Immigration Enforcement and Public Safety*

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Abstract

How does immigration enforcement affect public safety? Heightened immigration enforcement could reduce crime by deterring and incapacitating immigrant offenders or, alternatively, increase crime by discouraging victims from reporting offenses. We study the U.S. Secure Communities program, which expanded interior enforcement against unauthorized immigrants. Using national survey data, we find that the program reduced the likelihood that Hispanic victims reported crimes to police *and* increased the victimization of Hispanics. Total *reported* crimes are unchanged, masking these opposing effects. We provide evidence that reduced Hispanic reporting is the key driver of increased victimization. Our findings underscore the importance of trust in institutions as a central determinant of public safety.

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The debate surrounding the enforcement of immigration laws often revolves around consequences for public safety. Recent survey evidence shows that approximately half of Americans believe that "immigrants are making the crime situation in the country worse" and more Americans believe that immigrants have an adverse impact on crime than they do on jobs or the economy.[1] While a large body of research has found mixed evidence on the effect of immigration on local crime rates, proponents of immigration enforcement view it as a key policy tool for improving public safety.[2]

While heightened immigration enforcement has the potential to reduce crime through the deportation and deterrence of immigrant offenders, public safety could suffer if heightened enforcement degrades trust in law enforcement. Police rely on victims and witnesses for detecting, apprehending, and convicting offenders, making trust a key input into successful law enforcement. Consequently, immigration policies that reduce the willingness of victims to contact or cooperate with police could result in *more* criminal offending.

This paper studies the impact of immigration enforcement on public safety. We ask two main questions: First, what is the effect of increased enforcement on criminal victimization? Second, how does increased enforcement impact the likelihood that civilians contact the police when victimized? We answer these questions by leveraging quasi-experimental policy variation in U.S. interior immigration enforcement and using detailed restricted survey data on victimization and crime reporting behavior. Contrary to the policy goal that heightened enforcement would improve public safety, our analysis finds that an expansion of immigration enforcement did not reduce overall offending and, in fact, *increased* crime against Hispanics. Further, the policy reduced the likelihood that Hispanic victims report crime to the police.

Understanding the public safety consequences of immigration enforcement is a key issue in American immigration policy. The U.S. invests heavily in immigration enforcement, spending over $25 billion on these efforts annually (American Immigration Council, 2021). A stated goal of this enforcement is improved public safety, and the majority of interior (non-border) immigration enforcement is targeted toward unauthorized individuals with criminal records.[3] Nevertheless, law enforcement officials across the country have publicly raised concerns about the effectiveness of immigration enforcement efforts, claiming that these

[1] Gallup survey trends: `https://news.gallup.com/poll/1660/Immigration.aspx`.

[2] See, e.g., Butcher and Piehl (1998), Chalfin (2014), and Spenkuch (2014) for evidence from the U.S. on immigration and local crime, and Bell et al. (2013), Bianchi et al. (2012), and Piopiunik and Ruhose (2017) for evidence from Europe.

[3] The mission statement of the Immigration and Customs Enforcement Agency (ICE) is to "Protect America through criminal investigations and enforcing immigration laws to preserve national security and public safety" (`https://www.ice.gov/mission`).

policies have reduced trust in police among immigrant communities and thus reduced police effectiveness.[4] The "chilling effect" of these policies likely stems from victims' fear that they, a family member, or a neighbor will be detained and deported as a result of contact with law enforcement (Armenta, 2017; Watson and Thompson, 2022).

The primary obstacle to estimating the impact of immigration enforcement on public safety is the inherent difficulty in measuring changes in criminal behavior. Most administrative crime databases only include crimes that have been *reported to law enforcement*. Variation in reported crimes may thus be driven by changes in underlying criminality, changes in victim decisions to report crimes, or both.

We overcome this measurement challenge by utilizing data from the National Crime Victimization Survey (NCVS), a nationally representative survey which asks individuals whether they have been the victim of a crime, and if so, whether they reported that crime to police. This survey allows us to separately estimate the impact of immigration enforcement on the incidence of crime and on crime reporting behavior. Further, most administrative data consist of total counts of reported crimes with no information about demographic characteristics, a substantial limitation in a setting where 90% of deported individuals are of Hispanic ethnicity. The NCVS includes ethnicity of respondents, allowing us to separately estimate effects for Hispanics and non-Hispanics. We focus throughout on effects for Hispanic individuals — including both citizens and non-citizens — consistent with prior work showing that Hispanic citizens also respond to immigration enforcement policy due to fear that a family member or neighbor may be deported (Watson, 2014; Alsan and Yang, 2022).

To estimate the causal impact of immigration enforcement, we study the federal Secure Communities (SC) program, a policy that increased information sharing between local police and federal immigration authorities, thus reducing the cost of identifying unauthorized immigrants arrested by local police. This program was the largest expansion of interior immigration enforcement in U.S. history, and it resulted in large increases in immigrant detentions and deportations nationwide. We estimate that immigrant detentions by Immigration and Customs Enforcement (ICE) increased by 54% following the program's introduction. Importantly, the program was implemented piecemeal across counties between 2008 and 2013 due to resource constraints. We leverage the differential timing of program implementation to evaluate its impact using a staggered difference-in-differences design. We estimate effects separately for Hispanic and non-Hispanic respondents, comparing respondents of the same

[4] As an example, see "Fewer Immigrants Are Reporting Domestic Abuse. Police Blame Fear of Deportation," *The New York Times*, June 3, 2018.

ethnicity before and after SC adoption across counties with different program status.

We find that this immigration enforcement policy caused a significant increase in the victimization of Hispanics. Relative to a 0.9 percentage point monthly victimization rate in the pre-period, Hispanic victimization increased by 0.15 percentage points, a 16% increase. These estimates imply that Secure Communities resulted in 1.3 million additional crimes against Hispanics in the two years following program activation. This effect appears to be largely concentrated among property crimes, which comprise the majority of victimizations, though we find similarly sized but less precise increases in violent crime.[5] The victimization increase is larger for Hispanic females and in counties with a greater share of non-citizen Hispanics.

In contrast, we find no change in the overall victimization of non-Hispanic individuals. We do, however, observe an increase in the victimization of non-Hispanics who live in areas with a high share of Hispanic residents. Across the full population (pooling Hispanic and non-Hispanic respondents), we rule out declines in victimization of more than 3.3%, indicating quite precisely that the policy did not create meaningful improvements in aggregate public safety.

At the same time, we find that the policy led to a significant decline in the rate of reporting incidents to the police. Hispanics reduce their reporting rate by 9 percentage points, a significant and sizable decline of 30% relative to the pre-period reporting mean of 33 percentage points. This decline mirrors the increase in victimizations, occurring relatively quickly and appearing more pronounced among property offenses. Like with victimization, we find no changes in the reporting behavior of non-Hispanics.

Combining our primary outcomes, we find that *reported* crimes (i.e., the likelihood of being victimized *and* reporting a crime) are unchanged after the launch of the program. This precise null result aligns closely with prior work that studied the Secure Communities policy using police data on reported crimes (Miles and Cox, 2014; Treyger et al., 2014; Hines and Peri, 2019), and it underscores the importance of separately measuring victimization and victim reporting decisions to detect changes in public safety.

We conduct several analyses that confirm the robustness of the main findings. First, our results are unchanged when considering different choices for the sample definition and time window. Further, our baseline specification is an adapted difference-in-differences design following Sun and Abraham (2021), but our estimates are similar when using a traditional

[5] This result differs from concurrent work in criminology (Baumer and Xie, 2023), which estimates "between-within" logistic regressions and finds an 86% increase in violent victimizations of Latinos from SC program activation.

two-way fixed effects approach, other modified difference-in-differences designs (Borusyak et al., 2021; Callaway and Sant'Anna, 2021), or a triple-differences design that uses non-Hispanics as a comparison group for Hispanic individuals. We also show that the victimization increase is not an artifact of survey attrition or compositional changes among survey respondents, nor is the reporting decline due to a mechanical change in the types of victims or crimes committed (i.e., an increase in crimes that are less likely to be reported).

To examine how the policy may have affected the composition of offenders, we augment our NCVS analysis with novel administrative records on 911 calls and arrests from 75 medium to large police departments. These detailed data were hand-collected through records requests to individual police departments, and each record is geocoded to a Census tract in order to link the outcomes to neighborhood resident demographics. The arrest records allow us to examine the ethnic composition of arrested individuals. We find that the policy reduced the share of arrested individuals who are Hispanic, suggesting that the rise in offending was more concentrated among non-Hispanics.

In the final part of our paper, we provide evidence that the decline in reporting is the main driver of increased victimization. We first estimate victimization and reporting effects separately by cohort of program implementation and show that cohorts with larger declines in reporting also experienced larger increases in victimization. Next, we conduct a decomposition that quantifies the relative importance of the reporting decline in explaining the victimization increase compared to the program's effects on other social and economic factors that could also impact crime. From this analysis, we conclude that the reporting decline is substantially more important for the increase in victimization than other concurrent impacts of SC on unemployment, wages, the share of female-headed households, and the male immigrant share of the population. This finding contributes to a small but growing literature that highlights the importance of victim behaviors (Chalfin et al., 2023; Dominguez, 2022; Vasquez, 2022), and particularly victim reporting (Golestani, 2021; Miller and Segal, 2019), as key inputs into public safety.

Contribution to Literature — Our study contributes to a large literature exploring the various impacts of immigration enforcement. We build on work that documents the "chilling" effects of U.S. immigration enforcement on labor market outcomes (East et al., 2018; Ali et al., 2022), workplace safety (Grittner and Johnson, 2022), poverty rates (Amuedo-Dorantes et al., 2018), and participation in public assistance programs (Alsan and Yang, 2022; Santillano et al., 2020; Vargas and Pirog, 2016; Watson, 2014). Our work is most closely related to research that finds increases in Hispanic crime reporting rates following policies that *decrease* fear of deportation (Comino et al., 2020; Jácome, 2022). In particular, Comino et al. (2020) studies the 1986 U.S. amnesty law, which granted legal status to 2.7 million unauthorized immigrants but did not affect law enforcement tactics, and finds evidence that this policy increased crime reporting and may have reduced crime. We likewise build on previous work examining immigration enforcement and reported crime from administrative data, which has found mixed impacts (Amuedo-Dorantes and Deza, 2022; Chalfin and Deza, 2020; Dhingra et al., 2021; Hausman, 2020; Hines and Peri, 2019; Kang and Song, 2022; Miles and Cox, 2014; Pinotti, 2015; Treyger et al., 2014). We contribute by connecting the literatures on the "chilling" and public safety effects of immigration policy, providing novel evidence on the direct impact of immigration enforcement on victimizations (regardless of whether they were reported) as well as on victims' willingness to report crimes to law enforcement, the authority most directly empowered to enforce immigration policies.

This paper also relates to the literature on the impact of trust in formal institutions (Glaeser et al., 2002; Knack and Keefer, 1997), particularly within the criminal justice system (Ba, 2018; Bell, 2017; Tyler and Huo, 2002). Recent work in economics, sociology, and political science explores whether victim reporting is sensitive to high-profile scandals, like police shootings, that might negatively impact trust in law enforcement institutions (Ang et al., 2023; Cohen et al., 2019; Desmond et al., 2016; Zaiour and Mikdash, 2023; Zoorob, 2020a,b).[6] Given the theoretical importance of community trust for the detection and prevention of crime, scholars have posited that future improvements in public safety will rely on building relationships between law enforcement and the community (Tyler, 2005).

Along these lines, a small literature has documented an inverse association between crime reporting and domestic violence. Miller and Segal (2019) shows that increased hiring of female police officers results in both higher reporting rates and lower incidence of domestic violence. Likewise, Golestani (2021) finds that policies that sanction landlords for excessive 911 calls reduce the likelihood that tenants report assaults and increase intimate partner homicide. By linking declining trust in law enforcement with an increase in crime rates, we provide novel evidence that police-community relations are an important input to the production of public safety.

Finally, we add to a growing literature emphasizing the importance of directly measuring criminality and crime reporting behavior (Carr and Doleac, 2016, 2018; Miller et al., 2022) and of collecting data on victim demographics (Harvey and Mattia, 2022) for understanding

[6] Further, ethnographic and survey studies have documented a strong relationship between measures of police-community trust and the willingness of individuals to contact the police (Carr et al., 2007; Lerman and Weaver, 2014; Messing et al., 2015).

changes in public safety. Research on criminal behavior often relies, by necessity, on administrative records of reported crime and typically assumes that changes in reported crime reflect changes in underlying criminality. By leveraging survey data, we are able to show that changes in reporting behavior can be large and relatively quick, underscoring the importance of accounting for victim reporting decisions for accurately measuring changes in public safety. Moreover, our findings point to the importance of employing victim demographic information, especially in contexts where racial or ethnic minorities are disproportionately affected by law enforcement policies.

1 Institutional Background

Federal immigration enforcement has expanded significantly over the past three decades, marked by several notable policy changes. While many of these policies focused on strengthening border security, another set of policies has taken steps to enforce immigration laws in the interior of the country. This latter focus has increasingly involved the participation of state and local officials in enforcing federal immigration law (Watson and Thompson, 2022).

In 2008, the federal government launched the Secure Communities (SC) program, an information-sharing initiative that significantly expanded its ability to identify and detain individuals in violation of immigration law who had been arrested for a criminal offense. Upon the program's activation, the fingerprints of individuals booked into local jails were not only forwarded to the Federal Bureau of Investigation (FBI) (as they had been historically),[7] but they were also now sent to the Department of Homeland Security (DHS). This agency cross-references the fingerprint records with information on prior immigration infractions, border crossings, or expired visas to determine whether there is reason to deport the individual. If so, the Immigration and Customs Enforcement (ICE) agency within DHS issues a "detainer" request (i.e., an immigration hold) asking local officials to keep the individual in their custody until they can be transferred to federal custody for the initiation of deportation proceedings. Because fingerprints of jailed individuals were automatically forwarded to DHS with the activation of SC, local officials could not prevent federal officials from learning about the immigration status of an arrested individual (and thus opt out of participating in the program). The program's novel ability to screen every person arrested by local law enforcement anywhere in the country quickly made this program the largest

[7] The fingerprints of arrested individuals in a local jail are submitted to the FBI so this agency can conduct a standard criminal background check.

expansion of local involvement in immigration enforcement (Cox and Miles, 2013).[8,9]

Activation of the SC program was not immediate nationwide; rather, the rollout was staggered on a county-by-county basis due to factors like technological constraints and resource bottlenecks (Miles and Cox, 2014). Figures A.1 and A.2 depict the rollout. Early activation was not correlated with local crime rates; instead, early activation of SC was correlated with the share of a county's population that is Hispanic, the presence of a 287(g) agreement, and proximity to the border (Miles and Cox, 2014). Prior work studying the effects of SC shows that, in terms of economic characteristics and crime, the timing of the rollout can be considered as good as random (East et al., 2018; Medina-Cortina, 2022). We confirm these findings in Table A.1: levels and changes in crime rates and in economic characteristics (i.e., a county's unemployment and poverty rates) are not associated with SC activation timing after accounting for county demographic characteristics.

Following the launch of the program, the number of immigrant detentions and deportations rose quickly nationwide. Figure A.3 shows that the number of "honored" ICE detainers —those that result in a transfer to ICE custody —doubled between 2008 and 2012. The second panel of this figure plots the number of removals (i.e., deportations) that occurred in each month because of SC. In any given month, over 90% of detainers and removals were for individuals of Hispanic ethnicity. Finally, the scale of local involvement in immigration enforcement is evident in Figure A.4: over half of arrests resulted from local referrals, significantly exceeding the number of arrests that originated via referrals from state and federal prisons and those made directly by ICE in communities (including workplaces).

As SC was established nationally, some law enforcement leaders warned that increasing local involvement in immigration enforcement would compromise trust in police among immigrant communities. In particular, police chiefs cited concerns about decreased victim and witness willingness to report crimes, as these civilian groups may fear increased risk of

[8] Prior programs involving local participation in federal immigration enforcement included the Section 287(g) program — which permitted formal partnerships between federal and local law enforcement and authorized local police to act as immigration agents — and the Criminal Alien Program (CAP), which screened for unauthorized immigrants among arrestees. By the start of SC, 287(g) agreements were only in place in around 75 jurisdictions, and the CAP was only present in a small fraction of local jails (Cox and Miles, 2013; Watson and Thompson, 2022).

[9] Localities could not prevent federal officials from learning about an arrestee's immigration status; however, they could refuse to hold an individual in jail prior to the arrival of ICE officials (such localities are often termed "sanctuary cities"). Sanctuary cities were very uncommon during the first few years after SC implementation; 95% of these policies occurred in 2013–2015 (Hausman, 2020), after this paper's sample window.

deportation.[10] While the SC program targeted individuals arrested for a criminal offense, around 20% of individuals transferred from local jails to ICE custody were not charged or convicted of a crime (see Figure A.5).[11] The potential risk of deportation for non-offenders or non-serious offenders was acutely salient in the Hispanic community; in a 2012 survey, 44% of Latinos (70% of unauthorized immigrants and 28% of U.S.-born Latinos) reported that they were less likely to contact police if they were a victim of a crime because they feared police would inquire about their immigration status or that of the people they know (Lake et al., 2013). Moreover, even when local police have attempted to guarantee protection for immigrants who cooperate with criminal investigations, local authorities have often been limited in their ability to secure this protection given the competing jurisdictional authority of federal immigration agents.[12,13] Consistent with police chiefs' warnings, almost 50% of Latinos (and 65% of unauthorized Latinos) in the 2012 survey said they felt *less* safe because local law enforcement was involved with immigration enforcement, and that criminals had moved into their neighborhoods because they knew victims were less likely to report crimes (Lake et al., 2013).

2 Conceptual Framework

We outline here a simple conceptual framework to illustrate the predicted impacts of an increase in immigration enforcement on both crime reporting and criminal victimizations. For expositional simplicity, we assume that all offenders and victims are unauthorized immigrants, but the main predictions are unchanged if we allow a share of offenders and victims to be citizens. In Appendix B, we provide more details on this framework and discuss various extensions.

Potential offenders have a single choice of whether to commit an offense, which they

[10] See, for example, "Why cities are rebelling against the Obama administration's deportation policies," *Vox*, June 6, 2014.

[11] Moreover, not all detained individuals were unauthorized; Kohli et al. (2011) finds that roughly 3,600 citizens were unlawfully detained by ICE via the SC program between 2008 and 4/2011.

[12] See, for example, "The Teens Trapped between a Gang and the Law," *The New Yorker*, December 25, 2017.

[13] The U nonimmigrant status (U visa) program issues visas to victims of certain serious crimes who have suffered mental or physical abuse and are helpful to government officials in the investigation or prosecution of criminal activity. However, this program is small in practice and excludes property crime victims. Only 10,000 visas are issued each year, and there is a backlog of over 325,000 visas with a waitlist of 5–10 years (see "A visa program created to help law enforcement puts immigrant victims at risk instead," *National Public Radio*, August 12, 2023).

make by weighing the associated costs and benefits (Becker, 1968).There is a uniform benefit of committing a crime M and an offender-specific cost c (e.g., the opportunity cost of committing a crime), which has distribution $G(c) \in [0,1]$ across offenders. Offenders face a probability of getting caught — which is a function of the reporting behavior of victims r, because police can only investigate crimes that are known to them, and the probability police apprehend the offender a — as well as an expected punishment x. In addition, apprehended offenders also face a probability of being referred to immigration officials p_D and a cost of deportation D.[14] We normalize the value of abstaining from crime to 0, so offenders commit an offense if the benefits outweigh the costs: $M - rax - rap_D D - c > 0$. The number of offenses is thus $O = G(M - rax - rap_D D)$.

Analogously, victims of crime face the choice of reporting the incident to the police. There is a uniform benefit from reporting the crime b as well as a victim-specific hassle cost of reporting h with distribution $F(h) \in [0,1]$ across victims. There is an additional cost of reporting the incident related to immigration enforcement: this cost is a function of the probability that an individual is referred to immigration officials δp_D and the cost of deportation D.[15] Again, we normalize the value of not reporting to 0, so victims report an incident if the benefits outweigh the costs: $b - \delta p_D D - h > 0$. This rule implies a reporting probability $r = F(b - \delta p_D D)$.

The Secure Communities program increased information sharing between local law enforcement and federal immigration authorities, thereby raising the probability p_D that individuals would be referred to immigration officials. Notably, p_D enters into both a victim's reporting decision as well as an offender's decision to commit crime, and it thus affects both parties. An increase in p_D implies a higher cost of reporting offenses, and we thus expect the reporting probability r to unambiguously decline: $\frac{\partial r}{\partial p_D} < 0$. In contrast, the prediction for O is ex-ante ambiguous:

$$\frac{dO}{dp_D} = G'(\cdot)\Big[\underbrace{-\frac{\partial r}{\partial p_D}(ax + ap_D D)}_{\text{Lower Reporting} \uparrow \text{Crime}} \quad \underbrace{-\, raD}_{\text{Deterrence} \downarrow \text{Crime}}\Big] \lesseqgtr 0$$

Intuitively, an increase in p_D increases the cost of offending, but the decline in r among

[14] Non-citizen offenders can expect to serve two punishments: one through the criminal justice system (a period of incarceration in the US) and one through the federal immigration system (detention and deportation).

[15] The parameter $\delta \in [0,1]$ allows the probability of deportation to differ between victims and offenders. Importantly, δ incorporates a victim's belief about their likelihood of deportation — whether that likelihood is real or perceived — and it thus reflects trust in law enforcement.

victims also lowers this same cost by decreasing the likelihood that crimes are detected by police. The impact of increased immigration enforcement on public safety thus depends on which of these effects dominates. We note that the sign of the impact of immigration enforcement on *reported* crime, $C = r \cdot O$, is likewise ambiguously signed and may differ from the impact of enforcement on total offenses.

Ultimately, the effect of Secure Communities on both crime and reporting behavior is an empirical question. To overcome the challenges associated with estimating the effect of enforcement on these two outcomes, we utilize the National Crime Victimization Survey, which we now discuss in greater detail.

3 Data

3.1 Data Sources

Our primary data set is the National Crime Victimization Survey (NCVS) administered by the Bureau of Justice Statistics (BJS) at the Department of Justice and the U.S. Census Bureau. This survey is the nation's primary data source on victimizations and collects information from a nationally representative sample of approximately 240,000 persons each year. The NCVS encompasses records of serious crimes that are characterized by having a victim (namely violent and property crimes) and is distinct from administrative police records on reported crime collected by the FBI Uniform Crime Reports (UCR). Nevertheless, research conducted by criminologists and BJS statisticians has found a high degree of convergence between the NCVS and UCR for crimes reported to police, especially in urban and suburban areas and after the year 2000 (e.g., Morgan and Thompson, 2022; Berg and Lauritsen, 2016; Lauritsen et al., 2016).

The NCVS asks respondents whether they experienced a victimization in the prior six months and follow-up questions about each victimization incident, including whether they informed the police about the incident. These data thus allow us to measure changes in crime (i.e., victimizations) separately from changes in crime reporting behavior. We can also construct measures of *reported* crime rates (i.e., the likelihood that an individual is both victimized *and* reports the crime, as a share of all individuals). We utilize the restricted-access version of the NCVS, available through the Census Bureau's Research Data Centers, because this version includes the respondent's county of residence.

Given the retrospective nature of the survey, we build a dataset at the person × year ×

month level corresponding to the years and months for which a respondent is answering.[16] To construct our baseline sample, we first limit the sample to respondents residing in counties that are consistently included in the NCVS for the 2006–2015 survey waves. Following Alsan and Yang (2022), we also exclude southern border counties as well as counties in Massachusetts, Illinois, and New York. Enforcement began earlier in border counties and selection could have played a role in program activation in these locations (Cox and Miles, 2015), whereas the latter three states resisted the implementation of the SC program. Our baseline sample also focuses on counties whose population in the 2000 Census exceeded 100,000 individuals. Hispanic individuals are significantly more likely to live in these counties and accordingly, immigration enforcement tends to be concentrated in these areas.[17]

Due to Census disclosure rules, we cannot report the precise number of counties in our baseline sample. However, as a frame of reference, 458 counties in the U.S. meet these sample restrictions, representing 173 million individuals (61% of the total population) and 35.3 million Hispanic individuals (69% of the Hispanic population) based on population counts from the 2000 Census. In our robustness checks, we consider the sensitivity of our estimates to our sampling choices.

Information on the activation date of the Secure Communities program in each county comes from publicly available reports published by DHS. We supplement this information with records on ICE detainer requests and removals from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. Detainer data provide information on all ICE detainer requests from 2002–2015, including whether a detainer was "honored" (i.e., whether an individual was "booked into detention" by ICE). Unlike the information about detainers, removals data only pertain to removals that occurred as a result of the SC program and is thus only available post-treatment. We aggregate these data sources to construct measures of the number of overall detainer requests, the number of honored detainers, and the number of SC removals at the county $\times$ year $\times$ month level. Our preferred measure of immigration enforcement is honored detainers because this measure is available both before

[16] The NCVS is a panel dataset in which the same addresses are interviewed every six months for three and a half years, and new addresses rotate into the sample on an ongoing basis. In our baseline analysis, we do not utilize the panel nature of the survey and instead treat the data as repeated cross-sections. For more details on the sample design, see Appendix C.

[17] Throughout this paper, we estimate effects separately by Hispanic ethnicity, as defined by the U.S. Census Bureau. The Census defines this group as persons "of Cuban, Mexican, Puerto Rican, South or Central American, or other Spanish culture or origin, regardless of race." Additionally, we use the term "unauthorized immigrant" to signify an immigrant who does not have legal status to remain in the U.S., similar to the alternative term "undocumented immigrant."

and after treatment, and it is more closely linked to deportation actions, as it only includes individuals who were transferred to ICE custody.[18]

We augment the analysis with local area characteristics. We use the 2000 Census and American Community Survey via IPUMS to calculate demographic and economic characteristics; Census Bureau population estimates; FBI Uniform Crime Reporting data; Urban Institute data on state and local immigration policies; MIT Election Data for election results; and Bureau of Labor Statistics data for unemployment rates.

3.2 Summary Statistics

Table 1 shows that on average, Hispanic individuals have higher victimization rates than non-Hispanic individuals, mainly due to a higher likelihood of experiencing property crimes. Across crime incidents, Hispanic victims report incidents at modestly lower rates than non-Hispanic individuals. Finally, as has been shown in prior studies (see, e.g., Carr and Doleac, 2018), there is significant under-reporting of incidents, with only 34% of crime incidents being reported to the police.

