-0.0031	-0.0031		-0.0097	-0.0045	-0.0048	-0.0058
		(0.0019)	(0.0013)	(0.0010)	(0.0010)		(0.0027)	(0.0019)	(0.0014)	(0.0013)
1950-1959		-0.0096	-0.0060	-0.0041	-0.0041					
		(0.0017)	(0.0012)	(0.0012)	(0.0012)					
1940-1950										
Less than H.S.			0.1920	0.1153	0.1153			0.2468	0.1630	0.1621
			(0.0054)	(0.0038)	(0.0038)			(0.0046)	(0.0035)	(0.0035)
H.S. Degree			0.0491	0.0315	0.0315			0.0689	0.0469	0.0466
			(0.0014)	(0.0010)	(0.0010)			(0.0014)	(0.0010)	(0.0010)
Some College			0.0356	0.0243	0.0243			0.0349	0.0243	0.0242
			(0.0013)	(0.0010)	(0.0010)			(0.0011)	(0.0008)	(0.0008)
Black				0.0393	0.0393				0.0432	0.0430
				(0.0007)	(0.0007)				(0.0006)	(0.0006)
American Indian				0.0173	0.0173				0.0043	0.0043
				(0.0013)	(0.0013)				(0.0006)	(0.0006)
Asian or Pacific				0.0013	0.0013				0.0081	0.0083
				(0.0009)	(0.0009)				(0.0008)	(0.0008)
Other Race				0.0314	0.0314				-0.0010	-0.0010
				(0.0071)	(0.0071)				(0.0003)	(0.0003)
Hispanic				0.0119	0.0119				0.0165	0.0164
				(0.0005)	(0.0005)				(0.0005)	(0.0005)
U.S. Citizen					0.0000					-0.0053
					(0.0008)					(0.0010)
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-square	0.0072	0.0077	0.0779	0.1379	0.1379	0.0213	0.0221	0.1166	0.1739	0.1739

Notes: The marginal effects are calculated at the sample means. Number of observations for 1990 is 2,193,947. Number of observations for 2000 is 2,228,495. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen.