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The local effects of federal law enforcement policies: Evidence from sanctuary jurisdictions and crime

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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

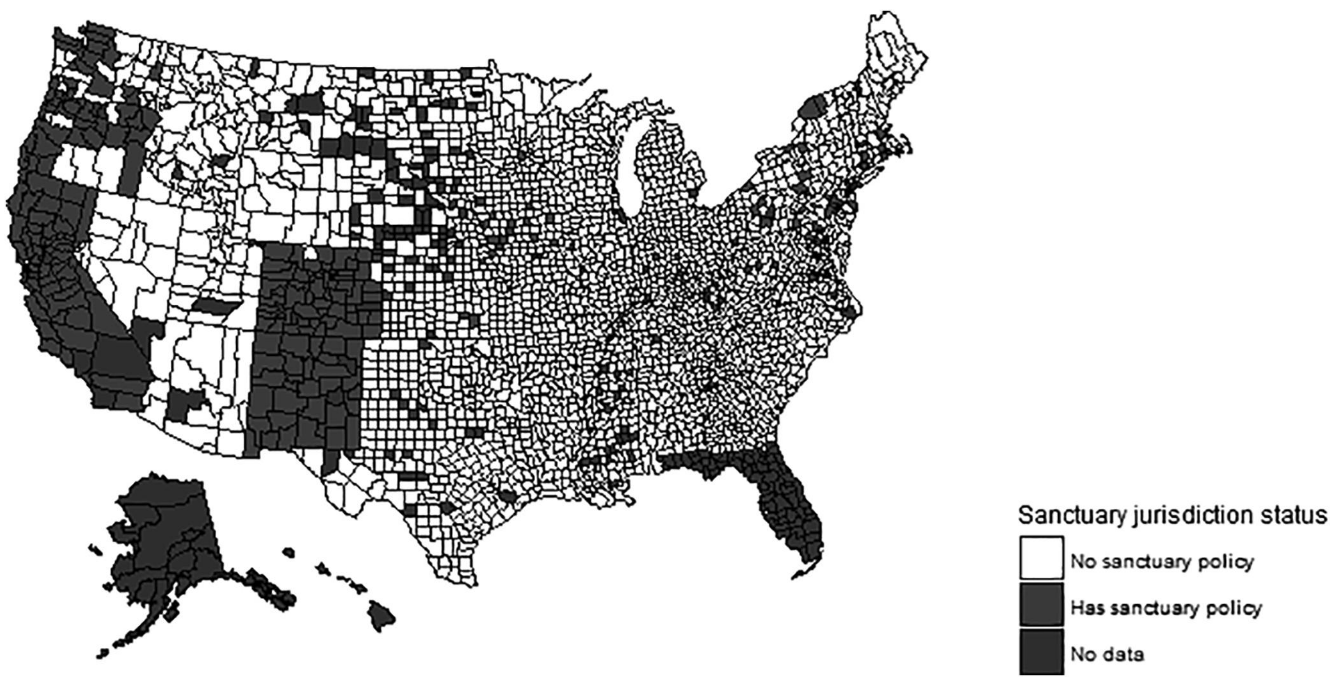

FIGURE 1 Map of sanctuary jurisdictions in the United States

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:

$$c_{ct}^{j} = \alpha_c^j + \gamma_t^j + X'_{ct}\beta^j + \sum_{\tau=2008}^{\tau=2016} (SJ_c * y_{\tau t})\delta_\tau^j + \epsilon_{ct}^j, \tag{1}$$

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 $y_{\tau t}$ 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 (11.588)	20.290** (9.155)	−5.111 (13.616)	−1.708 (2.042)	−7.817*** (2.296)	0.431 (2.606)	1.460 (11.431)	30.961*** (9.566)	−8.682 (11.926)
Treatment effect in 2011	−2.226 (8.706)	8.536 (10.530)	−5.914 (9.988)	−3.799* (2.143)	−8.490 (5.764)	−2.235 (1.919)	−0.871 (9.265)	19.372* (10.253)	−7.777 (9.590)
Treatment effect in 2012	−8.336 (6.766)	−0.971 (6.652)	−10.427 (7.780)	−1.638 (1.036)	−3.987* (1.995)	−0.684 (1.079)	−6.789 (7.011)	8.125 (5.572)	−11.617 (7.448)
Treatment effect in 2014	−6.096 (3.857)	7.336 (8.349)	−10.862*** (3.768)	−3.096 (2.499)	−8.050 (7.870)	−1.732 (1.223)	−3.062 (4.300)	15.799** (7.060)	−9.278*** (3.082)
Treatment effect in 2015	−17.411** (8.398)	14.756 (14.078)	−28.384*** (7.459)	−4.383* (2.281)	−0.352 (6.366)	−5.942*** (2.111)	−11.832* (6.979)	15.861 (11.096)	−21.070*** (6.354)
Treatment effect in 2016	−17.438* (10.196)	22.469 (16.609)	−30.869*** (8.828)	−3.868 (3.094)	0.047 (10.460)	−5.341* (2.795)	−14.227 (8.864)	20.765** (10.187)	−25.826*** (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** (7.879)	−0.025 (0.188)	−3.685 (2.652)	−4.950 (3.533)	−11.669* (6.715)	9.234 (40.842)	−8.721 (17.540)	5.669 (8.214)	12.285 (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.

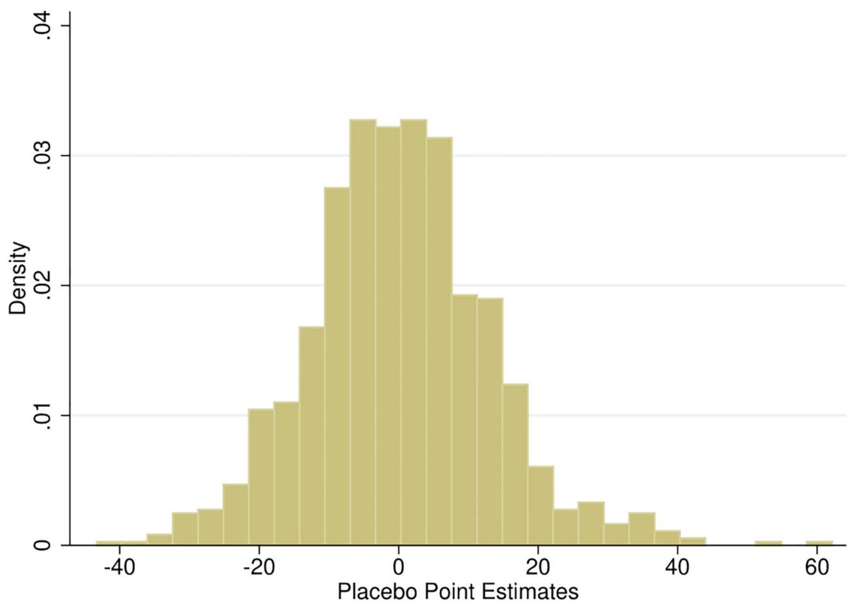

FIGURE 2 Distribution of placebo treatment effects [Colour figure can be viewed at wileyonlinelibrary.com]

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.

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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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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of the article at the publisher's website.

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