We preview our main results by plotting raw outcome means before and after SC implementation, separately for Hispanic and non-Hispanic individuals. Panel (a) of Figure 2 shows that the victimization rate of Hispanic respondents rises after the program implementation. At the same time, panel (a) of Figure 3 shows that the reporting rate of Hispanic respondents declines following the launch of the program. Non-Hispanic individuals do not appear to have any post-treatment change in either outcome. Finally, panel (a) of Figure 4 shows that *reported* crime rates, or the likelihood that a person is both victimized and reports the crime, appear stable over time for both ethnicity groups, illustrating the fact that reported crime rates can mask concurrent opposing changes in victimization and reporting rates.

[18] ICE detainer requests do not necessarily lead to a deportation. Unfortunately, information from TRAC on detentions cannot be linked to the removals data. Nevertheless, consistent with prior research (Alsan and Yang, 2022; Medina-Cortina, 2022), Figure A.6 shows a strong, positive correlation (0.86) between county-level SC removals and honored detainers in the post-SC period.

4 Empirical Strategy

Our empirical strategy leverages the differential timing of program implementation. Specifically, we estimate a difference-in-differences regression of the following form:

$$Y_{ict} = \beta_{\text{Post}} \text{SC}_{ct} + \mu_c + \delta_t + \epsilon_{ict} \tag{1}$$

where Y_{ict} is an outcome variable for person i in county c at time t (month×year). SC_{ct} is an indicator variable equal to one if county c had implemented the Secure Communities program at time t. The terms μ_c and δ_t correspond to county and time fixed effects, respectively. The error term is ϵ_{ict} and standard errors are clustered at the county level. Throughout the analysis, we use person-level survey weights to maintain sample representativeness.

The coefficient of interest is β_{Post}, which estimates the average difference in outcome Y in the two years after the implementation of the Secure Communities program relative to the difference in the outcome prior to the program's launch.[19] The availability of the victim's ethnicity in the NCVS allows us to separately estimate effects for Hispanic and non-Hispanic respondents.[20]

We also consider a fully dynamic version of this regression specification:

$$Y_{ict} = \sum_{\tau=-8}^{\tau=7} \beta_\tau \times \text{SC}^\tau_{ct} + \mu_c + \delta_t + \epsilon_{ict} \tag{2}$$

where we denote $\tau = 0$ as the first quarter after SC activation for each county c, and we include event-time dummies SC^τ_{ct} to quantify the effect of the program for the eight quarters before and after the implementation of the program.[21] We omit the quarter before the program introduction, so that each β_τ coefficient measures the difference in outcome Y relative to $\tau = -1$.

We have two main outcomes of interest: the likelihood that an individual is victimized and the likelihood that an individual reports a victimization to the police. The first outcome

[19] To separately identify the two-year effect, we also include an additional indicator variable that is equal to one for all time periods beyond the two years after the program's launch.

[20] Throughout the paper, we report and interpret estimates for all Hispanic respondents, rather than re-scaling the effects by the share of Hispanics that are unauthorized immigrants. We take this approach because it is not clear ex-ante that *only* unauthorized Hispanic immigrants would be affected by the policy. For instance, Alsan and Yang (2022) finds that citizen Hispanics change their behavior out of concern for their non-citizen contacts.

[21] We use the first and last indicators as "book-ends," so that they are equal to one for all time periods before and after the two years around implementation.

is a binary variable denoting whether an individual is victimized at time t among the sample of all individuals in the survey, where the unit of observation is a person-month. The second outcome is whether a criminal victimization that occurred at time t was reported by the victim to the police, where the unit of observation is a crime incident record. In our main results, we also consider a third outcome: the likelihood of being victimized *and* reporting the incident. This outcome is analogous to measures of reported crime rates available in administrative data sources (e.g., the FBI Uniform Crime Reporting program). For ease of exposition, we multiply outcome variables by 100.

A growing econometrics literature has documented issues with the standard ordinary least squares (OLS) approach to difference-in-differences regressions with two-way fixed effects (TWFE). The program we study has many features that correspond to the concerns raised in this literature. First, all counties in the U.S. implemented the program between October 2008 and January 2013. The universal rollout of the program means that a β_{Post} coefficient estimated via OLS will have a significant contribution from potentially undesirable comparisons; specifically, using already-treated units to estimate the effect of later-treated units. Second, the rollout of the policy occurs over a relatively short time frame, meaning that in a TWFE model, already-treated counties will comprise a meaningful share of control counties shortly after implementing the program. Third, past studies (Alsan and Yang, 2022; East et al., 2018) indicate that the Secure Communities program may have dynamic impacts that vary over time, which could cause the standard TWFE regression to place negative weight on later-treated time periods (Goodman-Bacon, 2021). Fourth, program effects may be heterogeneous across counties and depend on factors such as demographic composition, which could lead to dynamic regression coefficients which do not identify the true time path of impacts (Sun and Abraham, 2021).

Given these concerns, our baseline strategy follows Sun and Abraham (2021), using later-treated counties as the control units for counties treated earlier in time. We define the later-treated counties as the final 25% of counties to activate the program in our baseline sample (i.e., those that implemented the program after August of 2011). In Section 5, we show that the results are robust to alternative definitions of later-treated counties, as well as to the estimators proposed in Borusyak et al. (2021) and Callaway and Sant'Anna (2021). While the concerns highlighted above are important, in practice our results are also quite similar using the standard TWFE model or a triple-differences specification.

Throughout the paper, we estimate β_{Post} separately for Hispanic and non-Hispanic individuals. The identifying assumption for interpreting the estimate as the causal effect of the Secure Communities program is that individuals of a given ethnicity in earlier-treated counties would have continued to trend similarly to individuals of the same ethnicity in later-treated counties in the absence of the program. We consider the plausibility of this assumption by plotting the raw data as well as the β_τ coefficients from equation (2). We also estimate a triple-difference model leveraging comparisons across ethnicity, and likewise find similar results (see Section 6).

5 Results

5.1 First Stage: Immigration Enforcement

We first establish that the Secure Communities program did in fact increase immigration enforcement. Panel (a) of Figure 1 plots the average logged number of honored immigration detainer requests around the implementation of Secure Communities. Panel (b) then estimates equation (2) using the logged number of honored detainers as the outcome variable. This figure confirms that Secure Communities led to a large and sudden increase in immigration enforcement, consistent with findings in prior work (Alsan and Yang, 2022; Medina-Cortina, 2022). Our estimates using equation (1) suggest that Secure Communities increased county-level honored detainer requests by over 50%. If we instead use all detainer requests as the outcome variable, we find a similar increase of 40% (Table A.2). Using a similar event-study approach, Alsan and Yang (2022) finds a 25% increase in deportation-related Google searches following SC activation, confirming community awareness of the program.

5.2 Victimization

Our primary outcome is crime victimization, which is our measure of public safety. Table 2 displays the results using equation (1) for both Hispanic and non-Hispanic respondents, and panel (b) of Figure 2 plots the dynamic event-study estimates. The figure highlights that among both Hispanic and non-Hispanic respondents, those living in treated counties (early-treated counties) had comparable trends in victimization rates to those living in control counties (later-treated counties) prior to the introduction of the program, thereby providing support for the parallel trends assumption. After SC, Hispanic individuals become 0.15 percentage points, or 16%, *more* likely to report being victims of crime relative to Hispanic individuals in the comparison group. In contrast, when we consider the victimization rates of non-Hispanic individuals, we find precise null effects. Together, these results run contrary to the explicit goal of the SC program of improving public safety. In fact, the results imply that levels of public safety *worsened* among Hispanic individuals following changes in immigration enforcement. A back-of-the-envelope calculation suggests that the implementation of SC resulted in 1.3 million more crimes against Hispanics in two years than there would have been otherwise.[22]

What does the increase in victimizations among Hispanic respondents imply for the change in the overall level of public safety? If we pool all survey respondents and run an analogous specification, we find an increase in victimization rates of around 3%, though this estimate is not statistically significant. These findings underscore the importance of using data sources that identify a victim's ethnicity to detect changes in offending, especially in contexts in which one racial or ethnic group is disproportionately affected by a policy change. Overall, the estimates rule out declines in victimization larger than 3.3%, indicating that the policy did not generate meaningful improvements in aggregate public safety.

5.3 Willingness to Report Crimes to Police

The second outcome of this study measures the likelihood that a crime victim reports their incident to the police. Recall that the unit of observation is a criminal incident, so the sample size is markedly smaller. Panel (b) of Figure 3 displays the dynamic event-study results, and shows that Hispanic individuals in treated counties had comparable trends in reporting behavior prior to SC's implementation relative to Hispanics in control counties. After the introduction of SC, the likelihood that Hispanic individuals report an incident to the police declines by 9.5 percentage points, or a 30% decline relative to the average reporting rate among Hispanics in the sample (Table 2). Again, we find that SC had no impact on non-Hispanic reporting.

A decline in the likelihood of reporting incidents to the police following an increase in immigration enforcement is consistent with a "chilling effect," whereby individuals are afraid of interacting with law enforcement and are less likely to report victimizations. These results are consistent with Comino et al. (2020) and Jácome (2022), who find increases in Hispanic crime reporting (of 9% and 4%, respectively) following reforms that made the policy environment friendlier toward immigrants. Given that it is relatively easier for police to lose trust than gain trust (e.g., Skogan, 2006), it is unsurprising that our estimated percent change in the likelihood of reporting an incident after the launch of SC is substantially larger than the magnitudes in prior work.

[22] We calculate this number by multiplying the monthly victimization effect by 24 (months) and the Hispanic population in the counties that meet the sampling restrictions (35.3 million). NCVS estimates (Table 1) suggest that there are 35.7 million crimes in a two-year period for all individuals in these counties.

5.4 *Reported* Crime Rates

Next, we consider an outcome that incorporates both changes in victimizations as well as changes in reporting behavior: *reported* crime rates. Importantly, reported crime rates in the NCVS are most similar to measures of crime in other conventional data sources, like the FBI's Uniform Crime Reports (UCR), in which crime is only observed when reported to law enforcement. Accordingly, the results using this outcome can be used as a benchmark for what could be learned from administrative reported crime data.

Panel (b) of Figure 4 shows that the reported crime rate of Hispanic respondents — like that of non-Hispanic respondents — exhibits no changes around the program's introduction. This null effect is the consequence of this outcome masking two opposing causal effects of immigration enforcement, the increase in victimization and the decrease in reporting. For both groups, we find precisely estimated null effects, which are consistent with Hines and Peri (2019), Miles and Cox (2014), and Treyger et al. (2014), all of which rule out any meaningful effects on reported crime rates using administrative data.[23]

Overall, this final exercise stresses the importance of using measures of victimization rates that are isolated from reporting choices in order to detect changes in public safety. This result is especially relevant when considering policies that can change both an offender's incentives to commit crimes as well as a victim's incentives to report crimes.

6 Robustness & Alternative Hypotheses

We consider next the robustness of the main results: the increase in victimization and the decline in reporting among Hispanic individuals.

6.1 Sample Construction and Empirical Specification

We first test the sensitivity of our results to sample construction choices, and report the findings for Hispanic individuals in Table A.4 and Figure A.7. In our baseline sample, we follow Alsan and Yang (2022) and exclude Illinois, Massachusetts, and New York given that these states actively resisted implementation of SC. In row (2) of Table A.4, we include these three states in the sample and shift the county population threshold from 100,000

[23] Table A.3 confirms these findings using agency-level UCR data that aligns with our sampling restrictions and empirical approach. Like previous work, we find small, mostly statistically insignificant, effects of SC on reported crime (the even-numbered columns include agency-specific linear time trends for greater comparability to prior work which includes these controls).

to 75,000.[24] The results are comparable, although smaller in magnitude than those from our baseline sample, consistent with the fact that these additional states resisted SC, and thus their Hispanic residents were likely less responsive to the policy's implementation. In row (3), we keep the original set of states but shift the population threshold to 50,000 and likewise find similar results.

The baseline empirical approach follows Sun and Abraham (2021) and uses the last 25% of counties that activated SC as the comparison group for earlier-treated counties. In practice, this empirical strategy restricts the sample frame, only considering time periods before September 2011 (the date corresponding to when the comparison group begins to be treated). We test the sensitivity of the main results to this restricted time frame by using the final 10% of counties that activated the program as a control group, thereby extending the sample window to March 2012; here, we find nearly identical results to the baseline model. We also estimate a standard two-way fixed effects (TWFE) OLS specification using the baseline time period (ending in September 2011) as well as a fully extended time period (ending in June of 2015). In all of these checks, we find that the results are relatively unchanged.

Next, we report results from a specification that adds individual-level demographic characteristics as control variables. This specification helps alleviate concerns that the results may be driven by the changing composition of respondents or victims over time (we further address this concern in Section 6.3). An additional concern may be that economic conditions were also changing during the program's rollout period due to the Great Recession, and that these changes could simultaneously alter criminal behavior. We check the robustness of the results to including a county's time-varying unemployment rate as an additional control variable.[25] Across all of these checks, the results are highly similar to our baseline findings.

Further, we test the sensitivity of the main results to alternative adapted difference-in-difference models: namely, the approaches of Borusyak et al. (2021) and Callaway and Sant'Anna (2021).[26] Interestingly, the point estimates are actually larger using these strategies, which suggests that our baseline approach using Sun and Abraham (2021) may be more

[24] We are unable to separately add these three states and lower the population cutoff given Census disclosure guidelines that prohibit the presentation of estimates for samples that only differ slightly.

[25] We note that if changing economic conditions were driving the results, then we would also expect to see a victimization increase for non-Hispanics.

[26] One notable difference between our approach using Sun and Abraham (2021) and these alternatives is that Sun and Abraham (2021) implements an earlier vs. later comparison, while these alternatives leverage all units that have yet to be treated.

conservative in our setting. We also estimate an OLS triple-differences specification in which we additionally leverage differences between Hispanic and non-Hispanic respondents, considering the latter as a control group.[27] Across all of these strategies, we continue to find a significant increase in victimization and a decline in reporting among Hispanic respondents. Figure A.9 displays the event-study results from these alternative strategies and shows that the dynamic evolution of outcomes is comparable across these specifications.

Table A.4 and Figure A.5 show the results of analogous specification and sample checks for non-Hispanic respondents. Across all of these variants, we continue to find no impact of the SC program on the likelihood of being victimized or on the likelihood of reporting a crime to police.

Finally, one concern with the empirical strategy is that we focus on the county-specific rollout of the program, so our estimates are identified from changes in treated counties relative to those in not-yet-treated counties. If the program was salient nationwide, beginning with the first activation date, comparisons across counties may miss a national program impact. Figure A.10 shows our main outcomes by calendar time, separately by Hispanic ethnicity, and we denote the first activation month in 2008 with a vertical line. We do not see any evidence of a change in outcomes for Hispanics around the first activation date relative to non-Hispanics. There is, however, evidence that Hispanic victimization increases relative to non-Hispanic victimization beginning in 2010 through 2012, consistent with our main estimates. These plots provide reassurance that our county-level estimates are not missing important national-level impacts that occur with the first activation date.

6.2 Sample Attrition

One concern with using survey data for our outcome measures is that response rates could be directly impacted by program implementation, leading to possible sample selection bias. Specifically, if certain groups of respondents are less likely to respond to the survey after the launch of SC (i.e., a "chilling effect" in survey response), then the estimated increase in victimization could reflect a compositional change among respondents rather than a true increase in victimization.

The sample design of the NCVS allows us to directly measure household response rates to consider this possibility. As described in Appendix C, the NCVS contacts a fixed

[27] To preserve statistical power, this specification controls for "cohort" groups of counties based on the year-month of SC activation instead of individual counties, or Hispanic$\times$time fixed effects, cohort$\times$time fixed effects, and cohort$\times$Hispanic fixed effects.

set of addresses in each survey wave, and the data include information on whether residents at a given address responded to the survey. We can thus run a regression, similar to equation (1), to estimate whether households are less likely to respond to the NCVS after the implementation of SC.

The results of this analysis are presented in Table A.6. Because we do not know a household's ethnic composition if they do not respond to the survey, we use the address' Census tract to perform versions of this analysis in tracts with increasingly larger Hispanic shares of their populations. Each row considers a different sample, starting with all households and then restricting to tracts above the 50th, 75th, and 90th percentile of the tract-level Hispanic share distribution. In all samples, we find small and statistically insignificant coefficients for SC's impact on survey response rates, indicating no change in household response rates after the program's rollout, even in areas with a large Hispanic presence.[28]

Despite the reassuring evidence of minimal attrition, we use the point estimates from these regressions to conduct a back-of-the-envelope calculation of how much bias could be induced from changes in response rates, similar in spirit to Lee (2009). We conduct these calculations in rows (2) through (4), which find negative point estimates, and we refer here to the numbers from row (4) for illustration. Assuming that all SC-induced changes in response rates occur among Hispanic households, we use the share Hispanic to scale up the overall response rate effect, implying a change in Hispanic response rates of -2.4 (or 3%, assuming that all households have an average response rate of 79.1%). We can use the pre-SC Hispanic victimization rate (0.9 percentage points) to calculate the *worst-case* scenario for response bias, which would be that all sample attrition occurs among non-victimized Hispanic respondents. This scenario would imply a victimization rate of 0.928 percentage points, or a 0.028 p.p. increase, which is 18% of our estimated victimization effect. The same estimates in rows (2) and (3) yield similarly small values for the worst-case bias, at most 26% of our estimated effect. These calculations thus suggest that response rate bias cannot explain the observed victimization increase. We note that this exercise is quite conservative, in that it assumes that all respondents who did not respond to the survey were Hispanic *and* were not victimized. It also assumes a material effect of SC on response rates, despite the fact that none of the estimated effects are significantly different from zero.

Next, we leverage the panel nature of the NCVS, which allows us to focus on subsets of survey respondents. Specifically, we can restrict the sample to Hispanic respondents

[28] We note that in this time period, survey response rates were relatively high, at 77%. See Appendix C for details on the survey design and its implementation.

that were present at each of their interviews and thus do not leave the survey (row (12) of Table A.4 and Figure A.7).[29] We also estimate an OLS TWFE model with individual fixed effects, so that the treatment effects are estimated off of individuals interviewed both before and after SC (i.e., those who did not leave the survey after treatment). For both of these checks, we find point estimates that are quite similar to our baseline effects, though the victimization effects are not statistically significant, likely due to reduced power. Despite this reduced precision, the stability of the point estimates using subgroups of individuals who do not leave the survey further corroborates the conclusion that our results are not driven by sample attrition.

6.3 Respondent, Victim, and Crime Composition

We next address a concern closely related to sample attrition. If there are changes in the respondent pool or victimized group that coincide with the policy, our effects could be mechanical artifacts of these compositional changes. For example, East et al. (2018) and Medina-Cortina (2022) find that SC induced an out-migration of low-educated foreign-born men after a county's implementation. We test for the presence and importance of compositional changes in this section and argue that such changes cannot explain our effects.

First, as noted above and shown in Table A.4, we re-estimate the main specification including victim characteristics, such as age, gender, and educational attainment, and the estimates are nearly identical to the baseline effects. The robustness of the results to the inclusion of victim characteristics suggests that respondent and victim composition are uncorrelated with treatment.

Next, we conduct two parallel exercises to probe this issue further. First, we construct measures of *predicted victimization* for each survey respondent based on their demographic characteristics and the victimization patterns prior to SC, during the period of 2005–2007. We then re-estimate equation (1) using predicted victimization as our dependent variable. Figure A.11 reports the findings using several different approaches to construct measures of predicted victimization (i.e., linear regressions, a Lasso procedure, and cell averages). If the increase in victimization was driven by the changing sample composition, then we would also expect to see an increase in predicted victimization. However, we estimate precise null

[29] The NCVS samples addresses for three and a half years, so that the same person responds to the survey multiple times. We restrict the sample to the first household interviewed at each address, and keep the households that responded to every survey and the respondents who responded to all interviews. In this exercise and in others that follow, we minimize Census disclosure risk by using the baseline sample but overweighting subgroups to estimate effects.

effects, implying that the victimization increase is not due to a changing pool of survey respondents.[30]

Likewise, we consider whether the policy may have impacted the composition of victims or incidents and whether such a change could explain the decline in crime reporting that we observe. In particular, if the set of individuals who are victimized after SC differ in their reporting practices or if the composition of crimes changes to include crimes with lower reporting rates, the decline in reporting could be due to compositional changes, rather than behavioral responses in willingness to report crime.[31] To test for this concern, we conduct an analogous exercise to the one above, by constructing *predicted reporting* measures using pre-SC data for crime incidents. The results are presented in Figure A.12, showing that all predicted-reporting coefficients are much smaller than our baseline estimate. While the last three estimates are statistically significant, the largest point estimate is -1.45, over six times smaller than our main reporting effect. These results indicate that changes in victim and crime composition also cannot explain the reporting decline.

6.4 Does Reduced Reporting Translate to Fewer Arrests?

The importance of victim reporting stems from its central role in affecting police effectiveness. A natural question is thus whether the decline in reporting translates into a decline in the probability of an arrested offender. Without a victim report, it is unlikely that the police will be able to identify and apprehend an offender. On the other hand, if the incidents for which victim reporting is reduced have arrest rates that are already quite low, then a change in reporting may not meaningfully translate into declines in arrest rates.

In Table A.9, we present results from estimating equation (1) using an outcome that denotes whether an arrest was made for an incident. We estimate this regression separately on the sample of all victimizations and all *reported* victimizations in the NCVS. The first panel shows a negative point estimate for the arrest impact among all incidents with Hispanic victims. Off a base of 4.4%, the coefficient of -1.63 (S.E.$=1.2$) corresponds to a 37% effect size. Because the outcome is relatively rare, this coefficient is imprecisely estimated and marginally insignificant (p-value$=0.17$). However, it is noteworthy that the magnitude of the implied decline is comparable to the decline in reporting. The second row restricts

[30] The standard NCVS weights include a survey attrition adjustment, so Figure A.11 presents an estimate using alternative weights that do not adjust for attrition (results are nearly identical).

[31] Note that we find large, significant reporting declines when focusing on "always-responders" and when using individual-fixed effects (Table A.4), ruling out that the effect is driven by respondents with lower reporting rates entering the survey after SC.

attention to reported victimizations and shows a statistically insignificant coefficient of -2.00 (S.E.$= 2.8$) off a base of 8.97%, corresponding to a 22% effect size. This estimate reflects the degree to which SC changed the arrest rate conditional on a report to the police. Because both coefficients are imprecisely estimated, we are limited in how much we can conclude from these figures. However, the magnitude of effect sizes provides suggestive evidence that the decline in reporting translated to a lower arrest rate overall, but not necessarily among reported incidents. In Section 8, we show related evidence from police administrative data that the volume of arrests did not change with the policy.

7 Heterogeneity

In this section, we explore how the results vary by crime type, gender of respondents, county characteristics, and neighborhood characteristics.

7.1 Crime Type

We separate victimizations into violent crimes (i.e., rape and sexual assault, simple and aggravated assault, robbery, and verbal threats) and property crimes (i.e., burglary, theft, and larceny) to see whether the changes in victimizations and in victim reporting differ by crime type. Table 3 displays the results showing that the decline in reporting is primarily driven by a large and significant (34%) decline in Hispanic victims' willingness to report a property offense to the police, with no analogous significant decline in the likelihood of reporting violent offenses. These results suggest that the heightened levels of immigration enforcement dissuaded Hispanic individuals from contacting law enforcement over relatively less-serious, non-violent matters. We similarly find that the increase in victimization is driven by an increase (15%) in property crime victimizations of Hispanic individuals. To the extent that criminal offenders alter their behavior in response to changes in the probability of apprehension, then it is consistent to see an increase in *property crime* victimizations.[32] Finally, we also find a 15% increase in violent crime victimizations, though the estimate is not statistically different from zero, potentially due to reduced power for this outcome.

[32] Hines and Peri (2019) does not find changes in reported violent crime rates, but does find statistically significant (albeit small) increases in reported property crime rates.

7.2 Gender

We further separate program impacts by gender in Table A.7. The victimization point estimate for Hispanic males is positive (0.085 p.p.) but imprecise (S.E.= 0.088). For female respondents, the victimization effect is over 2.5 times larger, so that the likelihood that a Hispanic female is victimized increases by 0.23 p.p. (or 25%) after the implementation of SC. In contrast, the size of the decline in reporting is comparable across males and females. Collectively, this analysis suggests that, while fear inspired by immigration enforcement is likely similar for both gender groups, the increase in victimization is disproportionately borne by female victims.

7.3 Neighborhood Characteristics

We next consider whether individuals who live in neighborhoods with high shares of Hispanic residents may have differing treatment effects. Figure A.13 plots treatment effects according to neighborhood characteristics, separately for Hispanic and non-Hispanic respondents. Specifically, we use a respondent's Census tract to estimate effects for respondents living in neighborhoods with high shares of Hispanic and non-citizen Hispanic residents (i.e., above the 50th, 75th, and 90th percentile in the corresponding tract-level distributions).

Panel (a) shows that for Hispanic individuals, victimization effects appear relatively constant as the neighborhood resident share of Hispanics (or non-citizen Hispanics) increases, though SC's impact on victimization is higher — a 25% increase — in neighborhoods with the highest shares of non-citizen Hispanics. Moreover, the decrease in reporting for Hispanics in panel (b) also appears to be comparable across neighborhood types, suggesting a relatively uniform decline in reporting behavior.

For non-Hispanic individuals, a different picture emerges in panels (c) and (d). For this group, the likelihood of being victimized generally increases and the likelihood of reporting crimes to police decreases as neighborhoods become more Hispanic (or non-citizen Hispanic). This pattern is consistent with some offenders targeting Hispanic neighborhoods after the policy — potentially because the probability of arrest has declined in these places given a reduction in crime reporting — thereby increasing victimizations of non-Hispanics in these areas. Further, the reporting effects suggest that non-Hispanic victims in these neighborhoods may decrease their willingness to report crimes, potentially due to concerns about the rising threat of deportation for their Hispanic neighbors.

Given these results, we return to the baseline sample and consider how results for non-Hispanic individuals change if we allow their geographic composition to mirror that of Hispanic respondents. Specifically, we re-estimate the baseline specification for non-Hispanic respondents but re-weight respondents to reflect the county composition of Hispanic respondents. We find an 8% increase in victimizations for non-Hispanic respondents and no change in their reporting behavior (results shown in Table A.5 and Figure A.8). These results suggest that although the decline in public safety was most concentrated among Hispanic individuals, non-Hispanic residents in counties with large Hispanic populations were also more likely to be victimized following the increase in immigration enforcement.

7.4 County Characteristics

Next, we explore how the SC program's impacts vary across counties. We first document overall variation in county-level impacts by estimating equation (1) separately for each county in the earlier-treated group while continuing to use the later-treated counties as the comparison group. We generate an estimate $\hat{\beta}_c$ and standard error $\hat{\sigma}_c$ for SC's impact in each county c for each outcome and ethnicity group. The empirical distribution of estimated effects includes noise from estimation error and thus may overstate the degree of variation in county-level impacts. We address this issue with a deconvolution procedure (Goncalves and Mello, 2021; Kline et al., 2022), which we describe in Appendix D.

Figure A.14 displays the estimated densities of treatment effects, as well as the corresponding "deconvolved" density of treatment effects. The victimization (reporting) panels report the estimated share of counties with a positive (negative) effect. The distributions show that while the mean impacts align with our baseline results (red vertical lines), there is significant variation in effect sizes. The only outcome where effects are strongly concentrated in one direction is reporting among Hispanics: we estimate that 79% of counties have a negative treatment effect. Although the distribution of Hispanic victimization effects has more dispersion — a sizable share of counties have both large positive and negative impacts — we find that a high share (68%) of counties have a positive treatment effect. For non-Hispanic respondents, the outcome distributions are, unsurprisingly, centered around zero.

These distributions provide a natural motivation for exploring whether county-level characteristics are predictive of the magnitude of SC's impacts. Table A.8 first assesses whether the intensity of immigration enforcement varied across counties with different characteristics, using ICE removals (deportations) in the two-year post-period as a measure of the first stage. Column (1) shows that counties with a higher share of non-citizen Hispanic residents have higher removals per capita, or greater *total* immigration enforcement.

We next explore whether the victimization effects are likewise a function of county characteristics. We return to the baseline NCVS sample and estimate regressions similar to equation (1), but allowing the effect of β_{Post} to vary with county characteristics (columns

(3)-(5)). Counties with higher non-citizen Hispanic shares — which had higher levels of total immigration enforcement — have higher victimization effects.[33] For reporting, we find minimal evidence that county characteristics predict differences in the treatment effects among Hispanic individuals. These findings accord with the distribution analysis above, in which we found limited variation in the decline in Hispanic reporting across counties.

8 Supporting Evidence from Police Administrative Data

The central results of this paper use the NCVS, which is uniquely able to disentangle impacts on victimization and crime reporting separately by respondent ethnicity. While the NCVS provides a detailed accounting of victimization, it does not provide thorough information on *offenders*. In particular, most victims in the survey are unable to provide information about the offender, and ethnicity information about offenders is limited.[34]

To better understand the offender population, we augment our analysis using a novel data collection from individual U.S. police departments. We acquired micro-data on 911 calls and arrests from 75 departments for 2006–2013. These data also allow us to corroborate the NCVS estimates of impacts on reported crime using civilian calls to police. Every 911 observation records the date and time of the incident, as well as a basic description of the call type. Each arrest observation also records the date and time when the arrest occurred, as well as basic demographic information on the arrested individual including age, gender, and race/ethnicity. Importantly, these data include the address of the incident so that we can investigate differences by neighborhood type. We provide information on these data and our data cleaning and sample selection choices in Appendix E.

Our goal is to understand the impact of SC on the monthly volume of calls to the police and arrests made, as well as the ethnic composition of arrestees. Because we do not observe the ethnicity of the victim/caller, we split our analysis by neighborhood ethnic composition. Specifically, we designate the 25% of Census tracts in our sample with the highest Hispanic

[33] We also consider the share of removals from felony offenses, reflecting "targeted" enforcement toward serious offenders. Counties with higher shares of Hispanics tend to have more targeted enforcement. This pattern may imply that conditional on the non-citizen Hispanic share (the population susceptible to immigration enforcement), Hispanic voters may have a preference for targeted enforcement. These counties also have lower victimization effects, providing suggestive evidence that victimization could potentially decrease when serious offenders are targeted.

[34] For violent crimes (17% of victimizations), 40% of crimes were committed by strangers (Harrell, 2012). For theft and larceny, 84% of victims reported that the offender was a stranger or they did not know the number of offenders (Bureau of Justice Statistics, 2024). The NCVS added offender ethnicity in 2012, preventing us from learning about Hispanic ethnicity during our sample period.

population share as "Hispanic neighborhoods" and the remaining tracts as "non-Hispanic neighborhoods." The cutoff Hispanic share for Hispanic neighborhoods is 42%, with the average share being 68%. The average Hispanic share for the non-Hispanic neighborhoods is 14%.

We run a series of regressions using the same specification as equation (1), in which the unit of observation is a tract in a given year-month and we include tract and time fixed effects. We again follow Sun and Abraham (2021), using tracts in counties treated after September 2011 as our control group. Table 4 presents the findings separately for Hispanic and Non-Hispanic neighborhoods. Panel (a) shows that SC did not change the volume of 911 calls across any neighborhood types, and panel (b) similarly shows no change in the volume of arrests. These results closely align with our findings using NCVS data that SC did not change reported crime rates or arrest rates conditional on a reported crime.

Panel (c) shows the effects of SC on the Hispanic share of arrestees. The outcome mean shows that the Hispanic share of arrestees closely matches the overall tract composition. Across all tract types, the Hispanic share declines after SC. In Hispanic neighborhoods, there is a 1.5 p.p decline off a base of 54%, or a 2.7% decline.

Because the NCVS results indicate an increase in offending, one question is how the "marginal" offender from SC — who is induced to offend because of the policy — differs from the individuals who would have offended regardless. In Appendix F, we describe a procedure for using the change in the composition of arrestees, alongside our previous estimate of the policy's impact on offending, to infer the marginal offender. This exercise assumes that Hispanic victims' reporting decline was not a function of offender ethnicity and that arrest rates conditional on victim reporting do not depend on offender ethnicity. We calculate that 43% of marginal offenders in Hispanic neighborhoods are Hispanic, in contrast to the 54% Hispanic share of offenders prior to SC. Put differently, those who offend in response to the policy are *less* likely to be Hispanic than the offenders pre-SC. This decline is consistent with our conceptual framework: reduced reporting promotes higher offending, but Hispanic offenders now also face a higher expected cost of committing crime given the possibility of being detained and deported (see Appendix B). The composition of offenders should thus change to have relatively fewer Hispanics.

9 Is the Reporting Decline Driving the Increase in Victimization?

We have presented evidence that the Secure Communities program decreased the reporting rate of Hispanic crime victims and increased Hispanic victimization, impacts which may be directly linked. Since Becker (1968), economists have recognized that offender behavior depends on the probability of apprehension, which is tied to victim reporting. However, in addition to affecting crime victims' reporting probabilities, the Secure Communities program led to multiple economic and social changes within Hispanic communities (Ali et al., 2022; East et al., 2018; Medina-Cortina, 2022), each of which may have had an important impact on public safety. In this section, we present several pieces of evidence that suggest that the reporting decline was a key driver of increased victimization.

9.1 Relationship between Victimization and Reporting Effects

First, we use our county-level program effects from Section 7.4 and ask whether counties with larger reporting declines experienced larger victimization increases. Each county's impacts are estimated with error, so a raw correlation coefficient will likely understate the relationship. We extend our deconvolution procedure to estimate the joint-distribution of reporting and victimization impacts (described in Appendix D), and we find a strong correlation of $\rho = -0.33$ (S.E. $= 0.17$) between the effects.

To improve precision in our location-specific impacts, we re-estimate equation (1) among Hispanic respondents separately for each earlier-treated *cohort* (i.e., counties that activated the program in the same year-month), using Hispanic respondents in later-treated counties as the comparison group. Each cohort includes multiple counties, and their estimates are thus more precise than individual county effects.

Figure 5 shows a negative and statistically significant relationship between victimization and reporting effects across cohorts, consistent with reporting being a key input into public safety. Cohorts that experienced larger declines in reporting also experienced larger victimization increases: the slope of the regression line suggests that for a 10 p.p. decline in reporting, we would expect to see a 0.2 p.p. increase in the victimization rate (slightly larger than our baseline findings).[35]

9.2 Decline in Reporting vs. Other Economic and Social Changes

Next, we explore the relative importance of the SC program's economic and social impacts — vis-à-vis the decline in reporting — for the increase in Hispanic victimization. Intuitively, the goal of this exercise is to estimate SC's impact on these economic and social outcomes and to

[35] One concern might be that the negative correlation arises mechanically from the presence of dynamic treatment effects and different observable post-period lengths (i.e., earlier treated counties have larger victimization and reporting effects precisely because of their longer observable post-period). However, when we estimate the county-level correlation using one-year effects, we find a larger correlation ($\rho = -0.415$), suggesting that this concern is not driving the relationship.

subsequently ask: how large of a victimization response would we expect to see based on the elasticities of crime with respect to each of these outcomes from the existing literature? And analogously, how large of an increase in victimization would we have expected to see based on the decline in reporting and the corresponding elasticity from prior work? We summarize the approach and results here, but Appendix G provides more details.

Framework — We consider a set of outcomes that, while parsimonious, capture the fact that Secure Communities impacted both social and economic outcomes in Hispanic communities. Specifically, we consider the reporting rate of Hispanic victims; the employment-to-population ratio and logged hourly wage of low-educated foreign-born Hispanics (following East et al., 2018); the share of Hispanic household heads that are female; and the population share of male low-educated foreign-born Hispanics. All of these outcomes are impacted by Secure Communities and could have consequences for victimization.

We follow the mediation analysis framework and notation of Heckman et al. (2013) and Fagereng et al. (2021) to model how victimization relates to this set of outcomes (i.e., "mediators") impacted by Secure Communities. We index treatment status by the subscript 0 or 1 and observed mediators by the superscript j. Specifically, we can decompose the overall effect of SC on victimization, V, into a component explained by observed mediators θ^j and a "residual" term:

$$\mathrm{E}[V_1 - V_0] = \underbrace{\sum_{j \in \mathcal{J}_p} \alpha^j \mathrm{E}[\theta_1^j - \theta_0^j]}_{\substack{\text{Treatment effect due} \\ \text{to observed mediators}}} + \underbrace{\mathrm{E}[\tau_1 - \tau_0]}_{\substack{\text{Treatment effect due} \\ \text{to unobserved mediators}}} \tag{3}$$

The left-hand side of this equation is the overall victimization effect of Secure Communities, reported in Table 2. Our goal is to quantify the first expression on the right-hand side. To do so, we need estimates of $\mathrm{E}[\theta_1^j - \theta_0^j]$, measuring the effect of Secure Communities on each mediator variable, as well as estimates of α^j, measuring the effect of each mediator on victimization.

Results — We begin by estimating the effect of SC on the mediators, corresponding to the $\mathrm{E}[\theta_1^j - \theta_0^j]$ terms in equation (3). The effect of SC on Hispanic victim reporting behavior has already been discussed and is shown in Table 2. For the remaining mediators, we estimate SC impacts using equation (1) at the annual level and our baseline set of counties in the American Community Survey (ACS). Column 3 of Table A.10 reports the results indicating county-level declines in employment, wages, and the population share of men as well as an increase in the share of household heads that are female.

Next, we calculate the effects of the mediators on victimization (i.e., the α^j terms) by using existing elasticities from the literature that link these mediators to crime rates. Specifically, we use estimates from Golestani (2021), Gould et al. (2002), Glaeser and Sacerdote (1999), and Chalfin and Deza (2020) to calculate the elasticities of crime with respect to each of the mediators (column 1 of Table A.10). We then calculate the implied effect of each mediator on victimization given these elasticities (column 2).

We choose to borrow estimates of α^j from the literature, rather than estimate them from our own data, for two primary reasons. First, we do not have separate sources of exogenous variation for the mediators in our sample. In contrast, the studies we consult all use instruments to estimate the causal effect of each mediator on crime. Second, reporting rates in the NCVS are necessarily measured with error. A regression relating victimization rates and measured reporting rates will likely have an attenuated estimate relative to the true relationship.

The results from this decomposition exercise — also shown in Figure 6 and the final column of Table A.10 — offer three main takeaways. First, the decline in reporting that we document in this paper is substantial, and the existing elasticities from the literature would have predicted an even *larger* victimization effect than the one we find. Our choice of elasticity for reporting, borrowed from Golestani (2021), is conservative relative to comparable estimates from Miller and Segal (2019). Indeed, even with a conservative elasticity of crime to reporting, we would have predicted a victimization effect almost twice the size of our estimated effect. Second, although Secure Communities impacted other important outcomes that are related to crime, their predicted effect on victimization — based on elasticities from prior work — is relatively tiny. We thus conclude that economic and demographic responses to SC, while important outcomes in their own right, are not important drivers of increased Hispanic victimization.

Finally, our decomposition shows a notable negative "residual" effect. This finding is the converse of our first conclusion that the reporting decline predicts a greater-than-observed victimization increase. As we have noted, one of the key hopes for the Secure Communities program was that, by raising the risk of detention and deportation for unauthorized immigrants who commit crimes, it would deter offending. The large and negative residual impact provides suggestive evidence that, absent the policy's impact on victim reporting, offending may have gone down from the deterrence effect of greater sanctions.

9.3 Did Racial Animus Cause the Crime Increase?

Another possible driver of the rise in victimization is an increase in racial animus. If the policy raised the salience of illegal immigration as a political issue, it may have induced some individuals to engage in crimes targeted against Hispanics due to animus, rather than as a strategic response to lower reporting.[36]

Several pieces of suggestive evidence point against animus being the key driver of our results. First, after considering the role of a set of observable mediators in explaining the victimization increase in the previous subsection, the residual impact is negative. This fact indicates that the entire increase in victimization can already be explained by factors other than animus. Second, we estimate that roughly 40% of marginal offenders induced by the policy are Hispanic. While this share is lower than the share Hispanic among pre-SC offenders, it suggests that the increase in offending is not due solely to non-Hispanics, the group that would be the likely perpetrators of animus-driven crimes against Hispanics. Third, we do not find evidence of a higher Hispanic victimization effect in counties with a higher Republican vote share (Table A.8), where anti-immigrant sentiments are likely more common.[37] Finally, we find an increase in non-Hispanic victimization in counties (and neighborhoods) with larger Hispanic populations (Table A.5, Figures A.8 and A.13), suggesting that the victimization increase is not strictly determined by ethnicity. Together, these facts lead us to conclude that racial animus is unlikely to be a key driver of the policy's impact on victimizations.

10 Conclusion

Debates about immigration policy often center on concerns related to immigrant criminality, and views on these issues can affect broader beliefs and behavior as well as political polarization (e.g., Afrouzi et al., 2023; Ajzenman et al., 2023; Alesina et al., 2023; Alesina and Tabellini, 2022; Couttenier et al., 2021; Dustmann et al., 2019; Doherty et al., 2016, 2020). Within this debate, immigration enforcement policies are often promoted as a way to improve public safety.

We study increases in immigration enforcement resulting from the federal Secure Communities Program, which was implemented piecemeal at the county level between 2008 and

[36] In a separate setting, Grosjean et al. (2023) finds that after Trump political rallies that contain inflammatory speech about racial issues, police are more likely to stop Black drivers.

[37] See, for example, Afrouzi et al. (2023) and "Americans Still Value Immigration, but Have Concerns," *Gallup*, July 13, 2023.

2013. In sharp contrast to the goals of the program, we find that Hispanic residents are significantly more likely to be victims of crimes after program implementation. We also find a significant reduction in the crime reporting rate of Hispanic victims, consistent with a "chilling" effect of greater enforcement, and we argue that the decline in reporting is a key driver of increased Hispanic victimization.

These findings offer several implications for future research and policy. First, we show that the tracking and measurement of public safety outcomes can be distorted by changes in victim reporting. Administrative crime records typically only measure *reported* crime and contain coarsely aggregated outcomes. In our setting, reported crime does not change because the decline in victim reporting masks the increase in true victimization. Further, our findings stress the importance of collecting and utilizing data sources with granular demographic information in order to obtain a more complete understanding of the impact of policies that target certain subgroups. Our analysis shows that estimating the impact of Secure Communities on the full population obfuscates its effect on Hispanic individuals, a group that is 15% of the U.S. population.

Second, our study provides evidence that lower levels of trust in law enforcement, which can manifest through reduced reporting of crimes, can hinder public safety. This finding highlights an important trade-off between the ability of sanctions to decrease crime through deterrence or incapacitation and the potential for excessive enforcement to degrade public faith in law enforcement.

Finally, the efficacy of government, including law enforcement, can be hampered by the ways in which policies may deteriorate trust in public institutions. Our work shows that mistrust effects can be large and can develop quickly. Future research should thus explore how to design effective public safety policies, which may include immigration enforcement policies, that do not generate fear among victims and witnesses. American confidence in law enforcement has decreased in recent years (Kennedy et al., 2022), and future work should investigate ways in which trust can be improved.

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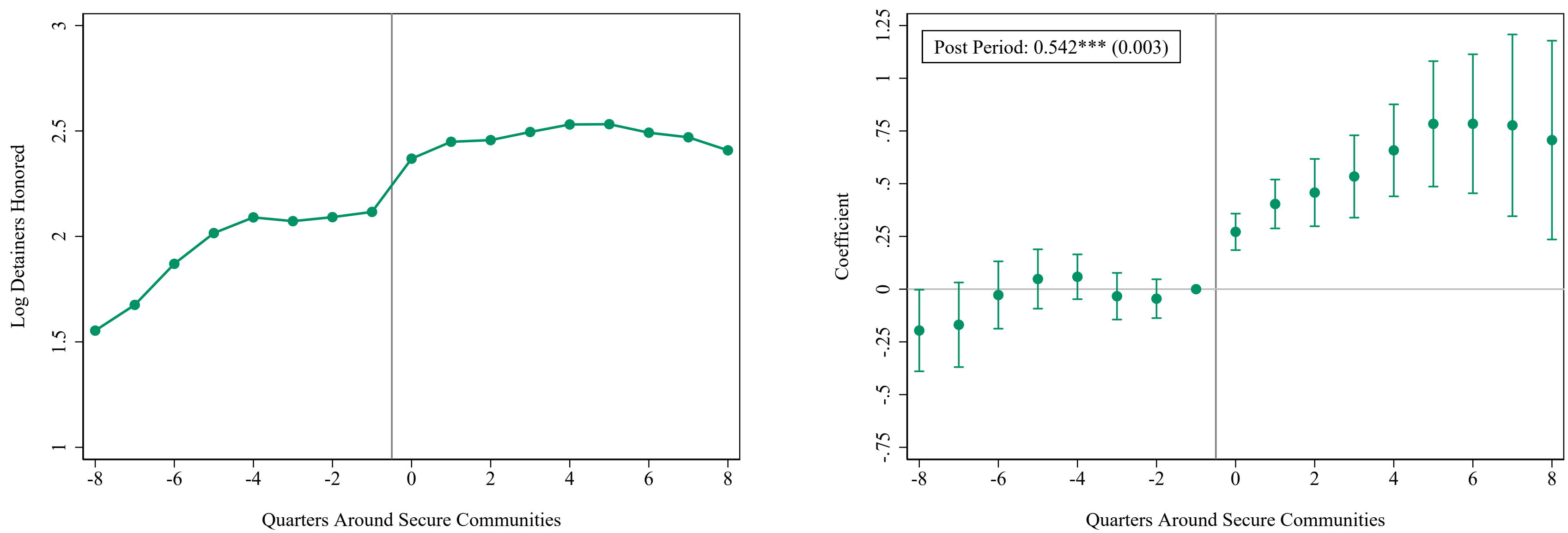
Figure 1: Logged Number of Honored ICE Detainer Requests around Secure Communities (SC) Implementation

(a) Raw Means

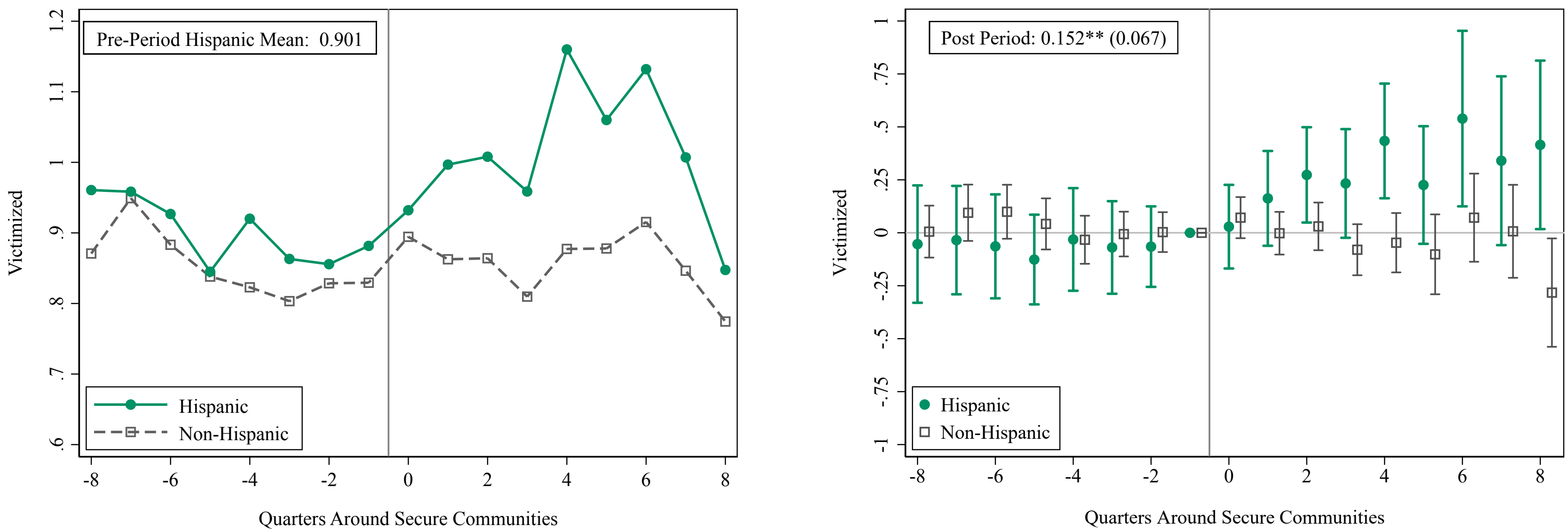
(b) Regression Estimates



Note: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the logged number of honored ICE detainers for the eight quarters before and after the implementation of the Secure Communities program. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. Honored detainers are available in both the pre- and post-period and are used in this study as a proxy for ICE removals (deportations). The sample of counties utilized in this figure follows the NCVS sample restrictions described in Section 3.1. Panel (b) plots the dynamic difference-in-differences estimates using equation (2) and reports the β_{Post} estimate and corresponding standard error from equation (1). Panel (b) utilizes later-treated counties as the control group for estimating the treatment effects of Secure Communities on the number of honored detainers in earlier-treated counties (Sun and Abraham, 2021). In panel (b), average outcomes corresponding to the first and last time period ($\tau = -8$ and $\tau = 8$) reflect averages for all time periods before and after that quarter, respectively. In both panels, estimates are weighted by the county's population in that year (U.S. Census Bureau, 2022).

Figure 2: Share of Persons Victimized around Secure Communities (SC), by Hispanic Ethnicity

(a) Raw Data (b) Regression Estimates

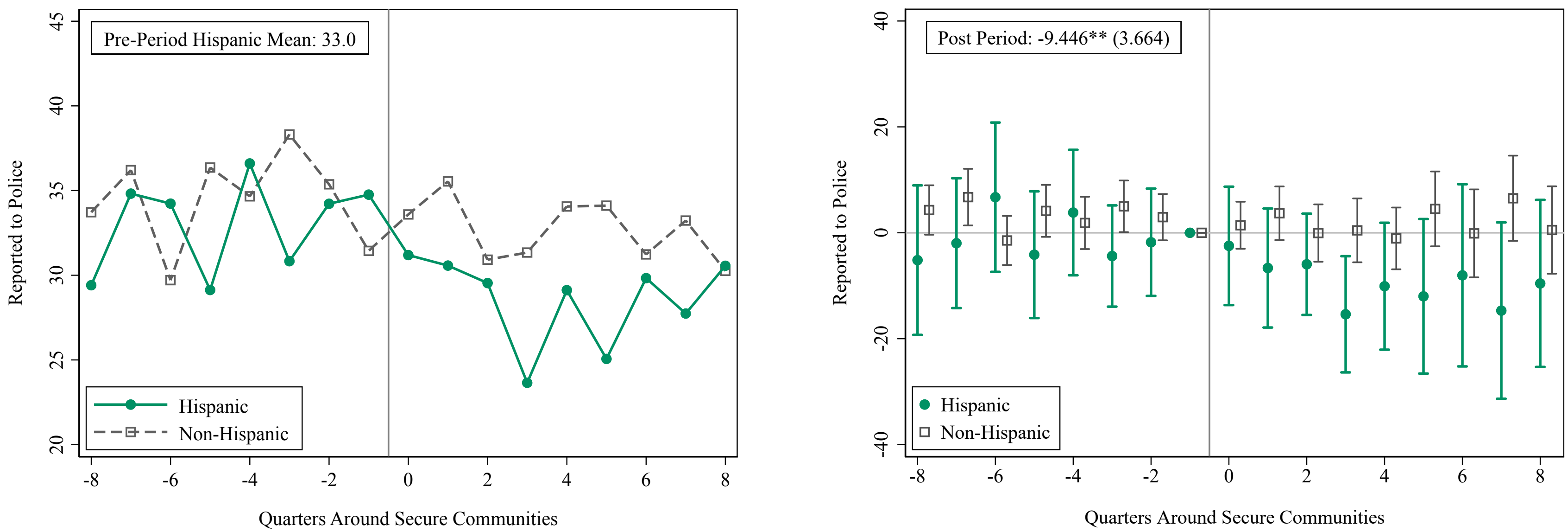


NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS respondents victimized for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3.1. Panel (b) plots the dynamic difference-in-differences estimates using equation (2) and reports the β_{Post} estimate and corresponding standard error from equation (1). Panel (b) utilizes later-treated counties as the control group for estimating the treatment effects of Secure Communities on the share of persons victimized in earlier-treated counties (Sun and Abraham, 2021). In panel (b), average outcomes corresponding to the first and last time period ($\tau = -8$ and $\tau = 8$) reflect averages for all time periods before and after that quarter, respectively. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

Figure 3: Share of Crimes Reported to Police around Secure Communities (SC), by Hispanic Ethnicity

(a) Raw Data

(b) Regression Estimates

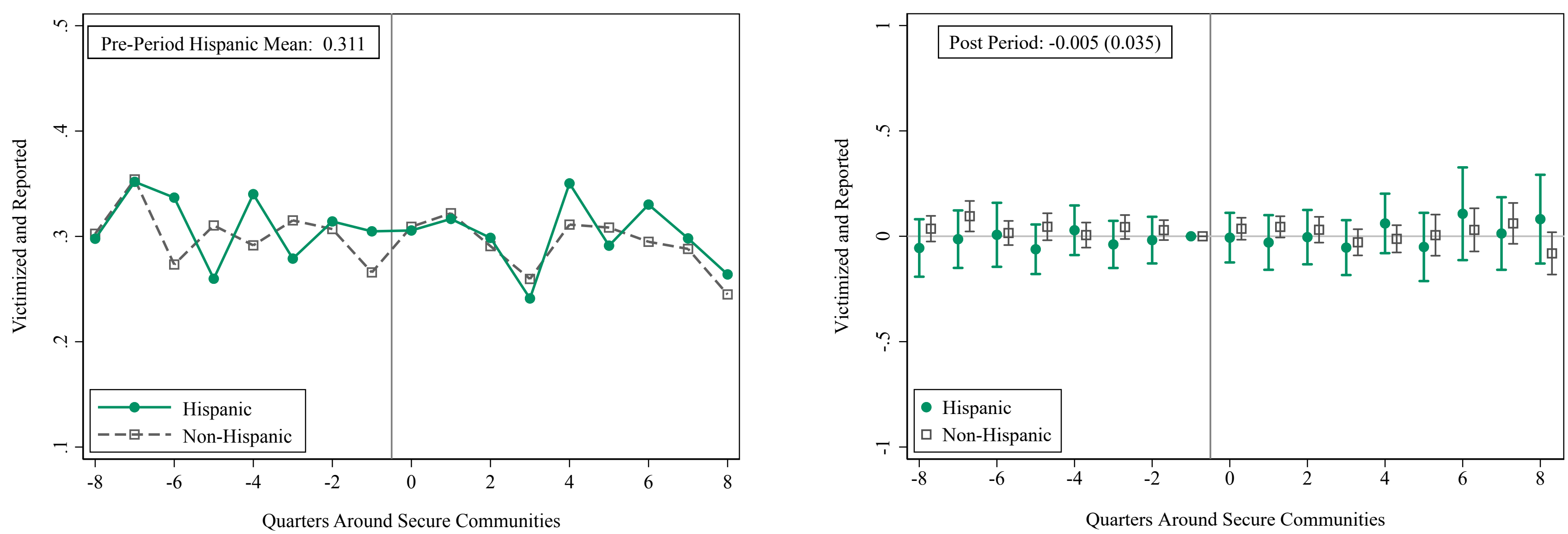


Note: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS crime incidents for which a victim reported the crime to police for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3.1. Panel (b) plots the dynamic difference-in-differences estimates using equation (2) and reports the β_{Post} estimate and corresponding standard error from equation (1). Panel (b) utilizes later-treated counties as the control group for estimating the treatment effects of Secure Communities on the share of crimes reported to police in earlier-treated counties (Sun and Abraham, 2021). In panel (b), average outcomes corresponding to the first and last time period ($\tau = -8$ and $\tau = 8$) reflect averages for all time periods before and after that quarter, respectively. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

Figure 4: Share of Persons Victimized who Reported Crimes to Police (*Reported* Crime Rate) around Secure Communities (SC), by Hispanic Ethnicity

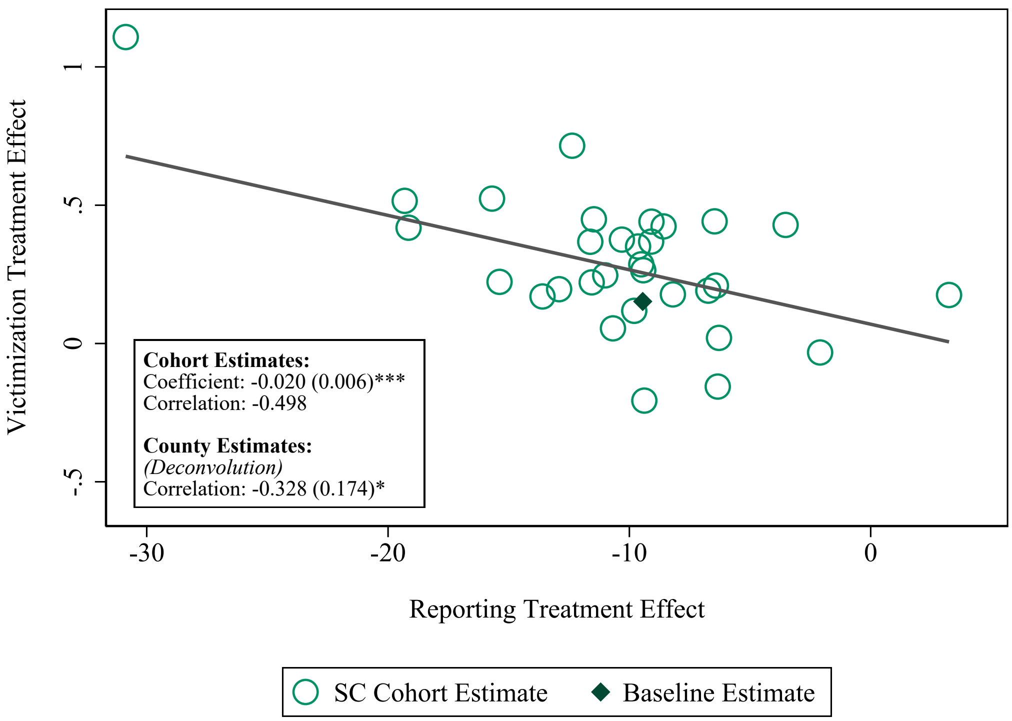
(a) Raw Data

(b) Regression Estimates



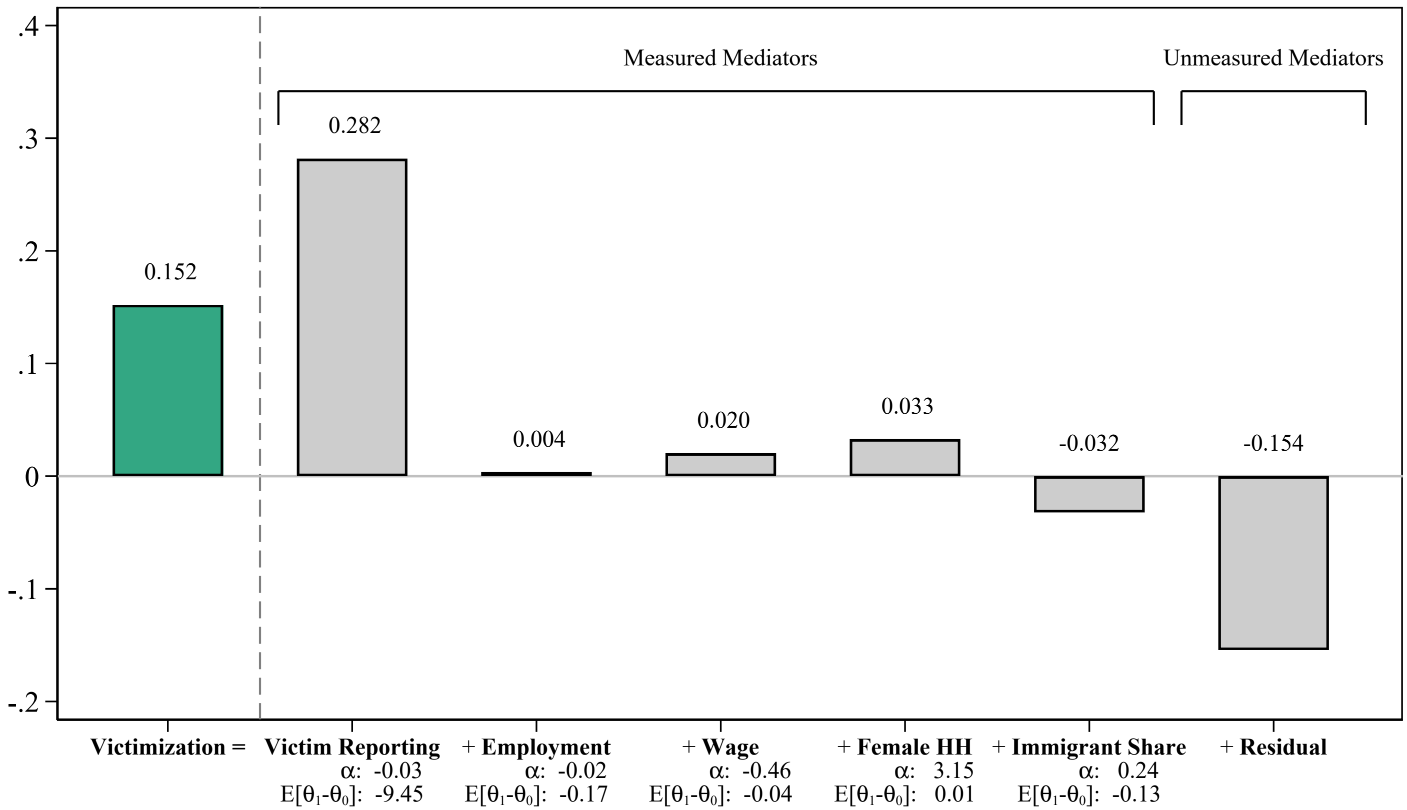
NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS respondents who were both victimized and reported the crime to police for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3.1. Panel (b) plots the dynamic difference-in-differences estimates using equation (2) and reports the β_{Post} estimate and corresponding standard error from equation (1). Panel (b) utilizes later-treated counties as the control group for estimating the treatment effects of Secure Communities on the reported crime rate in earlier-treated counties (Sun and Abraham, 2021). In panel (b), average outcomes corresponding to the first and last time period ($\tau = -8$ and $\tau = 8$) reflect averages for all time periods before and after that quarter, respectively. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

Figure 5: Victimization Effect vs. Reporting Effect, by Secure Communities (SC) Cohort



Notes: *p<0.1, **p<0.05, ***p<0.01. This figure plots the victimization treatment effects against the reporting treatment effects separately for each Secure Communities (SC) cohort. A cohort refers to counties that activated SC in the same year and month. Each circle reflects a cohort's β_{Post} estimate from equation (1) for the victimization and reporting outcomes. The diamond marker refers to the estimates that group all cohorts (see Table 2). The sample of counties utilized in this figure follows the NCVS sample restrictions described in Section 3.1 and we utilize later-treated counties as the control group for estimating the treatment effects of Secure Communities in earlier-treated counties (Sun and Abraham, 2021). The figure reports the coefficient and standard error of a regression of the cohort-specific victimization treatment effect on the corresponding reporting treatment effect. We also report the corresponding cohort-level pairwise correlation. Both of these estimates are weighted by one over the square of the victimization effect's standard error. "County Estimates (Deconvolution)" refers to the county-level correlation between reporting and victimization effects derived from a two-dimensional deconvolution exercise (see Appendix D for details).

Figure 6: Decomposing Victimization Increase into Various Components



Notes: This figure presents estimates from the decomposition outlined in Section 9.2 and also depicted in Table A.10. The left-most estimate corresponds to the effect of Secure Communities (SC) on crime victimization (Table 2). The remaining bars depict the predicted effect of each mediator on victimization. The α estimates under each bar correspond to the implied effect of the mediator on victimization using elasticities from the literature. The $\mathrm{E}[\theta_1 - \theta_0]$ estimates correspond to the effect of SC on each mediator. The effect of SC on victim reporting comes from the NCVS (Table 2). We estimate the effect of SC on the other mediators using the 2005–2014 American Community Surveys (ACS). "Residual" refers to the part of the total victimization effect that cannot be explained by the five mediators. For more details on these calculations, see Appendix G.

Table 1: Summary Statistics of NCVS Sample, by Hispanic Ethnicity

	All Respondents *(Person-Months)*			**Crime Victims** *(Incidents)*		
	All	Hispanic	Non-Hispanic	All	Hispanic	Non-Hispanic
White	0.66	0.00	0.78	0.62	0.00	0.74
Black	0.12	0.00	0.14	0.15	0.00	0.18
Hispanic	0.15	1.00	0.00	0.17	1.00	0.00
Female	0.53	0.52	0.53	0.52	0.50	0.52
Age	44.99	37.89	46.29	39.41	34.55	40.42
HS Degree or Less	0.45	0.67	0.41	0.45	0.63	0.42
Some College	0.25	0.19	0.26	0.31	0.25	0.32
BA or More	0.28	0.12	0.31	0.23	0.11	0.25
Student	0.17	0.22	0.16	0.21	0.23	0.21
Employed	0.56	0.56	0.56	0.60	0.61	0.59
Married	0.52	0.50	0.53	0.38	0.43	0.37
Urban Resident	0.91	0.97	0.90	0.94	0.98	0.93
Rural Resident	0.09	0.03	0.10	0.06	0.02	0.07
HH Inc. $<$ \$30k	0.17	0.26	0.15	0.26	0.30	0.25
HH Inc. \$30k-\$50k	0.15	0.20	0.14	0.17	0.20	0.16
HH Inc. \$50k-\$75k	0.13	0.12	0.13	0.11	0.11	0.12
HH Inc. $>$ \$75k	0.25	0.14	0.28	0.21	0.13	0.22
Victimized ($\times$100)	0.86	0.96	0.84	100.00	100.00	100.00
Victimized: Violent ($\times$100)	0.15	0.16	0.15	18.05	15.97	18.48
Victimized: Property ($\times$100)	0.71	0.82	0.69	81.75	83.86	81.30
Reported to Police ($\times$100)	30.58	31.80	30.36	34.41	31.58	35.00
Persons	170,000	28,500	141,000	17,500	3,000	14,500
Observations	2,541,000	391,000	2,150,000	23,500	4,100	19,500

Note: This table displays summary statistics for our baseline sample in the National Crime Victimization Survey (NCVS). The first three columns report summary statistics (averages) among NCVS respondents in the baseline sample (the dataset is at the person $\times$ year $\times$ month level corresponding to the years and months for which a respondent is answering). The final three columns report averages for individuals who have been victimized (the dataset is restricted to records of crime incidents). In all columns, measures of victimization and crime reporting have been multiplied by 100. All characteristics are denoted using indicator variables and missing values are counted as zeros. Observation numbers and estimates have been rounded following Census disclosure guidelines.

Table 2: Effect of Secure Communities (SC), by Hispanic Ethnicity

	β_{Post}	(S.E.)	Y Mean	N
A. Hispanic				
Victimized	0.152**	(0.067)	0.96	391,000
Reported to Police	-9.446**	(3.664)	30.98	4,100
Victimized and Reported	-0.005	(0.035)	0.31	391,000
B. Non-Hispanic				
Victimized	0.003	(0.035)	0.87	2,150,000
Reported to Police	-1.112	(1.343)	34.50	19,500
Victimized and Reported	-0.002	(0.016)	0.31	2,150,000
C. Total				
Victimized	0.030	(0.033)	0.88	2,541,000
Reported to Police	-2.247*	(1.247)	33.89	23,600
Victimized and Reported	-0.001	(0.015)	0.31	2,541,000

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1). The estimate β_{Post} and standard error correspond to an indicator variable equal to one in the eight quarters following the implementation of the SC program. This table considers the baseline sample of NCVS respondents and uses individuals in later-treated counties as the control group for estimating the treatment effects of Secure Communities on the outcomes of individuals in earlier-treated counties (Sun and Abraham, 2021). Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness. "Y Mean" refers to the average of the outcome variable in that specification. All outcomes are multiplied by 100 for ease of exposition (scale 0 to 100). Observations have been rounded following Census disclosure guidelines.

Table 3: Effect of Secure Communities (SC)
for Hispanic Individuals, by Crime Type

	β_{Post}	(S.E.)	Y Mean	N
A. Violent Crime				
Victimized	0.024	(0.025)	0.16	391,000
Reported to Police	-3.683	(6.363)	34.71	650
Victimized and Reported to Police	0.006	(0.015)	0.06	391,000
B. Property Crime				
Victimized	0.122**	(0.056)	0.82	391,000
Reported to Police	-9.321**	(4.134)	31.04	3,400
Victimized and Reported to Police	-0.015	(0.031)	0.27	391,000

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1) among Hispanic respondents for different crime types. The estimate β_{Post} and standard error correspond to an indicator variable equal to one in the eight quarters following the implementation of the SC program. This table considers the baseline sample of survey respondents and uses individuals in later-treated counties as the control group for estimating the treatment effects of SC on the outcomes of individuals in earlier-treated counties (Sun and Abraham, 2021). Estimates are weighted using NCVS person weights to maintain sample representativeness. "Y Mean" refers to the average of the outcome variable in that specification. All outcomes are multiplied by 100 for ease of exposition (scale 0 to 100). Standard errors are clustered at the county level. Observation numbers and estimates have been rounded following Census disclosure guidelines.

Table 4: Effect of Secure Communities (SC) on Outcomes from Police Administrative Data

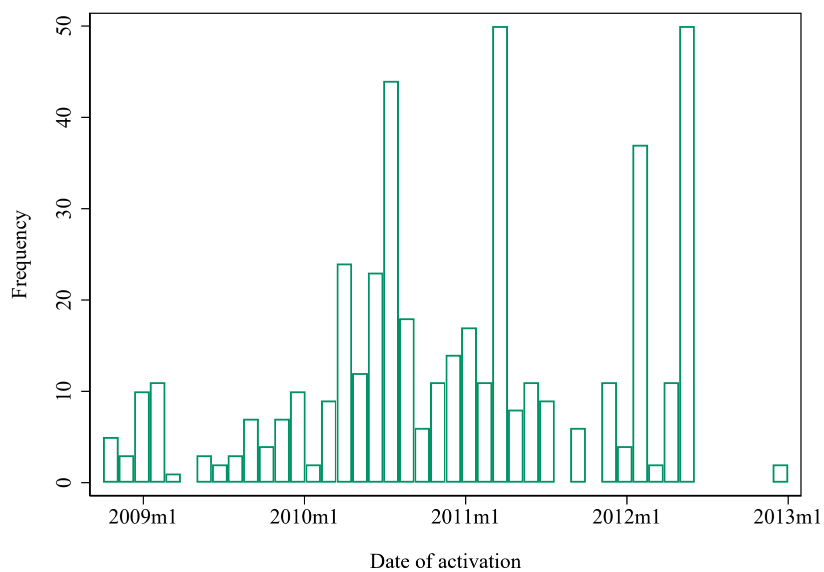
	(1) All Tracts	(2) Hispanic Tracts	(3) Non-Hispanic Tracts
A. 911 Calls per 1k capita			
β_{Post}	0.441	0.201	0.626
	(0.474)	(0.413)	(0.525)
Y Mean	56.248	51.238	57.656
Observations	220,070	48,262	171,808
Number of Cities	52	33	51
Tract Share Hispanic	0.244	0.655	0.129
B. Arrests per 1k capita			
β_{Post}	-0.054	0.047	-0.034
	(0.080)	(0.143)	(0.081)
Y Mean	3.528	3.146	3.698
Observations	218,182	67,201	150,981
Number of Cities	48	33	48
Tract Share Hispanic	0.315	0.701	0.144
C. Hispanic Share of Arrests			
β_{Post}	-0.006**	-0.015*	-0.004*
	(0.002)	(0.008)	(0.002)
Y Mean	0.280	0.539	0.147
Observations	81,892	27,848	54,044
Number of Cities	44	22	43
Tract Share Hispanic	0.328	0.677	0.147

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1). Outcomes are measured using micro-data from police administrative data, as described in Section 8. The unit of observation in each regression is a tract × year × month, and each regression includes city and time fixed effects. Standard errors are clustered at the county level. "Number of cities" refers to the number of unique cities represented in the regression. "Tract Share Hispanic" refers to the average Hispanic population share in the corresponding tracts using data from IPUMS (Manson et al., 2022).

Online Appendix

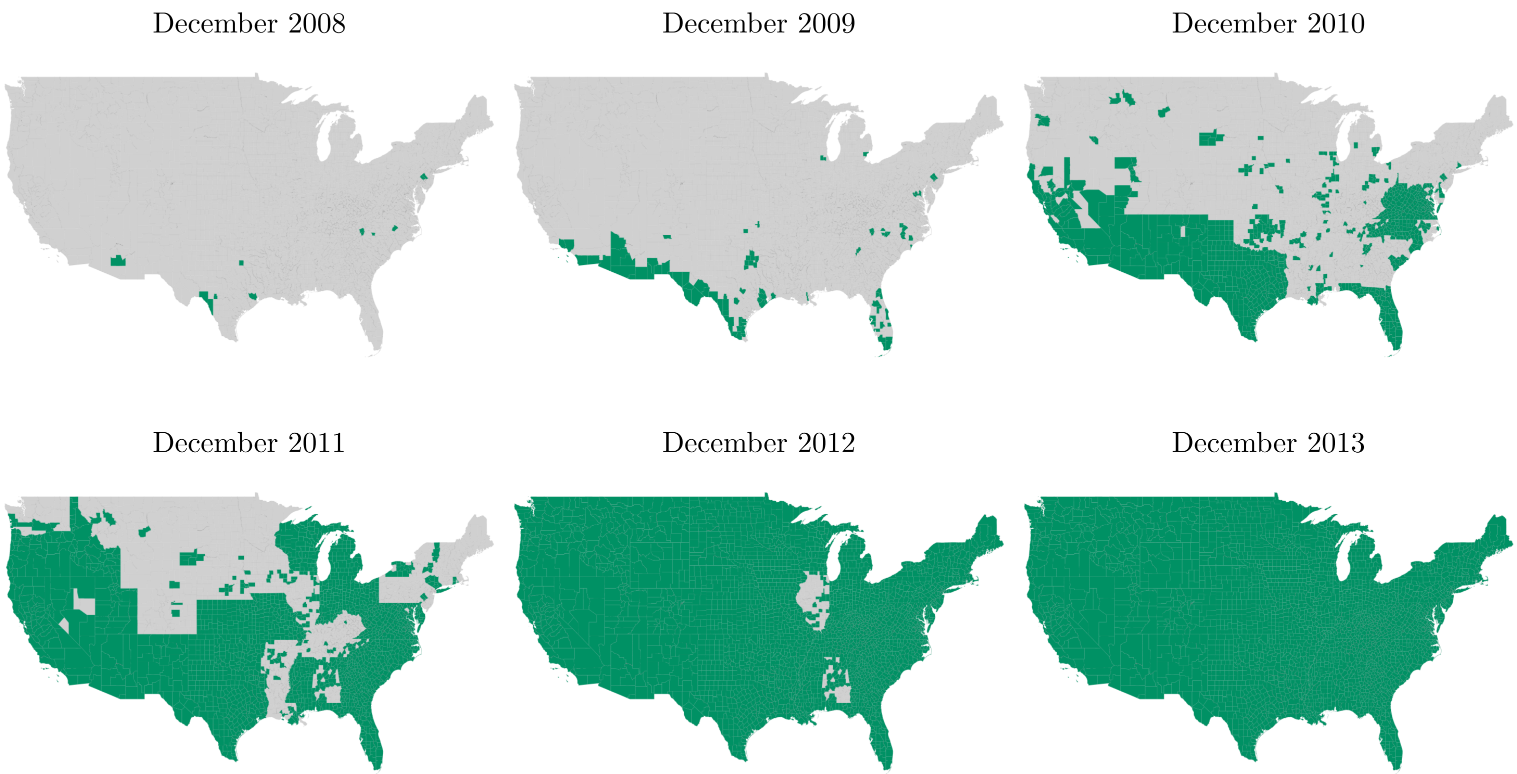
A Appendix Figures and Tables

Figure A.1: Activation of Secure Communities (SC) Program



Note: This figure displays the number of counties that activated the Secure Communities program in each month between October 2008 and January 2013 among counties that meet the sampling restrictions outlined in Section 3.1 (i.e., counties that are not border counties, that are not in IL, MA, or NY, and with populations exceeding 100,000 residents in 2000).

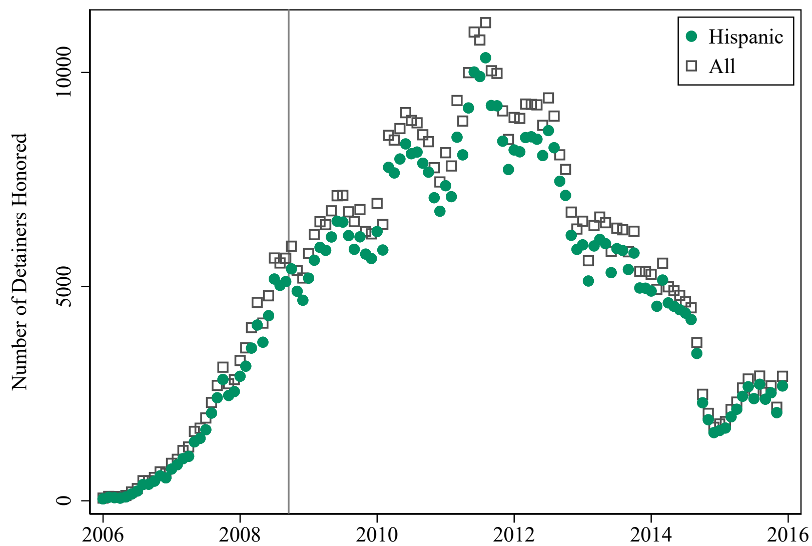
Figure A.2: Geography of Secure Communities (SC) Implementation



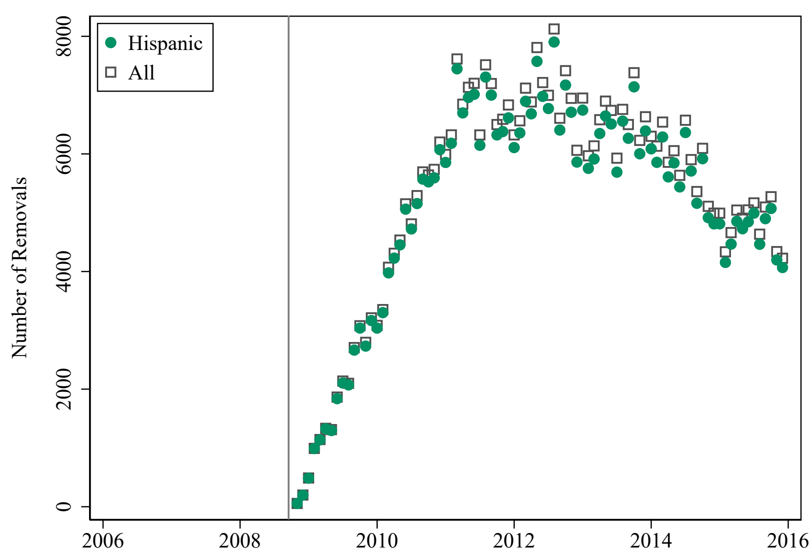
NOTE: This figure shows the county-level rollout of the Secure Communities Program over time, with counties that have implemented the program by each point in time highlighted in green.

Figure A.3: Number of ICE Honored Detainers and Removals Over Time

(a) Number of ICE Detainers Honored

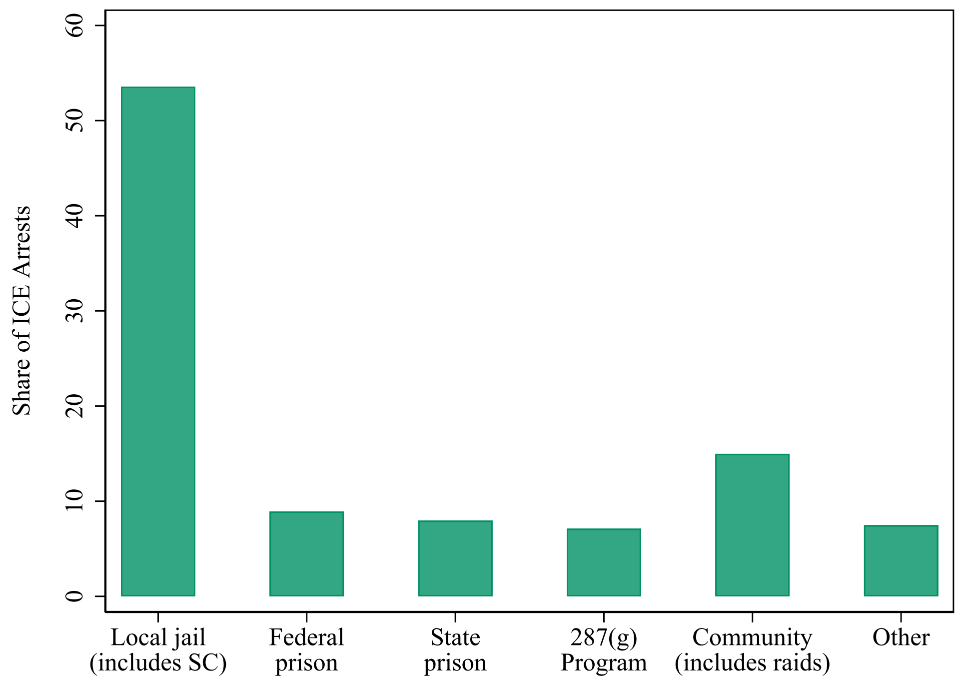


(b) Secure Communities (SC) Removals



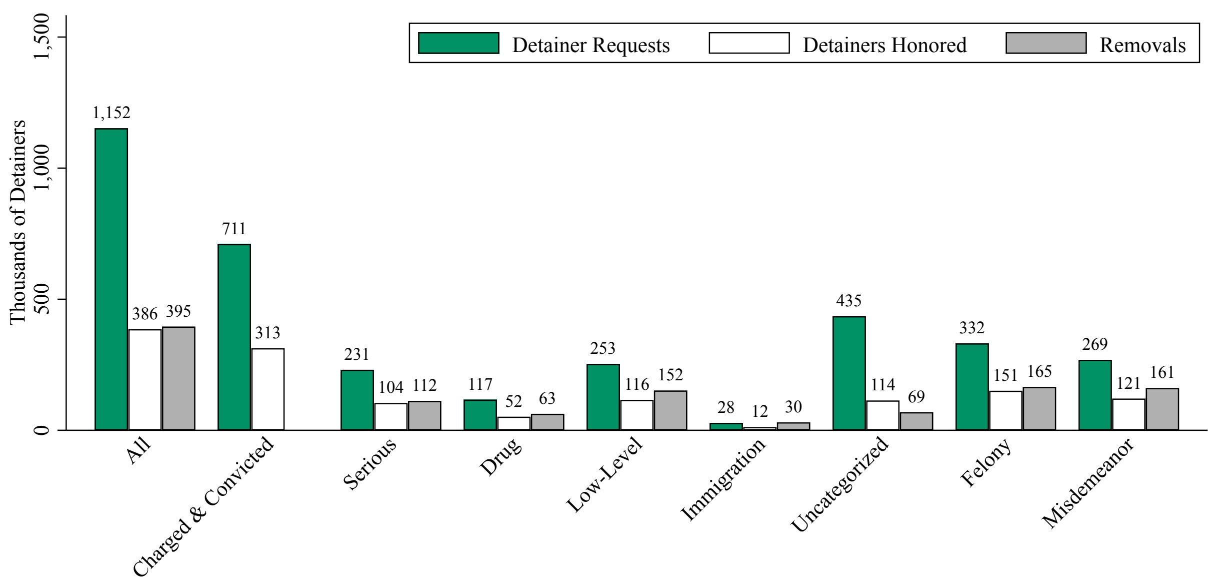
Notes: Panel (a) plots the number of monthly detainer requests honored by ICE using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. Honored detainers are available in both the pre- and post-period and are used in this study as a proxy for ICE removals (deportations). The black points consider all detainers and the green points consider detainers for individuals of Hispanic ethnicity (defined as individuals from Central and South American countries including Cuba and the Dominican Republic). Panel (b) plots analogous counts for the number of ICE removals documented through the Secure Communities (SC) program (only available once the policy is implemented).

Figure A.4: Interior ICE Arrests by Source



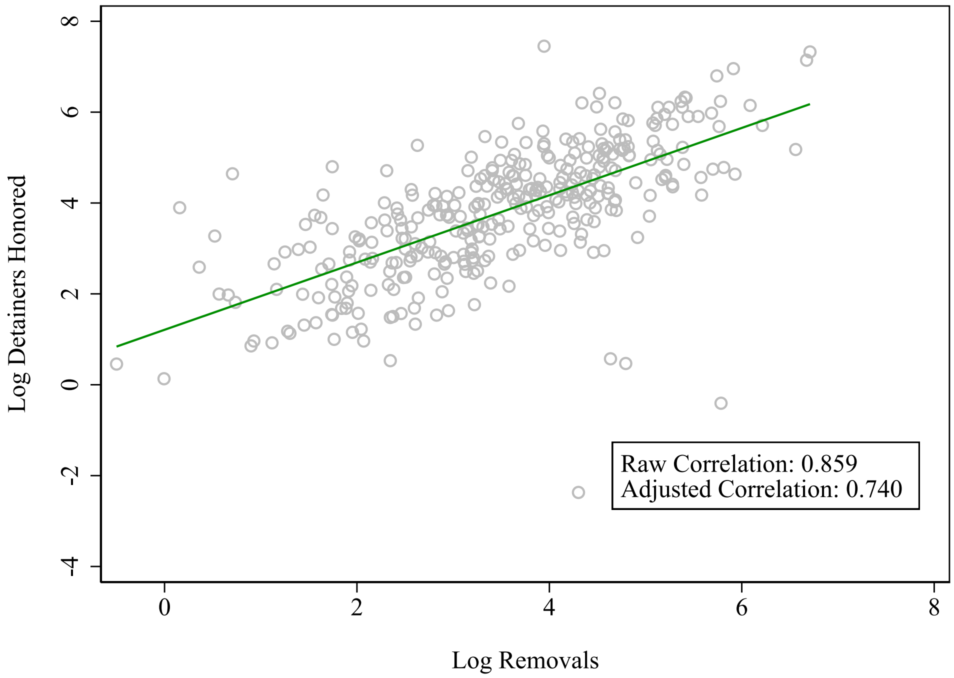
Notes: This figure plots the share of ICE interior arrests by source between October 2008 and August 2011. ICE arrest data are publicly available via the Transactional Records Access Clearinghouse (TRAC) at Syracuse University (TRAC, 2018). "Local" refers to individuals arrested by local police or sheriff's offices, after which point ICE assumes custody. "State" and "federal" refers to individuals who were transferred to ICE custody after being released from state and federal prison, respectively, at the end of their sentence. "287(g) Program" refers to arrests that involve law enforcement agencies that had signed cooperative agreements under a Memorandum of Agreement through ICE's 287(g) program. "Community" refers to individuals arrested at their homes, places of work, courthouses, or elsewhere in the community. "Other" refers to arrests from a miscellaneous combination of other sources.

Figure A.5: ICE Detainer Requests, Detainers Honored, and Removals, by Offense Type



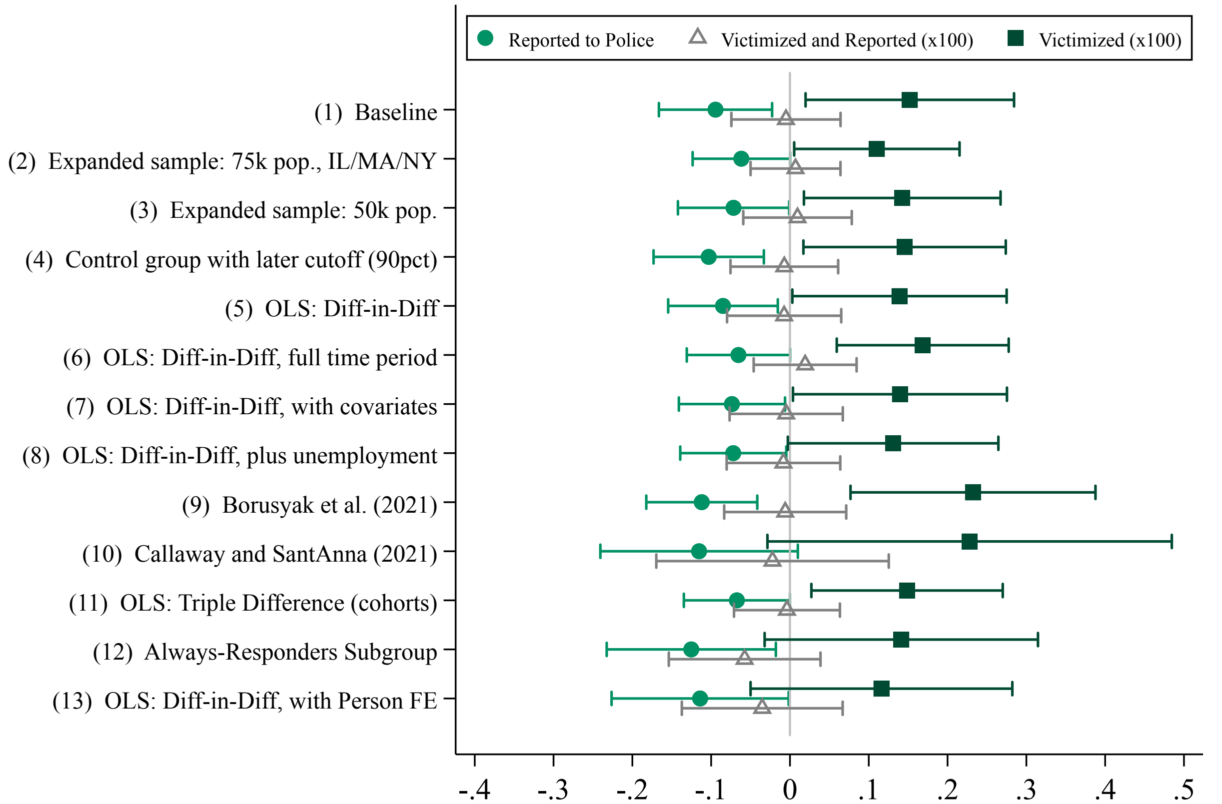
Notes: This figure shows the total number of ICE detainer actions by offense type using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. The data covers all actions after Secure Communities (SC) was implemented in each county from 10/2008 through 12/2014. A detainer request refers to a request made by ICE to hold an individual in a local facility while ICE decides whether he or she will be taken into federal custody for removal proceedings. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. Honored detainers are available in both the pre- and post-period and are used in this study as a proxy for removals. A removal is a record of an individual who was removed (or deported) from the U.S. as a result of the SC program. These records are only available when the program is active, do not include conviction status, and are indexed by removal date rather than detainer request date. "Charged & Convicted" refers to ICE records that indicate that an individual was charged and convicted for an offense, and is not available for removal records. The remaining bars utilize the description of the most serious criminal conviction in the detainer or removal record to classify offenses into categories. "Serious," "Drug," "Low-level" and "Uncategorized" are mutually exclusive categories. "Uncategorized" offenses refer to records for which ICE data did not provide an offense label in the data. "Immigration" offenses are a subset of low-level offenses. "Felony" and "misdemeanor" are alternative ways of classifying the seriousness of the offense and were provided by TRAC.

Figure A.6: Relationship between ICE Detainers Honored and Removals



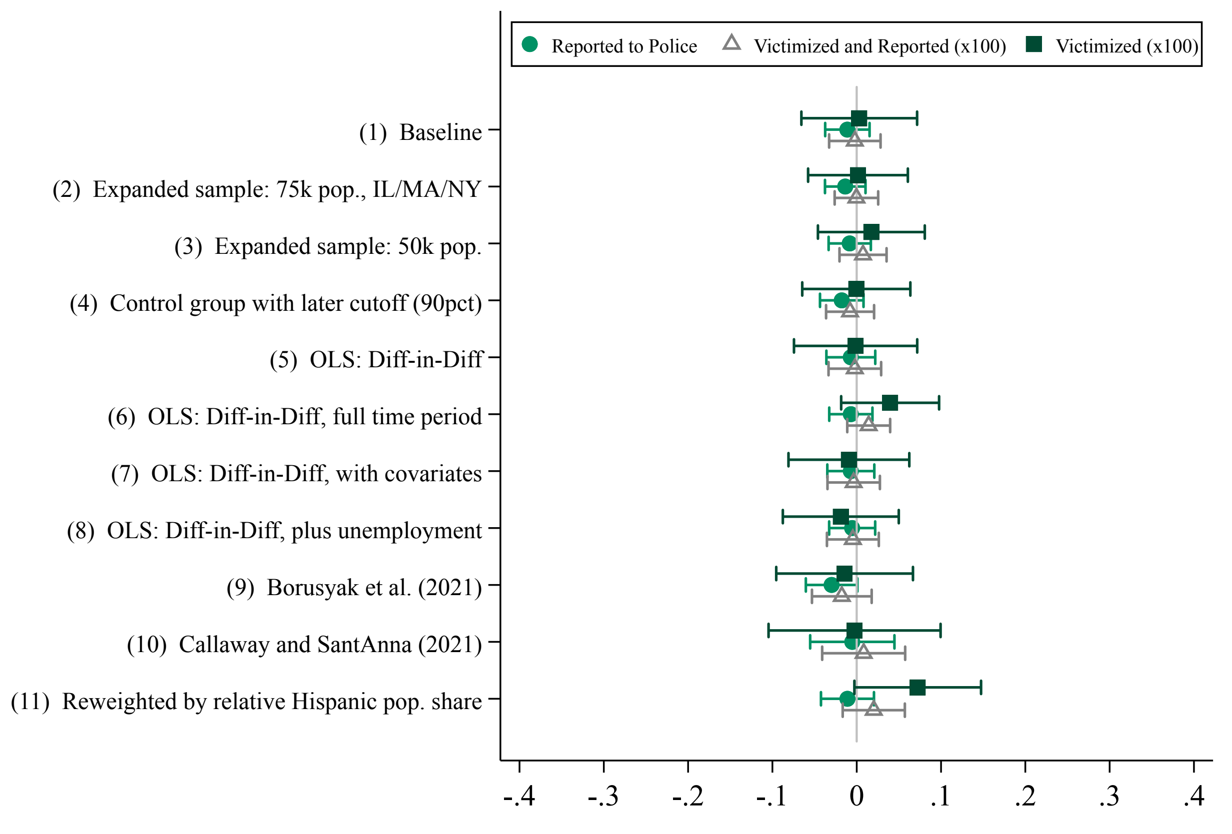
Notes: This figure plots the county-level relationship between the logged number of total removals and the logged number of total honored detainer requests using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. This figure shows the relationship between honored detainers and removals during our sample window (8/2008 through 8/2011), adjusted for county-level log population, share Hispanic residents, and share non-citizen Hispanic residents (measured in 2000). The sample of counties are those that meet the sampling restrictions described in Section 3.1. Logged values are calculated as $\ln(Y+1)$ to account for zero values. A removal is a record of an individual who was removed (deported) from the U.S. as a result of the SC program (these records are only available in a county after the program is implemented). An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention.

Figure A.7: Robustness of Main Results, Hispanic Respondents



Note: This figure reports variants of β_{Post} from equation (1) for the Hispanic sample. The bars refer to the 95% confidence interval for the two-year post-period estimate of the Secure Communities (SC) program. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (3) expand the analysis sample by including additional states or lowering the population threshold. (4) uses the last 10% of counties that activated SC as the control group (rather than the last 25%). (5) reports estimates from standard OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period to include respondents through June 2015 (full sample period). (7) re-estimates the specification in (5), adding respondent demographic controls (age, age squared; indicators for female, urban, Black, student, employed, married, HS degree, more than HS degree; and variables indicating missing characteristics). (8) re-estimates (7) controlling for time-varying county unemployment rates using U.S. Bureau of Labor Statistics (2023). (9) and (10) replicate the analysis using Borusyak et al. (2021) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (11) uses a triple-difference specification that considers non-Hispanic respondents as a control group, accounting for Hispanic×time, SC cohort×time, and Hispanic×SC cohort fixed effects (using the full time period sample). (12) restricts attention to households that responded to the NCVS survey in each wave, or "always-responders." (13) produces a variant of the OLS difference-in-difference specification in (5) that includes person fixed effects. "Victimized" and "Victimized and Reported" are multiplied by 100, while "Reported to Police" is not, for ease of exposition. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

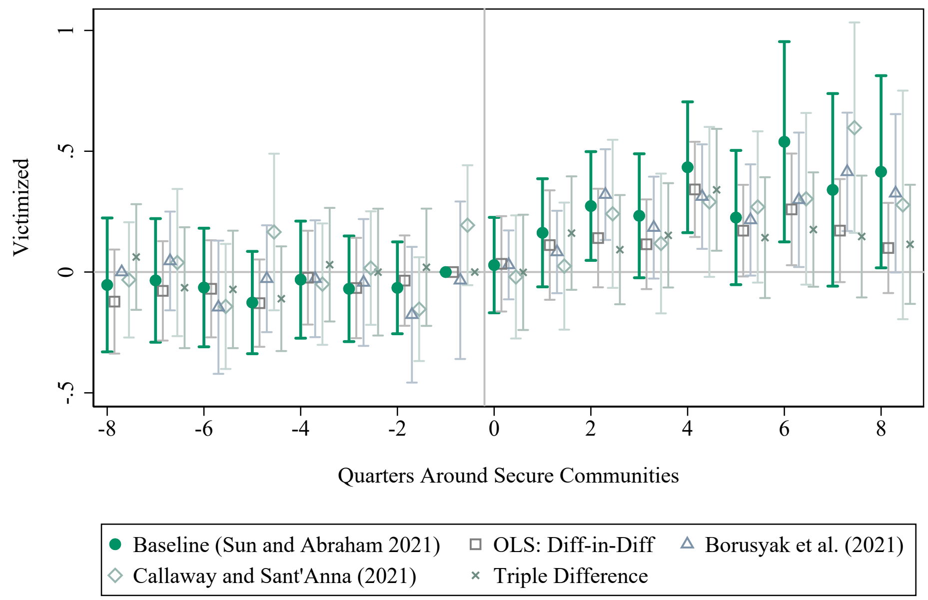
Figure A.8: Robustness of Main Results, Non-Hispanic Respondents



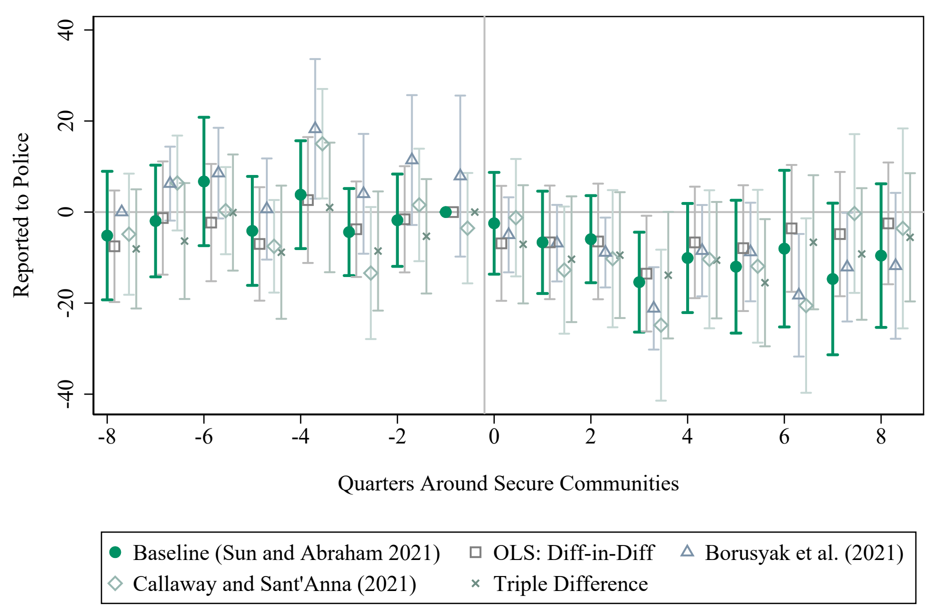
NOTE: This figure reports variants of β_{Post} from equation (1) for the non-Hispanic sample. The bars refer to the 95% confidence interval for the two-year post-period estimate of the Secure Communities (SC) program. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (3) expand the analysis sample by including additional states or lowering the population threshold. (4) uses the last 10% of counties that activated SC as the control group (rather than the last 25%). (5) reports estimates from standard OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period to include respondents through June 2015 (full sample period). (7) re-estimates the specification in (5), adding respondent demographic controls (age, age squared; indicators for female, urban, Black, student, employed, married, HS degree, more than HS degree; and variables indicating missing characteristics). (8) re-estimates (7) controlling for time-varying county unemployment rates using U.S. Bureau of Labor Statistics (2023). (9) and (10) replicate the analysis using Borusyak et al. (2021) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (11) re-weights non-Hispanic observations to resemble the geographic distribution of Hispanic respondents. "Victimized" and "Victimized and Reported" are multiplied by 100, while "Reported to Police" is not, for ease of exposition. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

Figure A.9: Robustness of Event-Study Results, Hispanic Respondents

(a) Victimized



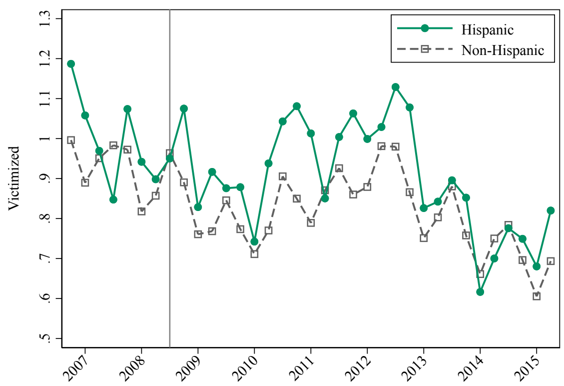
(b) Reported to Police



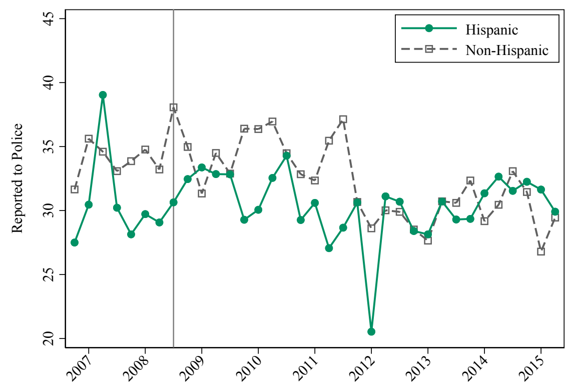
NOTE: This figure reports variants of β_{Post} from equation (2) for the Hispanic sample, with bars indicating 95% confidence intervals from standard errors clustered at the county level. The figures plot the baseline model estimates alongside the standard OLS difference-in-difference, a model following Borusyak et al. (2021), a model following Callaway and Sant'Anna (2021), and an OLS triple difference specification which considers non-Hispanic respondents as a control group, accounting for Hispanic×time, SC cohort×time, and Hispanic×SC cohort fixed effects.

Figure A.10: Victimization Rates, Share of Crimes Reported to Police, and Reported Crime Rate In Calendar Time

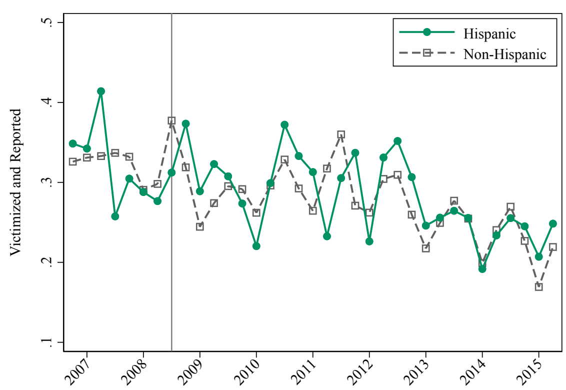
(a) Victimized



(b) Reported to Police

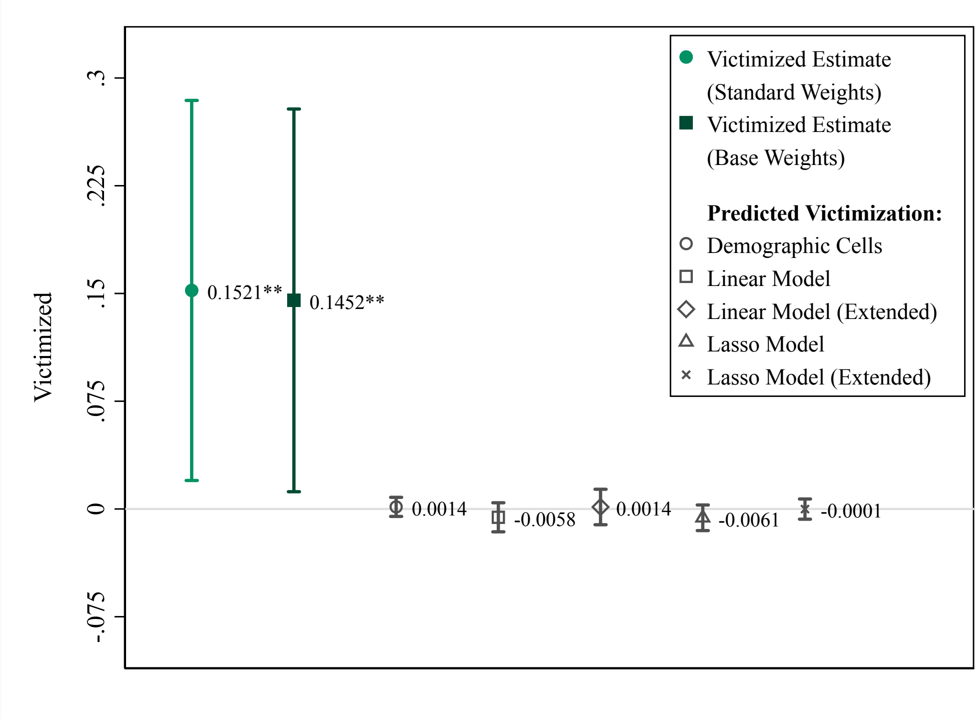


(c) Victimized and Reported to Police



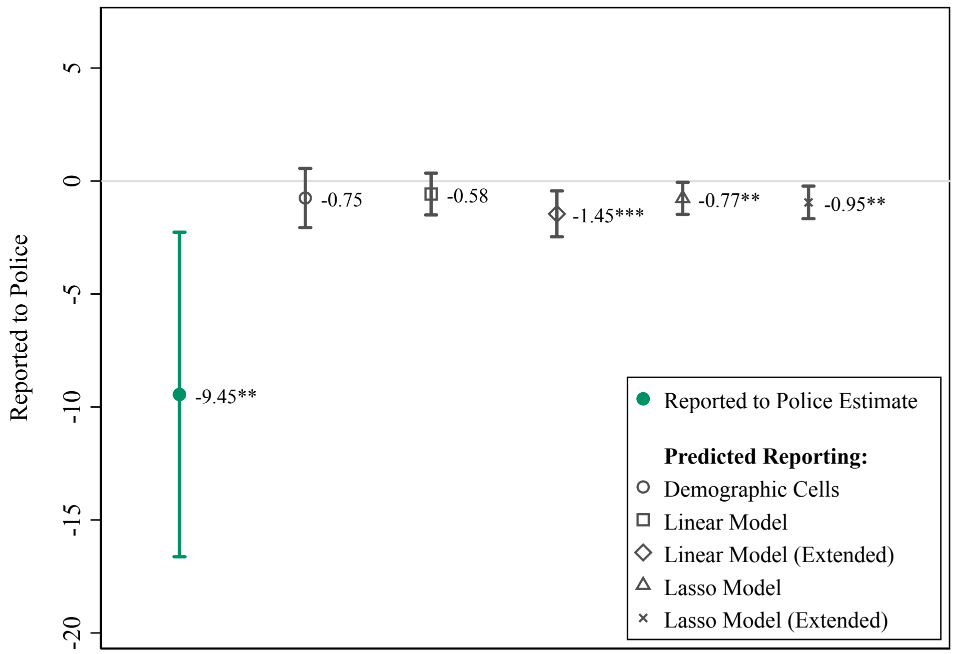
NOTE: These figures plot NCVS outcomes of whether a person is victimized, the share of crime incidents reported to police, and whether a person is both victimized and reported to police, separately for Hispanic and non-Hispanic respondents, between 2006 and 2015. Each outcome is multiplied by 100 for ease of exposition. The vertical line on each plot indicates the first activation date of the Secure Communities (SC) program.

Figure A.11: Victimized Regression Estimate vs. *Predicted Victimization* given Observable Characteristics



Note: *p<0.1, **p<0.05, ***p<0.01. The first estimate reproduces the baseline effect on victimization of Hispanic respondents following the implementation of Secure Communities (SC). This estimate uses the standard NCVS person weights, which partially adjust for survey non-response. The second estimate reports the baseline effect using alternative NCVS household "base weights," which do not include any adjustment for survey non-response. The remaining estimates use pre-period data to generate various measures of predicted victimization based on respondent characteristics. "Demographic cells" refers to using the average victimization rate based on age (defined as younger or older than 30), gender, and educational attainment (defined as less than high school, high school degree, more than high school degree). "Linear Model" refers to predicting victimization using a linear regression of the victimized outcome on age, age squared, urban, female, and educational attainment. The extended model augments this model with variables denoting employment status, marital status, and binned income levels. "Lasso model" predicts victimization using interactions of all the variables included in the linear model (excluding age and age squared). The second Lasso model uses interactions of the expanded set of covariates in the extended linear model.

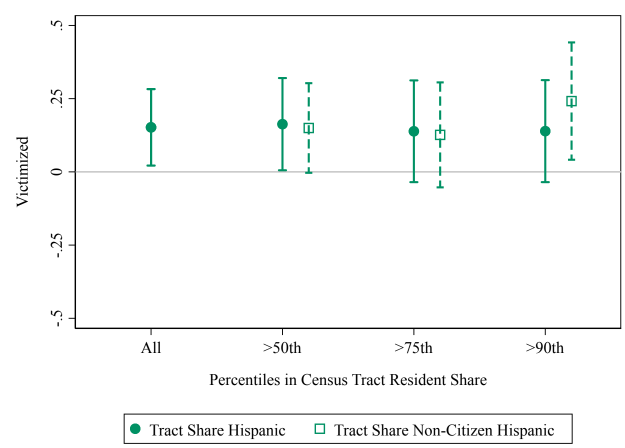
Figure A.12: Reported to Police Regression Estimate vs. *Predicted Reporting* given Observable Characteristics



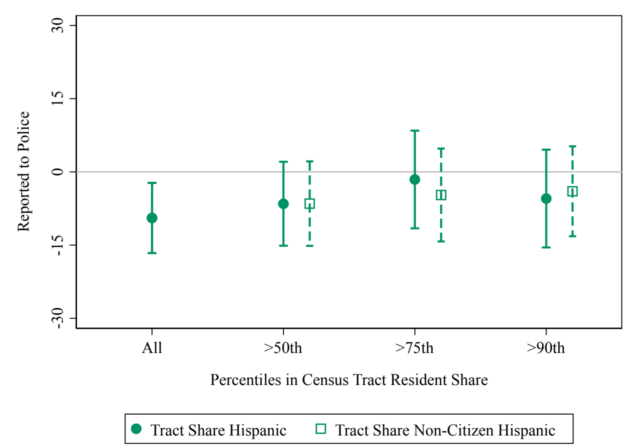
Note: *p<0.1, **p<0.05, ***p<0.01. The first estimate reproduces the baseline effect on the reporting behavior of Hispanic respondents following the implementation of Secure Communities (SC). The remaining estimates use pre-period data to generate various measures of predicted reporting based on victim and offense characteristics. "Demographic cells" refers to using the average reporting rate based on age (defined as younger or older than 30), gender, educational attainment (defined as less than high school, high school degree, more than high school degree), and crime type (violent crime, serious property crime, less serious property crime). "Linear Model" refers to predicting reporting behavior using a linear regression of reporting on age, age squared, urban, female, educational attainment, and crime type. The extended model augments this model with variables denoting employment status, marital status, and binned income levels. "Lasso model" predicts reporting using interactions of all the variables included in the linear model (excluding age and age squared). The second Lasso model uses interactions of the expanded set of covariates in the extended linear model.

Figure A.13: Results by Neighborhood Resident Characteristics

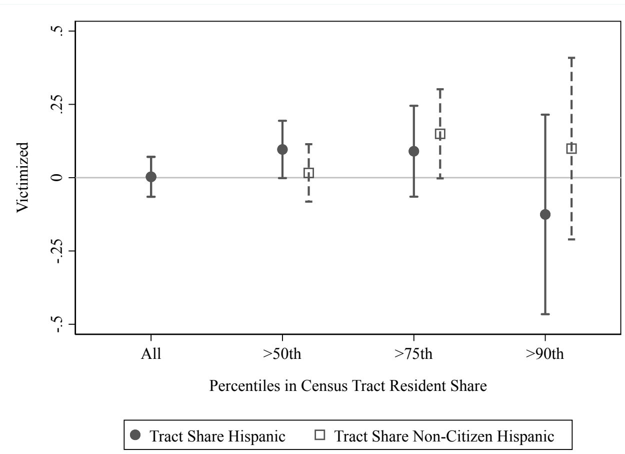
(a) Victimized, Hispanic



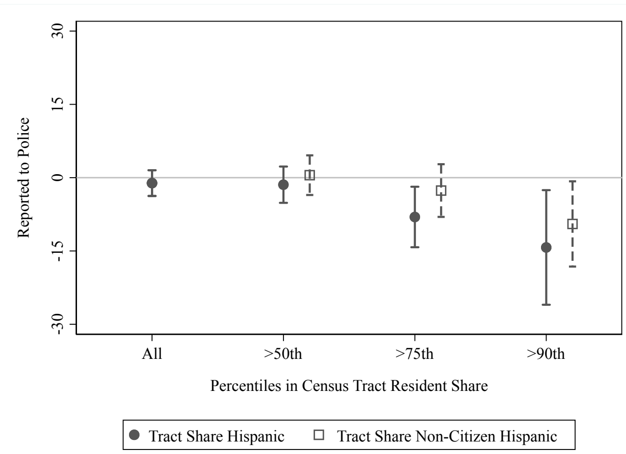
(b) Reported to Police, Hispanic



(c) Victimized, Non-Hispanic



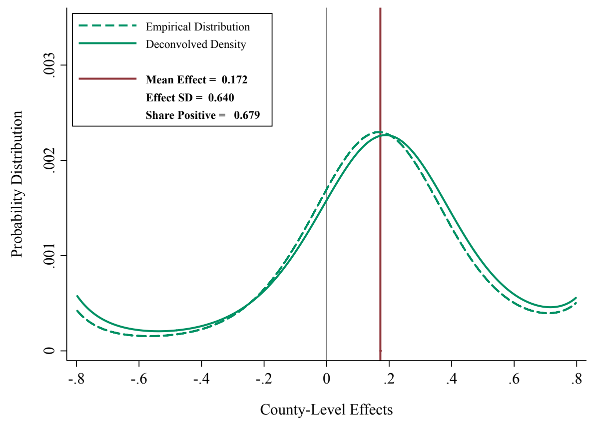
(d) Reported to Police, Non-Hispanic



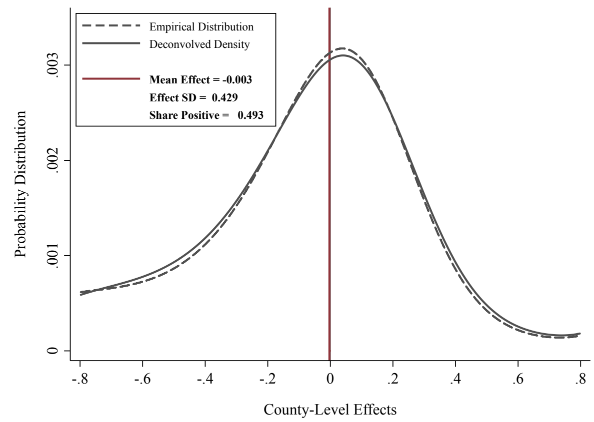
NOTE: This figure plots the estimates from equation (1) for subsamples of Hispanic and non-Hispanic survey respondents according to the share of their neighborhood that is Hispanic or non-citizen Hispanic. Data on neighborhood resident shares come from the 2000 Census and are linked to the NCVS based on the respondent's Census tract location. Bars represent 95% confidence intervals, where standard errors are clustered at the county level. All outcomes are multiplied by 100 for ease of exposition.

Figure A.14: County-Level Effects of Secure Communities

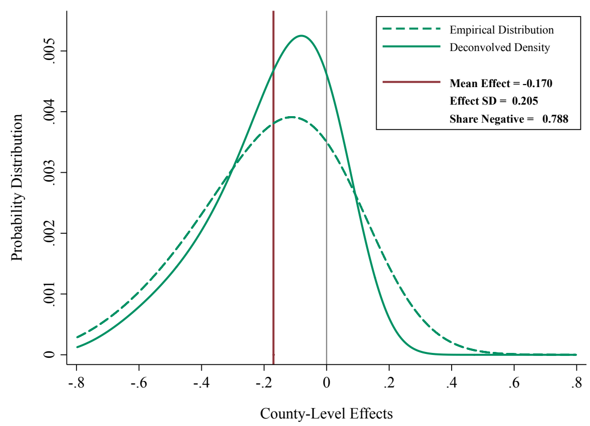
(a) Victimized, Hispanic



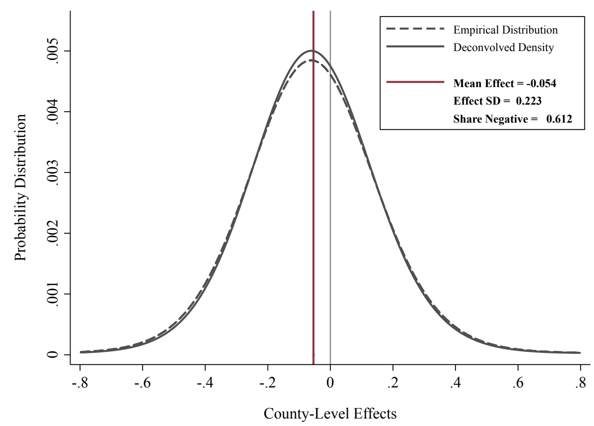
(b) Victimized, Non-Hispanic



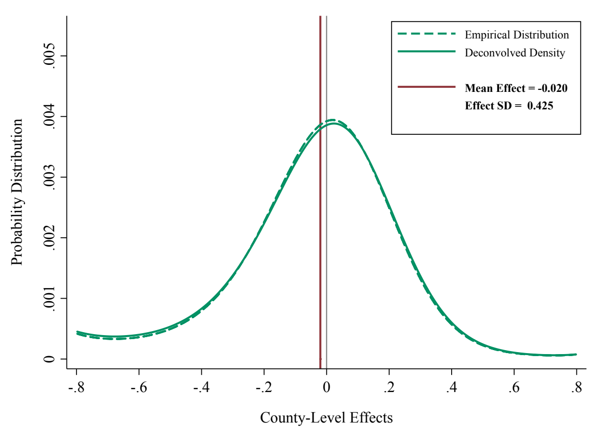
(c) Reported to Police, Hispanic



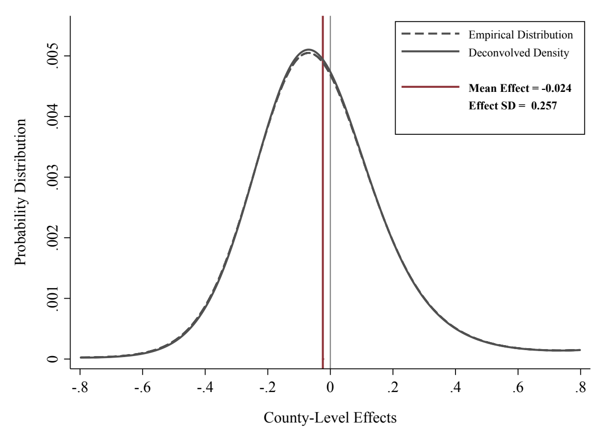
(d) Reported to Police, Non-Hispanic



(e) Victimized and Reported, Hispanic



(f) Victimized and Reported, Non-Hispanic



Notes: These figures plot the estimated distribution of Secure Communities program effects across counties, as described in Section 7. See Appendix D for details on estimation of the distributions.

Table A.1: Secure Communities Activation Timing and County-Level Characteristics

	(1)	(2)	(3)	(4)
Violent Crime (2005)	-0.014***	-0.006	-0.013***	-0.005
	(0.005)	(0.004)	(0.005)	(0.004)
% Change Violent Crime (2005 to 2007)			0.009	0.006
			(0.009)	(0.010)
Property Crime (2005)	0.000	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
% Change Property Crime (2005 to 2007)			-0.016	-0.008
			(0.020)	(0.020)
Population (2000)		-2.553***		-2.610***
		(0.758)		(0.742)
% Change Population (2000 to 2005-09)				-0.221***
				(0.052)
Black Share (2000)		-0.193***		-0.133***
		(0.047)		(0.048)
% Change Black Share (2000 to 2005-09)				0.029***
				(0.011)
Hispanic Share (2000)		-0.373***		-0.251***
		(0.055)		(0.061)
% Change Hispanic Share (2000 to 2005-09)				0.014
				(0.019)
Unemp. Rate (2000)		1.149***		0.529
		(0.425)		(0.444)
% Change Unemp. Rate (2000 to 2007)				0.018
				(0.016)
Poverty Rate (2000)		-0.038		-0.118
		(0.132)		(0.138)
% Change Poverty Rate (2000 to 2005-09)				-0.027
				(0.030)
Rep. Pres. Vote Share (2000)		-0.260***		-0.154***
		(0.042)		(0.045)
% Change Rep. Pres. Vote Share (2000 to 2004)				0.350***
				(0.086)
287(g) Before 2008		-5.393**		-5.360**
		(2.464)		(2.302)
Observations	458	458	458	458

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table regresses a discrete variable denoting the timing of Secure Communities activation (52 months, ranging from 10/2008 to 01/2013) on county-level characteristics. The sample of counties is restricted to non-border counties with more than 100,000 residents in 2000, that are not in IL, MA, or NY, and with available crime data. Violent and property crime rates (per 100,000 residents) come from Kaplan (2020) (using the largest agency serving each county). County-level demographic characteristics come from Manson et al. (2017) and are based on the 2000 Census and the 2005–2009 ACS. Unemployment rates come from U.S. Bureau of Labor Statistics (2023). Republican vote shares come from MIT Election Data and Science Lab (2018). 287(g) agreement data come from Gelatt et al. (2017) and Bernstein et al. (2022). Population refers to the logged population and we calculate the percentage change using population levels.

Table A.2: Alternative Measures of Immigration Enforcement (First Stage)

	β_{Post}	(S.E.)	Y Mean	N
(1) Log Detainers Honored (Baseline)	0.542***	(0.003)	1.880	27,022
(2) Log Detainer Requests	0.407***	(0.004)	2.772	27,022
(3) Detainers Honored Per 100,000 Residents	1.465***	(0.053)	2.380	27,022
(4) Detainer Requests Per 100,000 Residents	4.197***	(0.203)	6.797	27,022

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1). The estimate and standard error correspond to the two-year post-period effect of the Secure Communities (SC) program. The sample of counties are those that meet the sampling restrictions described in Section 3.1. Logged values are calculated as $\ln(Y+1)$ to account for zero values. A detainer request refers to a request made by ICE to hold an individual in a local facility while ICE decides whether he or she will be taken into federal custody for removal proceedings (deportation). An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. Honored detainers are available in both the pre- and post-period and are used in this study as a proxy for ICE removals (which are only available in the post-period of the policy), and as the primary first stage measure. Per-capita outcomes are adjusted by county population in the year 2000. "Y Mean" refers to the average of the outcome variable in that specification. Standard errors are clustered at the county level. Estimates are weighted by the county's population in that year (U.S. Census Bureau, 2022).

Table A.3: Effect of SC on Reported Crime Rates using FBI Uniform Crime Reports

	Index Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
β_{Post}	-4.636	-2.317	-1.555**	-0.183	-3.081	-2.134
	(4.451)	(4.061)	(0.703)	(0.459)	(4.140)	(3.806)
Y Mean	388.84	388.84	50.57	50.57	338.27	338.27
Observations	13,098	13,098	13,098	13,098	13,098	13,098
Linear Trend		✓		✓		✓

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using the FBI's Uniform Crime Reports and equation (1) at the agency-by-month level. The estimate β_{Post} and standard error correspond to an indicator variable equal to one in the eight quarters following the implementation of the SC program. The outcome variables are the per capita index, violent, and property crime rates (per 100,000 residents). This table considers the 186 agencies reporting crime consistently between October 2006 and August 2011, in counties that meet the sampling criteria described in Section 3.1, and with local populations above 100,000 in 2000 using Manson et al. (2017). The regressions use later-treated agencies as the control group for estimating the treatment effects of SC on the outcomes of agencies treated earlier in time (Sun and Abraham, 2021). Columns (2), (4), and (6) include agency-specific linear time trends. Estimates are weighted using the 2000 agency population. "Y Mean" refers to the average of the outcome variable in that specification. Standard errors are clustered at the county level.

Table A.4: Robustness of Main Results, Hispanic Respondents

	Victimized			Reported to Police			Victimized & Reported		
	β_{Post}	(S.E.)	Y Mean	β_{Post}	(S.E.)	Y Mean	β_{Post}	(S.E.)	Y Mean
(1) Baseline	0.152**	(0.067)	0.96	-9.446**	(3.664)	30.98	-0.005	(0.035)	0.31
(2) Expanded sample: 75k pop., IL/MA/NY	0.110**	(0.054)	0.92	-6.167*	(3.155)	30.89	0.007	(0.029)	0.30
(3) Expanded sample: 50k pop.	0.143**	(0.064)	0.95	-7.159**	(3.600)	31.27	0.010	(0.035)	0.31
(4) Control group with later cutoff (90pct)	0.146**	(0.066)	0.97	-10.310***	(3.570)	30.43	-0.007	(0.035)	0.31
(5) OLS: Diff-in-Diff	0.139**	(0.069)	0.96	-8.496**	(3.549)	30.98	-0.007	(0.037)	0.31
(6) OLS: Diff-in-Diff, full time period	0.169***	(0.056)	0.92	-6.537*	(3.345)	30.31	0.019	(0.033)	0.29
(7) OLS: Diff-in-Diff, with covariates	0.140**	(0.069)	0.96	-7.362**	(3.436)	30.98	-0.005	(0.037)	0.31
(8) OLS: Diff-in-Diff, plus unemployment	0.131*	(0.068)	0.96	-7.196**	(3.432)	30.98	-0.008	(0.037)	0.31
(9) Borusyak et al. (2021)	0.232***	(0.079)	0.97	-11.190***	(3.593)	30.29	-0.006	(0.040)	0.31
(10) Callaway and SantAnna (2021)	0.228*	(0.131)	0.97	-11.530*	(6.393)	30.29	-0.022	(0.075)	0.31
(11) OLS: Triple Difference (cohorts)	0.149**	(0.062)	0.85	-6.733*	(3.434)	32.21	-0.004	(0.034)	0.28
(12) Always-Responders Subgroup	0.141	(0.088)	0.81	-12.520**	(5.475)	31.45	-0.057	(0.049)	0.26
(13) OLS: Diff-in-Diff, with Person FE	0.116	(0.085)	0.96	-11.400**	(5.732)	30.98	-0.035	(0.052)	0.31

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1) for the Hispanic sample. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (3) expand the analysis sample by including additional states or lowering the population threshold. (4) uses the last 10% of counties that activated SC as the control group (rather than the last 25%). (5) reports estimates from standard OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period to include respondents through June 2015 (full sample period). (7) re-estimates the specification in (5), adding respondent demographic controls (age, age squared; indicators for female, urban, Black, student, employed, married, HS degree, more than HS degree; and variables indicating missing characteristics). (8) re-estimates (7) controlling for time-varying county unemployment rates using U.S. Bureau of Labor Statistics (2023). (9) and (10) replicate the analysis using Borusyak et al. (2021) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (11) uses a triple-difference specification that considers non-Hispanic respondents as a control group, accounting for Hispanic×time, SC cohort×time, and Hispanic×SC cohort fixed effects (using the full time period sample). (12) restricts attention to households that responded to the NCVS survey in each wave, or "always-responders." (13) produces a variant of the OLS difference-in-difference specification in (5) that includes person fixed effects. "Victimized" and "Victimized and Reported" are multiplied by 100, while "Reported to Police" is not, for ease of exposition. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

Table A.5: Robustness of Main Results, Non-Hispanic Respondents

	Victimized			Reported to Police			Victimized & Reported		
	β_{Post}	(S.E.)	Y Mean	β_{Post}	(S.E.)	Y Mean	β_{Post}	(S.E.)	Y Mean
(1) Baseline	0.003	(0.035)	0.87	-1.112	(1.343)	34.50	-0.002	(0.016)	0.31
(2) Expanded sample: 75k pop., IL/MA/NY	0.001	(0.030)	0.83	-1.362	(1.227)	33.82	0.000	(0.013)	0.29
(3) Expanded sample: 50k pop.	0.017	(0.032)	0.85	-0.830	(1.279)	34.13	0.007	(0.014)	0.30
(4) Control group with later cutoff (90pct)	-0.001	(0.033)	0.86	-1.776	(1.317)	34.16	-0.008	(0.015)	0.31
(5) OLS: Diff-in-Diff	-0.001	(0.037)	0.87	-0.701	(1.478)	34.50	-0.002	(0.016)	0.31
(6) OLS: Diff-in-Diff, full time period	0.040	(0.030)	0.83	-0.698	(1.307)	32.63	0.014	(0.013)	0.28
(7) OLS: Diff-in-Diff, with covariates	-0.009	(0.037)	0.87	-0.703	(1.422)	34.50	-0.004	(0.016)	0.31
(8) OLS: Diff-in-Diff, plus unemployment	-0.019	(0.035)	0.87	-0.541	(1.392)	34.50	-0.005	(0.016)	0.31
(9) Borusyak et al. (2021)	-0.014	(0.041)	0.86	-2.978*	(1.561)	34.07	-0.018	(0.018)	0.30
(10) Callaway and SantAnna (2021)	-0.003	(0.052)	0.86	-0.524	(2.550)	34.07	0.008	(0.025)	0.30
(11) Reweighted by relative Hispanic pop. share	0.072*	(0.038)	0.87	-1.098	(1.603)	34.50	0.020	(0.019)	0.31

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1) for the non-Hispanic sample. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (3) expand the analysis sample by including additional states or lowering the population threshold. (4) uses the last 10% of counties that activated SC as the control group (rather than the last 25%). (5) reports estimates from standard OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period to include respondents through June 2015 (full sample period). (7) re-estimates the specification in (5), adding respondent demographic controls (age, age squared; indicators for female, urban, Black, student, employed, married, HS degree, more than HS degree; and variables indicating missing characteristics). (8) re-estimates (7) controlling for time-varying county unemployment rates using U.S. Bureau of Labor Statistics (2023). (9) and (10) replicate the analysis using Borusyak et al. (2021) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (11) re-weights non-Hispanic observations to resemble the geographic distribution of Hispanic respondents. All outcomes are multiplied by 100 for ease of exposition. Standard errors are clustered at the county level. Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness.

Table A.6: Effect of Secure Communities (SC) on Survey Response,
by Household Neighborhood Hispanic Share

Sample	Survey Response Treatment Effect			Share Hispanic	Implied Hispanic Attrition	*Worst-Case* Victimization "Effect"	Percent of Estimated Effect
	β_{Post}	(S.E.)	Y-Mean				
(1) All Households	0.521	(0.418)	76.99	0.121	—	—	—
(2) Census Tracts >50th pct. Hispanic	-0.112	(0.573)	77.47	0.219	-0.513	0.006	4.0%
(3) Census Tracts >75th pct. Hispanic	-1.209	(0.800)	78.15	0.367	-3.291	0.040	26.1%
(4) Census Tracts >90th pct. Hispanic	-1.416	(1.164)	79.12	0.597	-2.373	0.028	18.3%

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates of the effect of SC on household survey response, as described in Section 6.2. The first row considers all households. The subsequent rows sequentially restrict the sample to respondents living in tracts above the 50th, 75th, and 90th percentile of the tract-level distribution of the share of the population that is Hispanic. The estimate β_{Post} and standard error correspond to an indicator variable equal to one in the eight quarters following the implementation of the SC program. Estimates are weighted using household base weights to maintain sample representativeness (these weights reflect the probability of selection into the sample but do not incorporate non-response adjustments). Standard errors are clustered at the county level. "Y Mean" refers to the average of the outcome variable in that specification, or household response rate. "Share Hispanic" is the share of residents who are Hispanic in the corresponding sample. "Implied Hispanic Attrition" assumes that all persons who leave the survey are Hispanic, and is thus a conservative calculation of the change in response rates of Hispanics (in percentage points). The "Worst-Case Victimization 'Effect"' calculates the implied change in victimization against Hispanic respondents, under the conservative assumption that all persons who leave the survey due to the policy are Hispanic *and* were not victims of any crimes. This column uses the pre-period Hispanic victimization level of 0.9 percentage points in the calculation. The "Percent of Estimated Effect" column calculates the share of the total victimization effect we observe that could be explained by the "Worst Case Victimization Effect." Estimates have been rounded following Census disclosure guidelines.

Table A.7: Effect of Secure Communities (SC) for Hispanic Individuals, by Gender

	β_{Post}	(S.E.)	Y Mean	N
A. Male respondents				
Victimized	0.085	(0.088)	1.01	189,000
Reported to Police	-9.645*	(5.582)	30.36	2,100
Victimized and Reported to Police	-0.007	(0.052)	0.32	189,000
B. Female respondents				
Victimized	0.230***	(0.085)	0.91	202,000
Reported to Police	-5.230	(3.659)	31.67	2,000
Victimized and Reported to Police	0.002	(0.041)	0.30	202,000

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1) among Hispanic respondents separately by gender. The estimate β_{Post} and standard error correspond to an indicator variable equal to one in the two years following the implementation of the SC program. This table considers the baseline sample of survey respondents and uses individuals in later-treated counties as the control group for estimating the treatment effects of SC on the outcomes of individuals in earlier-treated counties (Sun and Abraham, 2021). Estimates are weighted using NCVS person weights to maintain sample representativeness. "Y Mean" refers to the average of the outcome variable in that specification. All outcomes are multiplied by 100 for ease of exposition. Standard errors are clustered at the county level. Observation numbers and estimates have been rounded following Census disclosure guidelines.

Table A.8: Relationship between County-Level Characteristics and Immigration Enforcement, Victimization, & Reporting

	ICE Removals		Victimized			Reported to Police		
	Per Capita (1)	Felony Share (2)	(3)	(4)	(5)	(6)	(7)	(8)
β_{Post}			1.094	0.159	0.435	-5.797	-11.030**	-52.230*
			(1.048)	(0.111)	(1.035)	(33.800)	(5.150)	(31.170)
County Characteristics (Rows (3)-(8): β_{Post} Interactions)								
Removals Per Capita			0.304			4.433		
			(0.282)			(9.144)		
Removal Felony Share			-3.727			-24.820		
			(3.839)			(123.300)		
Hispanic Share	0.002	0.165*		-0.743	-1.046*		-5.752	-18.170
	(0.554)	(0.095)		(0.495)	(0.595)		(12.180)	(15.320)
Hispanic Non-Citizen Share	10.137**	0.394		2.192*	2.875*		43.640	-2.728
	(3.921)	(0.456)		(1.277)	(1.734)		(28.670)	(46.800)
Log Population	0.106	0.013			-0.015			2.950
	(0.143)	(0.012)			(0.068)			(2.095)
Share with BA or More	1.175**	-0.032			-0.741			-9.279
	(0.484)	(0.104)			(0.802)			(33.050)
Poverty Rate	0.939	-0.451**			0.402			61.570
	(0.961)	(0.182)			(2.399)			(100.200)
Republican Vote Share (2004)	1.509**	0.000			0.202			2.055
	(0.678)	(0.077)			(0.585)			(20.000)
Outcome Mean	0.91	0.33	0.96	0.96	0.96	30.98	30.98	30.98

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table relates immigration enforcement, victimization, and reporting against county-level characteristics. (1) regresses county-level measures of total removals (deportations) per capita on county characteristics. (2) uses the share of removals that resulted from a felony offense as the outcome. Both outcomes are measured in the first two years after SC. For columns (3)-(8), we estimate OLS models in the NCVS sample, including interaction terms of the SC effect (β_{Post}) with county characteristics (reported in this table) as well as controls for the main effects of county characteristics (not reported in this table). County demographic variables come from IPUMS and are measured in the year 2000. Vote share refers to the share of the county that voted Republican in the 2004 presidential election. Counties are restricted to all counties that meet the baseline characteristics described in Section 3.1.

Table A.9: Arrest Effects of Secure Communities (SC), by Hispanic Ethnicity

	β_{Post}	(S.E.)	Y Mean
A. Hispanic Victims			
Arrest Made, All Victimizations	-1.625	(1.202)	4.36
Arrest Made, Reported Victimizations	-2.002	(2.812)	8.97
B. Non-Hispanic Victims			
Arrest Made, All Victimizations	-0.408	(0.632)	5.13
Arrest Made, Reported Victimizations	0.261	(1.436)	9.74
C. All Victims			
Arrest Made, All Victimizations	-0.579	(0.547)	5.00
Arrest Made, Reported Victimizations	0.066	(1.224)	9.61

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1). The outcome is whether an arrest is made for a criminal victimization. The second row of each subgroup restricts the sample to only victimizations that are reported to the police. The estimate β_{Post} and standard error correspond to an indicator variable equal to one in the eight quarters following the implementation of the SC program. This table considers the baseline sample of NCVS respondents and uses individuals in later-treated counties as the control group for estimating the treatment effects of Secure Communities on the outcomes of individuals in earlier-treated counties (Sun and Abraham, 2021). Standard errors are clustered at the county level. Estimates are weighted using NCVS person weights to maintain sample representativeness. "Y Mean" refers to the average of the outcome variable in that specification. All outcomes are multiplied by 100 for ease of exposition (scale 0 to 100). Estimates have been rounded following Census disclosure guidelines.

Table A.10: Decomposing Victimization Increase into Various Components

				Total Victimization Effect	0.152
Mediator Variable	Elasticity & Source		Implied Effect on Victimization	Effect of SC (% Effect)	Predicted Effect on Victimization
	(1)		(2)	(3)	(4)
Victim Reporting	-1.09	(Golestani 2021)	-0.03	-9.45 (-28.62%)	0.282
Employment	-1.66	(Gould et al. 2002)	-0.02	-0.17 (-0.24%)	0.004
Hourly Wage	-1.35	(Gould et al. 2002)	-0.46	-0.04 (-1.67%)	0.020
Female-headed Household	1.46	(Glaeser & Sacerdote 1999)	3.15	0.01 (2.50%)	0.033
Male Immigrant Share	1.07	(Chalfin & Deza 2020)	0.24	-0.13 (-3.34%)	-0.032
Residual					-0.154

NOTE: This table presents estimates from the decomposition outlined in Section 9.2 (also depicted in Figure 6). The top-right estimate corresponds to the effect of Secure Communities (SC) on victimization (Table 2). "Elasticity & Source" refers to the implied elasticity of crime with respect to each mediator using estimates from the listed study. "Implied Effect on Victimization" re-scales the elasticity by the average victimization rate of Hispanics and the average of each mediator as measured in the American Community Survey (ACS) prior to SC. "Effect of SC" refers to the effect of Secure Communities on each of the mediators. The number in parentheses is the percent change in each mediator from its pre-period baseline value. The effect of SC on victim reporting comes from the NCVS (Table 2); the percent change differs from the 30% effect discussed in the main text, as it is adjusted by the pre-period mean rather than the overall mean. We estimate the percent effect of SC on the other mediators using the ACS. "Predicted Effect on Victimization" is the product of the effect of SC on the mediator and the implied effect of that mediator on victimization (columns 2 and 3). "Residual" (the bottom-right estimate) refers to the part of the total victimization effect that cannot be explained by the five mediators. For more details on these calculations, see Appendix G.

B Conceptual Framework

We present here a simple conceptual framework with two groups: potential offenders and potential victims. For simplicity, we begin by assuming that all individuals are unauthorized and face a risk of deportation.

There is a unit mass of potential offenders who have to make a single choice of whether to commit a crime or not. If the offender chooses to commit a crime, they are randomly matched with a victim, and they receive a uniform value of M, which reflects the monetary value of their crime.

They also face a cost for their crime, c, which has distribution $G(c) \in [0, 1]$ across offenders. This cost includes the psychic and opportunity cost of offending but not the punishment cost.

If they commit an offense and are caught (which is a function of victim behavior, discussed below), they face two costs. First, is the standard punishment $x > 0$. Second, there is a probability p_D they are referred to immigration enforcement, in which case they face punishment D. The value of not offending is normalized to 0.

There is also a unit mass of potential victims. They only act if they have been victimized, in which case they face the binary choice of reporting the crime to the police. They face a uniform benefit of reporting b, which can include the expected psychic benefits from the offender's apprehension, the remuneration of stolen property, and future safety benefits. They also face a hassle cost of reporting, $h > 0$, which has distribution $F(h) \in [0, 1]$. Reporting also includes a potential risk of being referred to immigration enforcement, δp_D, in which case the victim faces a punishment D. Here, $\delta \in [0, 1]$ indicates that a victim's likelihood of being referred to immigration enforcement may differ from that of offenders. This risk of being referred to immigration enforcement may be real or perceived. The value of not reporting is normalized to 0.

The victim's choice of reporting follows a simple threshold crossing rule, $b - h - \delta p_D D > 0$, so victims with a sufficiently low hassle cost, $h < b - \delta p_D D$, report to the police. This rule generates a reporting probability:

$$r = F(b - \delta p_D D)$$

If a crime is reported to the police, there is a uniform probability a that the police apprehend the offender. Because offenders and victims are randomly matched, the offender only knows the overall probability r that the offense will be reported to the police. Their decision to offend follows a threshold crossing rule, $M - c - rax - rap_D D > 0$, so potential offenders with a sufficiently low cost, $c < M - rax - rap_D D$, choose to offend. The probability of an offense (and hence the number of offenses) is:

$$O = G(M - rax - rap_D D)$$

The immigration enforcement policy tool we consider is a shift in p_D. This can be thought of as the probability that federal immigration authorities become aware of an individual's immigration status. Secure Communities increased information sharing between federal immigration officials and local law enforcement, thereby increasing p_D in counties that implemented the program. Notably, p_D is contained within the reporting cost for victims and within the expected punishment for offenders, so a shift in p_D affects both parties.

We will consider the comparative statics from an increase in p_D. We are interested in the policy's impact on three key outcomes: the probability a crime is reported, r, the number of offenses, O, and the number of offenses that are reported, which we refer to as the reported crime rate and denote by $C \equiv rO$.

The reporting probability responds to a change in p_D as follows:

$$\frac{\partial r}{\partial p_D} = -F'(\cdot)\delta D \leq 0$$

As long as $\delta > 0$, then the change in the reporting probability with respect to increased immigration enforcement will be negative. This expression highlights that as long as a victim believes that their likelihood of deportation is greater than zero (whether that belief is real or perceived), then the share of incidents that are reported will unambiguously decline given that the cost of reporting has increased.

The number of offenses depends on both the change in deportation risk for the offender and how the probability of reporting has changed:

$$\frac{dO}{dp_D} = G'(\cdot)\big[\underbrace{-\frac{\partial r}{\partial p_D}(ax + ap_D D)}_{\text{Lower Reporting} \uparrow \text{Crime}} \quad \underbrace{-\, raD}_{\text{Deterrence} \downarrow \text{Crime}}\big] \lesseqgtr 0$$

The two expressions inside the brackets have opposite signs, so the impact is ambiguous. Intuitively, the sign of $\frac{\partial O}{\partial p_D}$ will be negative if $\frac{\partial r}{\partial p_D}$ is sufficiently small (i.e., not very negative). However, if victim reporting behavior is sufficiently responsive to changes in p_D, then $\frac{\partial O}{\partial p_D}$ will be positive.

Finally, the reported crime rate, $C = r \cdot O$, combines these two impacts and also has an ambiguous direction of response to higher p_D:

$$\frac{\partial C}{\partial p_D} = \frac{\partial r}{\partial p_D}O + rG'(\cdot)\Big[-\frac{\partial r}{\partial p_D}(ax + ap_D D) - raD\Big]$$

The first term is negative and the second term is ambiguously signed, so the impact is also ambiguous.

This simple model gives us a clear prediction for the policy's impact on victim reporting, but it also clarifies why the impacts on offending and crime rates are ambiguous. As long as $\frac{\partial r}{\partial p_D}$ is negative and r factors into an offender's decision to commit a crime, $\frac{\partial O}{\partial p_D}$ is ambiguously signed. Note also that the model shows that the policy's impact on the offending rate is not necessarily the same sign as the impact on the reported crime rate. Specifically:

Proposition 1 (Relationship between Crime and Reported Crime). $\frac{\partial O}{\partial p_D} \leq 0 \Rightarrow \frac{\partial C}{\partial p_D} \leq 0$ and $\frac{\partial C}{\partial p_D} \geq 0 \Rightarrow \frac{\partial O}{\partial p_D} \geq 0$. But, $\frac{\partial O}{\partial p_D} \geq 0 \nRightarrow \frac{\partial C}{\partial p_D} \geq 0$ and $\frac{\partial C}{\partial p_D} \leq 0 \nRightarrow \frac{\partial O}{\partial p_D} \leq 0$.

Extensions

We next outline various extensions that relax the simplifications in the basic framework outlined above. In particular, we extend the framework to incorporate citizens and allow for other features of the setting to respond to a change in immigration enforcement policy.

Framework with Citizen Victims and Offenders — Until now, we have assumed that all victims and offenders are unauthorized. Here, we allow for a share of victims α_c to be citizens and for a share of offenders γ_c to be citizens.[38] For simplicity, we assume that offenders cannot choose whom to target, so they face a uniform reporting probability, and all victims face the same offending rate.

The same cost-benefit decision from the baseline framework applies to citizen and non-citizen victims of crime, so that the share of reported incidents is:

$$r = (1 - \alpha_c)F(b - \delta p_D D) + \alpha_c J(b)$$

where $J(h) \in [0, 1]$ is the distribution of hassle costs for citizens. Notice here that citizens' reporting decisions are not a function of immigration enforcement (i.e., $p_D = 0$). Just like in the baseline framework, the reporting probability responds to a change in p_D as follows:

$$\frac{\partial r}{\partial p_D} = -(1 - \alpha_c)F'(\cdot)\delta D \leq 0$$

Again, the change in the reporting probability with respect to increased immigration enforcement will be negative.

Analogously, the number of offenses is:

$$O = (1 - \gamma_c)G(M - rax - rap_D D) + \gamma_c K(M - rax)$$

where the costs of crime have distribution $K(c) \in [0, 1]$ among citizen offenders and this group does not factor immigration enforcement into their offending decisions (i.e., $p_D = 0$).

The number of offenses depends on both the change in deportation risk for non-citizen

[38] For notational simplicity, we assume here individuals are either citizens or unauthorized immigrants (non-citizens), but extending the framework to include non-citizen, authorized immigrants would yield the same conclusions.

offenders and how the probability of reporting has changed:

$$\frac{dO}{dp_D} = (1-\gamma_c)G'(\cdot)\Big[\underbrace{-\frac{\partial r}{\partial p_D}(ax + ap_D D)}_{\text{Lower Reporting} \uparrow \text{Crime}} \quad \underbrace{-\, raD}_{\text{Deterrence} \downarrow \text{Crime}}\Big] + \gamma_c K'(\cdot)\Big[\underbrace{-\frac{\partial r}{\partial p_D}(ax)}_{\text{Lower Reporting} \uparrow \text{Crime}}\Big] \lesseqgtr 0$$

Just like before, the two expressions inside the brackets for non-citizen offenders have opposite sign, so the impact of this change on non-citizen offenders remains ambiguous. In contrast, we expect citizen offenders' likelihood of offending to unambiguously increase given the decline in the reporting probability. However, on net, the overall impact on offending is ambiguously signed.

Changes in Citizens' Reporting Behavior — On the victim side, we have modeled the reporting decision to be a function of an individual's own probability of deportation and the cost of deportation. For citizens, $p_D = 0$, so their reporting decisions are unchanged following changes in immigration enforcement (although on aggregate we still expect the overall reporting rate to decline due to non-citizens' responses). However, prior work shows that citizens can also alter their behaviors in response to immigration enforcement, especially if they live in mixed-status households (e.g., Alsan and Yang, 2022). Such concerns for family members or neighbors could therefore also enter as an additional cost into individual reporting decisions. Notationally, we denote this extra cost with ηp_D, reflecting the (actual or perceived) probability that a neighbor or family member will be referred to immigration officials following victim reporting. Hence, non-citizens report if $h < b - \delta p_D D - \eta p_D D$ and citizens report if $h < b - \eta p_D D$.

The reporting probability responds to a change in p_D as follows:

$$\frac{\partial r}{\partial p_D} = -(1-\alpha_c)F'(\cdot)(\delta D + \eta D) - \gamma_c J'(\cdot)\eta D$$

In this scenario in which individuals factor in their family's or neighbors' probability of deportation into their reporting decisions, we expect an even *larger* decline in the aggregate reporting rate relative to the scenario in which individuals only consider their own probability of deportation.

A related extension is one in which citizens, especially Hispanic citizens, worry about their *own* likelihood of being (unlawfully) detained because of heightened immigration enforcement. In such a scenario, individuals might be less likely to report crimes to the police not because of empathy for their non-citizen neighbors, but because of increased fear of becoming ensnared in the immigration system. Here, we would also expect a larger decline in the aggregate reporting rate relative to the baseline framework.

Changes in Benefits to Reporting — Returning to the baseline framework, we have modeled the benefits of reporting b for unauthorized immigrants as constant and unrelated to the immigration enforcement environment. However, it could be the case that such benefits change in response to changes in p_D, so that changes in reporting with respect to immigration enforcement are:

$$\frac{\partial r}{\partial p_D} = -F'(\cdot)\Big(\delta D - \frac{\partial b}{\partial p_d}\Big)$$

As one example, consider a scenario in which victims of crime face backlash from their community for calling the police to report an incident — especially in communities with high shares of unauthorized immigrants — so that $\frac{\partial b}{\partial p_d} < 0$. In that case, we would expect the aggregate reporting rate to decline even more so than in the baseline framework. Alternatively, consider a scenario in which victims of crime are scared of offender retribution, and thus prefer deportation over traditional punishments, in which the offender may return to the community relatively soon after and could seek revenge. In this case, $\frac{\partial b}{\partial p_d} > 0$. Here, the direction of the reporting response would depend on the relative sizes of δD and $\frac{\partial b}{\partial p_d}$, so we expect the change in reporting to be ambiguously signed.

Changes in Police Effectiveness — In the baseline framework, we have assumed that the probability of apprehension a does not change with a change in p_D. If victims (and/or witnesses) are less willing to cooperate with investigations of reported crimes, we may expect a decline in a. In contrast, a could increase if reported crime declines and police have more resources to devote to each incident. Allowing a to change in response to p_D, we see:

$$\frac{\partial O}{\partial p_D} = -G'(\cdot)\Big[\underbrace{\frac{\partial r}{\partial p_D}(ax + ap_D D)}_{\leq 0} + \underbrace{raD}_{>0} + \underbrace{\frac{\partial a}{\partial p_D}(rx + rp_D D)}_{\lesseqgtr 0}\Big]$$

The ambiguity in the direction of $\frac{\partial a}{\partial p_D}$ leaves the prediction for $\frac{\partial O}{\partial p_D}$ ambiguous as well. However, it is worth noting that in a scenario in which the decline in reporting outweighs the cost of offending (so that the number of offenses is expected to increase), a decline in police effectiveness a would further contribute to the increase in criminal victimizations.

Distribution of Offender Costs — We also assumed that offenders' cost of offending c are unchanged by a change in p_D. The Secure Communities program intensified fears of participating in formal labor markets (East et al., 2018), so it may induce a leftward shift in the distribution of c, acting as another driver of more offenses O.

Offender Incapacitation — While offenders respond to the probability of apprehension and deportation, the baseline model does not allow apprehension to affect the total number of potential offenders. We consider here a simple extension of our baseline model that makes this allowance, whereby deported individuals are not able to offend in the future. To allow this feature requires considering dynamics, given that in every period some offenders are deported and are no longer available to offend in the following period. However, we also need to allow for entry of offenders, so that the pool of offenders does not continuously shrink.

An individual is deported if they offend, their victim reports, the police apprehend them, and they are referred to immigration enforcement, which occurs with probability $Orap_D$. We assume that in every period there is a mass λ of new potential offenders who enter the economy. In addition, a share of non-deported individuals, θ, exit the economy. If there are N_t potential offenders in period t, the following period's number can be represented as

$$\begin{aligned} N_{t+1} &= \lambda + [N_t(1-O) + N_t O(1 - rap_D)](1-\theta) \\ &= \lambda + N_t[1 - (1-\theta)Orap_D - \theta] \end{aligned}$$

We will consider steady-state equilibria, where the number of potential offenders is constant across time, so $N_t = N_{t+1} \equiv N$. Solving for this equilibrium gives us:

$$N = \frac{\lambda}{Orap_D(1-\theta) + \theta}$$

Now, we can express the total number of offenses and reported crimes as a function of this mass of potential offenders:

$$\text{Number of offenses:} \quad NO = \frac{O\lambda}{Orap_D(1-\theta)+\theta}$$
$$\text{Number of reported offenses:} \quad rNO = \frac{rO\lambda}{Orap_D(1-\theta)+\theta}$$

Incorporating the number of potential offenders adds an additional margin along which changes in p_D could impact overall offending, NO. This margin is summarized by $Orap_D$, which is the number of individuals who are deported in a given period. It is now no longer the case that the change in total offending always has the same direction of response as the change in the "per capita" offending rate.[39] In particular, even if the offending rate is unchanged ($\frac{\partial O}{\partial p_D} = 0$), overall offending could still decline in response to the policy if the mass of potential offenders shrinks ($\frac{\partial Orap_D}{\partial p_D} > 0$).

[39] In the baseline framework, there is no distinction between total offending and per capita offending. Here, we distinguish between these concepts by allowing the number of offenders to change with the policy.

C NCVS Sample Design

C.1 Overview of Survey

The National Crime Victimization Survey (NCVS) is a nationally representative survey that collects information on criminal victimizations. The survey is sponsored by the Bureau of Justice Statistics (BJS) and the Census Bureau serves as the primary data collection organization. The survey interviews around 240,000 individuals ages 12 or older (in around 150,000 households) every year.

To select survey respondents, the Census Bureau randomly selects addresses across the country to represent the country's population. Once that address is selected, individuals living at that address respond to the survey either in person or by telephone (though the first interview is supposed to be in person), and the interview lasts around 25 minutes. Households residing at the selected address are then interviewed every six months for a total of seven interviews over three years. If a new household moves into the selected address at some point during the three-year period, then the new household begins answering the survey (i.e., the survey follows addresses, not households). NCVS interviews are conducted continuously throughout the year with rotating groups: new addresses are incorporated into the survey every month to replace outgoing addresses that have completed their three-year interview process.

For more information on the NCVS sampling design, we refer the reader to Bureau of Justice Statistics (2014).

C.2 Survey Non-Response

There are three types of "missing" data in the NCVS. As in most surveys, there is item nonresponse when a respondent completes part of the survey but does not answer one or more individual questions. The second type of non-response is a person-level non-response, in which an interview is obtained from at least one member at the selected address, but an interview is not obtained from other eligible persons at that address. This could occur if a person is not home or is unwilling or unable to participate in the survey.

The final type of non-response is a household nonresponse, which occurs when an interviewer arrives at the selected address but is not able to obtain an interview. This could occur — similarly to the person-level non-response — because the household is not home or is unwilling or unable to participate in the survey. However, this type of non-response could also occur for other reasons (e.g., if the living quarters are vacant or the address is no longer used as a residence). Interviews that do not occur despite the persons in the household being eligible for the interview are referred to by the Census Bureau as "Type A" interviews.[40]

[40] "Type B" interviews refer to those in which the sample household is no longer eligible for interview, but could become eligible later (e.g., a vacant address). "Type C" interviews refer to those

Field representatives are instructed to keep Type A interviews to a minimum (for example, by contacting respondents when they are most likely to be home). If household members refuse to be interviewed by telephone, the field representative is required to make a personal visit to the address to conduct the interview (an interview cannot be labeled a "Type A" interview without an in-person visit).

After the data collection, the Census Bureau creates weights to adjust the sample counts and correct for differences between the sample and population totals.[41] Throughout our baseline analysis, we use person-level weights to maintain sample representativeness. This weight incorporates a non-response weighting adjustment that allocates the sampling weights of both non-responding households and non-responding persons to respondents with similar characteristics.

In Section 6.2, we use the fact that the NCVS indicates whether an address did not respond to a survey to consider the possibility that sample attrition might be biasing our estimates. We also leverage the panel nature of the survey to hone in on certain subgroups (i.e., Hispanic individuals present at each of their interviews). In Section 6.3, we conduct additional robustness checks to consider whether compositional changes of survey participants could be driving our main results. Throughout these checks, we fail to find evidence that these concerns are meaningful in our context, providing confidence that our results are due to behavioral changes in victimization and reporting, rather than due to changes in the survey sample over time.

in which the address should be permanently removed from the sample (e.g., the housing unit has been demolished).

[41] There is a non-response weighting adjustment that allocates the sampling weights of non-responding households to households with similar characteristics. Furthermore, there is a *within*-household non-response adjustment that allocates the weights of non-responding persons to respondents.

D Details on Deconvolution Procedure

In Section 7.4, we estimate a series of county-specific treatment effects from the Secure Communities program. These estimated effects are a sum of the true county-level effect and estimation error, $\hat{\beta}_c = \beta_c + \epsilon_c$, where ϵ_c is normally distributed and has a standard deviation that is estimated by $\hat{\sigma}_c$. Our goal is to identify the distribution of true effects, $f(\beta_c)$. Supposing that the effects can take on a finite set of values on a fine grid, $\beta_c \in \{\beta^k\}$, the likelihood of observing a county with a given estimated effect can be written as $\Pr(\hat{\beta}_c) = \sum_{\{\beta^k\}} f(\beta^k) \cdot \Pr(\epsilon_c = \hat{\beta}_c - \beta^k)$. We estimate the distribution $f(\beta^k)$ using maximum likelihood, where we parametrize the effect distribution with a four-parameter exponential family distribution (Efron, 2016). To produce the density of *estimated* effects in Figure A.14, we conducted the same deconvolution procedure but divided all standard errors by 100. For Census disclosure purposes, we chose this procedure as an alternative to the standard kernel density approach, where each point in the distribution is estimated on a potentially small set of observations.

Two-Dimensional Deconvolution

In Section 9.2, we use a two-dimensional deconvolution to estimate the county-level correlation between reporting and victimization impacts from Secure Communities.

We denote the true county-level reporting and victimization impacts of SC by β_{rc} and β_{vc}, respectively, and the estimated effects by $\hat{\beta}_{rc}$ and $\hat{\beta}_{vc}$. The likelihood of observing a given pair of estimates is given by

$$Pr(\hat{\beta}_{rc}, \hat{\beta}_{vc}) = \sum_{\{\beta^r\}} \sum_{\{\beta^v\}} f(\beta^r, \beta^v) \times Pr(\epsilon_r = \hat{\beta}_{rc} - \beta^r, \epsilon_v = \hat{\beta}_{vc} - \beta^v)$$

where $\{\beta^r\}$ and $\{\beta^v\}$ are fine grids of points. We make the simplifying assumption that the error terms ϵ_r and ϵ_v are independent. Each error term is normally distributed, and its standard error is estimated from the county-specific regressions.

We parametrize the distribution of true effects to have an exponential family distribution:

$$\begin{aligned} f(\beta_r, \beta_v) = \exp\big(&\alpha_0 + \alpha_1\beta_r + \alpha_2\beta_r^2 + \alpha_3\beta_r^3 \\ &+\alpha_4\beta_v + \alpha_5\beta_v^2 + \alpha_6\beta_v^3 \\ &+\alpha_7\beta_r\beta_v + \alpha_8\beta_r^2\beta_v + \alpha_9\beta_r\beta_v^2\big) \end{aligned}$$

The intercept term α_0 is set so that the function sums to 1 across all values of β_r and β_v.

We estimate the parameters $\alpha_1, ..., \alpha_9$ through maximum likelihood. We then calculate the correlation between β_r and β_v as $\sum_{\{\beta^r\}} \sum_{\{\beta^v\}} (\beta^r - \hat{\mu}_{\beta^r})(\beta^v - \hat{\mu}_{\beta^v}) \hat{f}(\beta^r, \beta^v)$, where $\hat{\mu}_{\beta^j} = \sum_{\{\beta^r\}} \sum_{\{\beta^v\}} \beta^j \hat{f}(\beta^r, \beta^v)$, for $j = r, v$. We calculate standard errors for the correlation by a bootstrap procedure, where each iteration draws an 80%-sized sample of county-level estimates with replacement.

E Description of Police Administrative Data

We acquired micro-data on 911 calls and arrests from police departments across the country for the years 2006 to 2013. Every 911 observation records the date, time, and address of the incident, as well as a basic description of the call type. Each arrest observation also records the date, time, and address where the arrest occurred, as well as basic demographic information on the arrestee including their age, gender, and race/ethnicity.

Coverage of Outcomes — The data were obtained through public records requests to medium and large cities in the U.S. We only include cities in our sample that provided data for all months in the period between October 2006 and December 2013. In addition, we only include cities that satisfy our baseline sample restriction of non-border counties, counties with $> 100,000$ residents in 2000, and outside of Massachusetts, Illinois, and New York. We have 75 cities that satisfy these restrictions and have either calls or arrest data (or both). The list of cities in our data is reported in Table E.1.

We have information on 911 calls for 52 cities and information on arrests for 48 cities, and 25 cities provided information on both outcomes. Likewise, a subset of the cities that provided data on arrests also provided information on the race/ethnicity of arrestees, allowing us to determine the Hispanic arrestee share (44 cities).

Linking Data to Census Tracts — Each city's data provides either an address or longitude/latitude location for most observations. We geocode these variables to identify the 2000 Census tract of each observation.

For each city, some share of observations were unable to be linked to tracts due to missing or incomplete addresses. We assume that the rate at which an address cannot be linked to a tract is constant within a city, and we evenly "distribute" these counts across tracts, in proportion to each tract's share of 911 or arrest counts. We do this by multiplying the counts of calls and arrests in all tracts by a constant such that the total count for the whole city is equal to the original count including the cases without an assigned tract.

In some cases the address is truncated to report only the first two digits of the street number (e.g., "23XX Campus Drive, Evanston, IL."). In these cases, we imputed the missing digits to be either "01" or "02" and assigned a tract to the closest existing address.

Removing Officer-Initiated Calls — In most 911 call dispatch data, officer-initiated interactions will appear in the data (these records are notifying the dispatcher that the officer is occupied). An important cleaning step is to remove these calls from the final count, which is meant to reflect the volume of *reported* requests for police assistance by civilians. This is also a metric of the volume of reported crime incidents by victims.

We identified officer-initiated incidents by hand-coding the categories of call types. We assigned two research assistants to code each city's 911 call categories. In cases where they disagreed on whether a category should be designated as officer initiated, a third research assistant made a final decision.

Sample Selection — Our initial set of 75 cities come from public records requests with complete coverage of the sample window and where the data quality of the obtained records met a minimum threshold. We vetted data quality by plotting the time series of outcomes to identify odd breaks, trends, or levels that likely stem from data quality issues.

We collapse the data to be at the level of tracts-by-months. Because the data comprise every call taken and arrest made by a department, some tracts only appear rarely. Many of these cases occur because of enforcement actions taken outside of a department's typical jurisdiction. To restrict attention to tracts that we are confident are consistently covered by the department, we take several sample selection steps: (1) we drop tracts in counties where the county contains less than five percent of all calls from an agency; (2) we drop tracts that are observed in more than one agency in our data; (3) in our calls data, we drop tracts that appear in fewer than 50% of months; in our arrest data, we drop tracts that appear in fewer than 5% of months. Our final data set has 3,730 tracts with calls data and 3,698 tracts with arrest data.

Table E.1: Cities with Police Administrative Data

	(1) 911 Calls	(2) Arrests
Antioch, CA	X	X
Austin, TX	X	
Avondale, AZ		X
Bainbridge, WA	X	X
Beaverton, OR		X
Bellingham, WA		X
Billings, MT		X
Cedar Rapids, IA	X	X
Charlotte, NC	X	X
Chico, CA		X
Cranston, RI	X	X
Durham, NC		X
Elk Grove, CA		X
Farmington, NM		X
Federal Way, WA	X	
Fresno, CA	X	X
Frisco, TX	X	
Gilbert, AZ	X	X
Glendale, AZ	X	X
Grand Rapids, MI	X	
Greensboro, NC	X	
Hartford, CT	X	
Hialeah, FL	X	
High Point, NC	X	X
Houston, TX	X	
Huntington Beach, CA	X	
Irvine, CA	X	X
Irving, TX	X	X
Kalamazoo, MI	X	X
Kennewick, WA		X
League City, TX	X	X
Lewisville, TX	X	X
Lexington, KY	X	
Long Beach, CA	X	
Longview, TX	X	X
Los Angeles, CA		X
Mansfield, TX	X	X

	(1) 911 calls	(2) Arrests
Maple Grove, MN	X	X
Marysville, WA		X
Melbourne, FL		X
Menlo Park, CA		X
Mesquite, TX		X
Miami, FL	X	X
Milwaukee, WI	X	X
Mission Viejo, CA	X	
Ontario, CA	X	
Overland Park, KS	X	
Pasadena, TX		X
Plano, TX	X	
Providence, RI	X	X
Reno, NV		X
Richardson, TX	X	
Rochester, MN	X	
Roseville, CA		X
Round Rock, TX		X
Sacramento, CA		X
San Clemente, CA	X	
Santa Clara, CA	X	X
St. Louis, MO	X	
St. Paul, MN	X	X
Stockton, CA	X	X
Sunnyvale, CA	X	
Surprise, AZ		X
Tallahassee, FL	X	
Temecula, CA	X	
Temple, TX	X	X
Topeka, KS	X	
Torrance, CA	X	
Tustin, CA		X
Ventura, CA		X
Virginia Beach, VA	X	X
Waco, TX	X	
Walnut Creek, CA		X
Wichita, KS	X	
Yorba Linda, CA	X	

NOTE: This table lists cities with police administrative data, described in Section 8 and Appendix E.

F Identifying Race of Marginal Offenders

In this appendix, we describe the calculations related to the ethnic composition of marginal offenders (i.e., those who offend against Hispanics because of SC but would not offend otherwise), as discussed in Section 8. We make several assumptions to identify the marginal offender Hispanic share, and we show that our estimate would be larger under violations of two key assumptions. We therefore treat our main estimate as a lower bound on the true marginal offender Hispanic share.

We start with a population N of potential offenders. Each has a Hispanic status, $H_i \in \{0,1\}$, and a share γ of the population is Hispanic. For each offender, they have a potential outcome O_{ij} that reflects whether they will offend in the presence of Secure Communities ($j = 1$) or not ($j = 0$). After offending, they face a probability of the victim reporting r_j. We allow the reporting probability to vary with the policy, but we will assume that all offenders face the same reporting rate regardless of their ethnicity. Then, there is some probability of apprehension conditional on reporting, a, which we also assume to be constant. We are interested in identifying the share Hispanic among offenders who would offend regardless of the policy, $O_{i0} = O_{i1} = 1$, a group that can be called the "always offenders." We are also interested in the offenders who only offend when the policy is in place: $O_{i0} = 0,\ O_{i1} = 1$. We call these the "marginal offenders," who can be thought of as "compliers" in the language of Angrist et al. (1996).

To identify the Hispanic share of these two groups, we will first make the strong assumption that there are no individuals who only offend *without* SC (i.e. $O_{i0} = 1,\ O_{i1} = 0$). Again in the language of Angrist et al. (1996), we are ruling out the presence of "defiers." This assumption is reasonable for non-Hispanic offenders, for whom the only effect of the policy is a decline in victim reporting. This assumption is stronger for Hispanics who face both reduced victim reporting and higher sanctions from offending. We consider below how our estimates would change with the inclusion of defiers.

First, we show that under our assumptions, the pre-SC Hispanic arrestee share identifies the Hispanic share of always offenders:

$$\begin{aligned}
\text{HispSharePre} =& \frac{\gamma N Pr[O_{i0} = 1 | H_i = 1] r_0 a}{N Pr[O_{i0} = 1] r_0 a} \\
=& \frac{\gamma Pr[O_{i0} = 1,\ O_{i1} = 1 | H_i = 1]}{Pr[O_{i0} = 1,\ O_{i1} = 1]} \\
=& Pr[H_i = 1 | O_{i0} = 1,\ O_{i1} = 1]
\end{aligned}$$

Next, we show how the post-SC Hispanic arrestee share is a weighted average of the always offenders and the marginal offenders:

$$
\begin{aligned}
\text{HispSharePost} =& \frac{\gamma N Pr[O_{i1}=1|H_i=1] r_1 a}{N Pr[O_{i1}=1] r_1 a} \\
=& \gamma \frac{Pr[O_{i1}=1,\ O_{i0}=1|H_i=1] + Pr[O_{i1}=1,\ O_{i0}=0|H_i=1]}{Pr[O_{i1}=1]} \\
=& \frac{Pr[O_{i1}=1,\ O_{i0}=1]}{Pr[O_{i1}=1]} Pr[H_i=1|O_{i1}=1,\ O_{i0}=1] \\
&+ \frac{Pr[O_{i1}=1,\ O_{i0}=0]}{Pr[O_{i1}=1]} Pr[H_i=1|O_{i1}=1,\ O_{i0}=0]
\end{aligned}
$$

Our estimates of the probability of offending pre-policy and post-policy can be used to estimate the weights on each Hispanic share:

$$
\begin{aligned}
\text{HispSharePost} =& \frac{0.009}{0.0105} Pr[H_i=1|O_{i1}=1,\ O_{i0}=1] + \frac{0.0015}{0.0105} Pr[H_i=1|O_{i1}=1,\ O_{i0}=0] \\
=& \frac{0.009}{0.0105} \text{HispSharePre} + \frac{0.0015}{0.0105} Pr[H_i=1|O_{i1}=1,\ O_{i0}=0]
\end{aligned}
$$

Where 0.009 is the pre-period victimization mean for Hispanic victims and 0.0015 is the treatment effect on victimization for this group. Using the share of Hispanic offenders in the pre- and post-period from the arrests data (described in Appendix E), we can then estimate the Hispanic share among marginal offenders π_m by solving:

$$
0.524 = \frac{0.009}{0.0105} 0.539 + \frac{0.0015}{0.0105} \pi_m
$$

This translates to $\pi_m = 0.43$.

Allowing for defiers — How would our estimate of the Hispanic share of marginal offenders change if we allowed for defiers? We show here that, in that case, the Hispanic share of marginal offenders would *increase*. We will allow only Hispanics to be defiers, $Pr[H_i = 1|O_{i0} = 1,\ O_{i1} = 0] = 1$. Revisiting our equations for the pre-policy Hispanic share:

$$\begin{aligned}
\text{HispSharePre} =& \frac{\gamma Pr[O_{i0}=1|H_i=1]}{Pr[O_{i0}=1]} \\
=& \frac{\gamma Pr[O_{i0}=1,\ O_{i1}=1|H_i=1]+\gamma Pr[O_{i0}=1,\ O_{i1}=0|H_i=1]}{Pr[O_{i0}=1]} \\
=& \frac{Pr[H_i=1|O_{i0}=1,\ O_{i1}=1]Pr[O_{i0}=1,\ O_{i1}=1]}{Pr[O_{i0}=1]} \\
&+ \frac{\overbrace{Pr[H_i=1|O_{i0}=1,\ O_{i1}=0]}^{=1}Pr[O_{i0}=1,\ O_{i1}=0]}{Pr[O_{i0}=1]} \\
\equiv& Pr[H_i=1|O_{i0}=1,\ O_{i1}=1]\lambda+(1-\lambda)
\end{aligned}$$

where λ is a number between 0 and 1 reflecting the share of pre-policy offenders who are always-offenders. $\lambda = 1$ corresponds to our previous assumption of no defiers. Now revisiting the calculation of the marginal offender share:

$$\begin{aligned}
\text{HispSharePost} =& \frac{0.009}{0.0105}Pr[H_i=1|O_{i1}=1,\ O_{i0}=1]+\frac{0.0015}{0.0105}Pr[H_i=1|O_{i1}=1,\ O_{i0}=0] \\
=& \frac{0.009}{0.0105}\frac{\text{HispSharePre}-(1-\lambda)}{\lambda}+\frac{0.0015}{0.0105}Pr[H_i=1|O_{i1}=1,\ O_{i0}=0]
\end{aligned}$$

With $\lambda = 1$, we have our previous estimate of $Pr[H_i = 1|O_{i1} = 1,\ O_{i0} = 0] = 0.43$. As λ decreases with the allowance of defiers, our estimate of the marginal offender Hispanic share increases, showing that our previous estimate was a lower bound.

Race-specific decline in reporting — What if the SC-induced decline in reporting differs by offender race? Victims may be less willing to contact the police if the offender is Hispanic than if the offender is non-Hispanic. In our conceptual framework outlined in Appendix B, such a scenario would arise if victims factor in the offender's ethnicity, and by extension their probability of deportation, into their reporting decision. We show here that allowing for a relatively larger reporting decline against Hispanic offenders will, again, increase the Hispanic share of marginal offenders.

The reporting probability that non-Hispanic offenders face under SC is still r_1, but the probability for Hispanics is now δr_1, where $\delta \leq 1$. In that case, we can express the Hispanic arrestee share as:

$$
\begin{aligned}
\text{HispSharePost} =& \frac{\gamma N Pr[O_{i1}=1|H_i=1]\delta r_1 a}{\gamma N Pr[O_{i1}=1|H_i=1]\delta r_1 a + (1-\gamma) N Pr[O_{i1}=1|H_i=0] r_1 a} \\
=& \frac{\gamma Pr[O_{i1}=1|H_i=1]}{\gamma Pr[O_{i1}=1|H_i=1] + \frac{1}{\delta}(1-\gamma) Pr[O_{i1}=1|H_i=0]} \\
=& \frac{\gamma Pr[O_{i1}=1|H_i=1]}{Pr[O_{i1}=1]} \times \frac{Pr[O_{i1}=1]}{\gamma Pr[O_{i1}=1|H_i=1] + \frac{1}{\delta}(1-\gamma) Pr[O_{i1}=1|H_i=0]} \\
=& \frac{\gamma Pr[O_{i1}=1|H_i=1]}{Pr[O_{i1}=1]}\mu, \quad \mu \leq 1
\end{aligned}
$$

The μ coefficient captures how much the Hispanic arrestee share differs from the Hispanic offender share. When $\delta = 1$, we have $\mu = 1$, and the two shares coincide as in our original setting. Otherwise, if $\delta < 1$, the Hispanic arrestee share is smaller than the Hispanic offender share.

Using this relationship and revisiting the calculation of the marginal offender share:

$$
\text{HispSharePost} = \mu \times \left[\frac{0.009}{0.0105}\text{HispSharePre} + \frac{0.0015}{0.0105} Pr[H_i = 1|O_{i1}=1,\ O_{i0}=0]\right]
$$

As μ decreases from 1, the implied marginal offender Hispanic share increases, showing that our baseline case is again a lower bound.

G Mediation Analysis of Victimization Increase

In this appendix, we provide further detail on the decomposition exercise discussed in Section 9.2. Specifically, we follow the mediation analysis framework and notation of Heckman et al. (2013) and Fagereng et al. (2021) to model how the increase in victimization that we find is related to a set of "intermediate" outcomes (i.e., mediators) that were also impacted by the Secure Communities program. To conduct this exercise, we first quantify the effect of Secure Communities on various mediator variables and then we utilize estimates from the economics literature to assess the plausible effect of these mediators on victimization.

G.1 Framework

We follow the mediation analysis framework and notation of Heckman et al. (2013) and Fagereng et al. (2021) to model how victimization relates to a set of "intermediate" outcomes impacted by Secure Communities. Let V_0 and V_1 be the counterfactual victimization outcomes for a given individual depending on whether Secure Communities is active in their county, and let $D \in \{0, 1\}$ denote Secure Communities activation status. The observed victimization outcome of an individual can be represented by $V = DV_1 + (1 - D)V_0$. The Secure Communities program can affect several "mediator" variables (beyond victimization), and these mediators may each be partially responsible for the increase in victimization. We denote these mediators by the vector $\theta_d = (\theta_d^j : j \in \mathcal{J})$ where $\mathcal{J}$ is the full set of mediators.

We assume that the relation between victimization and the mediators can be represented by the following linear model:

$$\begin{aligned} V_d &= \kappa_d + \underbrace{\sum_{j \in \mathcal{J}_p} \alpha_d^j \theta_d^j}_{\text{Measured Mediators}} + \underbrace{\sum_{j \in \mathcal{J} \setminus \mathcal{J}_p} \alpha_d^j \theta_d^j}_{\text{Unmeasured Mediators}} + \tilde{\epsilon}_d \\ &= \tau_d + \sum_{j \in \mathcal{J}_p} \alpha_d^j \theta_d^j + \epsilon_d, \end{aligned}$$

where κ_d is an intercept term, and α_d is a $|\mathcal{J}|$-dimensional vector of parameters. Here, $\mathcal{J}_p$ are the set of mediators that we can measure.

To simplify the analysis, we make the assumption that the causal effects of mediators on victimization do not depend on the Secure Communities program treatment status ($\alpha_1^j = \alpha_0^j \equiv \alpha^j$). Then, taking the expected difference between treated and untreated outcomes, we can decompose the overall effect of the program on victimization into a component explained by our observed mediators and a "residual" term:

$$\mathrm{E}[V_1 - V_0] = \underbrace{\sum_{j \in \mathcal{J}_p} \alpha^j \mathrm{E}[\theta_1^j - \theta_0^j]}_{\substack{\text{Treatment effect due} \\ \text{to observed mediators}}} + \underbrace{\mathrm{E}[\tau_1 - \tau_0]}_{\substack{\text{Treatment effect due} \\ \text{to unobserved mediators}}} \tag{4}$$

The left-hand side of this equation is the overall victimization effect of Secure Communities, reported in Table 2.

Our goal is to quantify the first expression on the right-hand side. To do so, we need estimates of $\mathrm{E}[\theta_1^j - \theta_0^j]$, measuring the effect of Secure Communities on each mediator variable, as well as estimates of α^j, measuring the effect of each mediator on victimization.

G.2 Effect of Secure Communities on Mediators

We follow the approach of East et al. (2018) and use the 2005–2014 American Community Surveys to quantify the effect of the Secure Communities program on labor market and demographic outcomes. To quantify the two-year effect of the program, we estimate a specification analogous to equation (1), as follows:

$$Y_{ct} = \beta_{\text{Post}}\text{SC}_{ct} + \mu_c + \delta_t + \epsilon_{ct}$$

where Y_{ct} is an outcome variable for county c at year t. SC_c is an indicator variable equal to one if county c had implemented the Secure Communities program for at least half of year t. μ_c and δ_t correspond to county and year fixed effects, respectively. Standard errors are clustered at the county level and the model is weighted by the county's 2000 population. The coefficient of interest is β_{Post}, estimating the average difference in outcome Y in the two years after the implementation of the Secure Communities program relative to the difference in the outcome prior to the program's launch.[42]

In this exercise, we consider four outcomes that Secure Communities may have affected beyond Hispanic individuals' reporting behavior. Specifically, we estimate the effect of SC on the employment-to-population ratio and logged hourly wages of Hispanic low-educated foreign-born individuals; the share of Hispanic household heads that are female; and the population share of Hispanic low-educated foreign-born men, which is a proxy for the unauthorized immigrant share of the population.[43] Like East et al. (2018), we define this final outcome as the number of individuals in this group divided by the working-age population in that county in 2005. Given time trends, we use a de-trended version of this outcome: specifically, we implement a two-step method in which we first estimate a linear trend for each county using pre-period data only, and then we subtract the fitted trend from all of the county's data points (Goodman-Bacon, 2021).

[42] There are a few notable differences between this specification and that in East et al. (2018). First, to remain consistent with NCVS data, we use counties — rather than commuting zones — as the measure of geography. Second, we quantify the two-year effect of the program, rather than focusing on all time periods after the program's implementation. And third, we omit county-specific linear time trends from the specification.

[43] East et al. (2018) shows large impacts on the employment and wages of low-educated foreign-born workers, so we similarly focus on this subgroup of Hispanics. For the immigrant population share, we additionally restrict our attention to males given the more marked decline in their population share.

G.3 Effect of Mediators on Crime

We convert each estimate from the literature into an elasticity of victimization with respect to the mediator. While this exercise requires making judgments about which estimates to borrow from the literature, we deliberately choose those that would lead us to understate the effect of reporting behavior and overstate the effect of the other mediators we consider.

For victim reporting, we rely on Golestani (2021), which studies nuisance property ordinances (NPOs) that increase the cost of contacting 911 for residents. This paper finds that NPOs lead to a -0.075 p.p. decline in victim reporting off a mean rate of 0.585 (Table 3) and a 0.21 p.p. increase in the likelihood of assault victimization off a mean of 1.5 (Table 6). These estimates imply an elasticity of assault victimization on reporting of -1.09. An alternative paper studying reporting behavior and crime is Miller and Segal (2019). This paper finds that a 10% increase in the female officer share leads to a 0.18 p.p. increase in victim reporting (Table 2, column 3) off a mean reporting rate of 0.55 (Table 1, panel A). A 10% increase in the female officer share leads to a 0.003 decline in the rate of domestic violence victimization (Table 6, column 1) off a mean victimization rate of 0.002 (calculation using paper's replication files). These estimates imply an elasticity of domestic violence victimization on reporting of 4.96. Given the larger elasticity in this study relative to Golestani (2021), we prefer to use the estimates from the latter paper.

For employment and wages, we rely on Gould et al. (2002), which studies the relationship between local labor market opportunities and crime rates. These estimates imply an elasticity of crime on unemployment of -1.66 and an elasticity of crime on wages of -1.35 (Table 3).

For the share of household heads that are female, we rely on Glaeser and Sacerdote (1999), which studies characteristics of cities that predict higher crime rates and finds that a significant portion of high crime rates can be explained by the presence of more female-headed households in cities. In particular, the estimates imply an elasticity of crime on female household heads of 1.46 (Table 5).

For the presence of male immigrants, we rely on Chalfin and Deza (2020), which studies the impact of labor market immigration enforcement on crime rates. This paper finds that the Legal Arizona Workers Act (LAWA) reduced Arizona's foreign-born Mexican (non-citizen) population share by 17%. After LAWA's passage, violent and property crime fell by 10.7% and 19.7%, respectively. These estimates imply an elasticity of violent crime on immigrant share of 0.63, and an elasticity of property crime on immigrant share of 1.16. Since 83% of victimizations are for property offenses (see Table 1), we weight the elasticities accordingly to get an overall elasticity of 1.07.

G.4 Implied Effect of Mediators on Victimization

The results from this exercise are displayed in Table A.10. The elasticities and corresponding sources are displayed in column 1. To calculate the implied effect of these mediators on victimization, we re-scale the implied elasticity by the average victimization rate of Hispanic individuals prior to Secure Communities (from the NCVS) and by the corresponding pre-period average of that mediator (from the ACS); these estimates are displayed in column 2. Column 3 displays SC's effect on the mediator using the ACS as well as the corresponding percent change. Finally, the predicted effect of the mediator on victimization (column 4) is the product of SC's effect on the mediator and the implied effect of the mediator on victimization.