Community Engagement and Public Safety: Evidence from Crime Enforcement Targeting Immigrants*

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Abstract

Increasing criminal enforcement can improve public safety by deterring or incapacitating offenders, but it may also alter community engagement with law enforcement. If victims fear interactions with police, they may be less likely to report crimes, reducing the probability of offender apprehension. This paper studies the Secure Communities program, a crime-reduction policy that involved local police in the detection of unauthorized immigrants who were arrested for criminal offenses. While the policy aimed to lower crime by deterring offenders, it also increased fear of deportation in immigrant communities. We show that the policy reduced the likelihood that Hispanic victims report crimes to the police and *increased* victimization of Hispanics. The number of crimes that are reported is unchanged, masking these opposing effects. We provide evidence that reduced reporting drives the increase in victimization, highlighting community engagement as a central determinant of public safety.

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Supplemental Appendix:
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Criminal activity imposes high social and economic costs, making the advancement of public safety a principal objective of government. The canonical model of the economics of crime posits that an increase in the certainty or severity of punishment will reduce crime because offenders respond to the higher expected cost of offending (Becker, 1968). In accordance with this framework, policymakers rely on criminal enforcement as an essential tool for reducing crime.

A competing consideration is how the population *beyond* offenders — in particular, victims of crime — might respond to enforcement. Victim cooperation is a key input into successful law enforcement, as it helps police detect crime and apprehend offenders. When enforcement intensifies, victims may perceive policies to be disproportionate or unfair, or they may fear interactions with law enforcement (Owens and Ba, 2021). If heightened enforcement reduces victim engagement with police, then it may actually *worsen* public safety. The effect of enforcement on community engagement is thus critical to understanding the impact of criminal justice policy.

This paper studies the effect of criminal enforcement on public safety, paying close attention to the role of victim behavior. Specifically, we study the U.S. Secure Communities program, a large-scale federal policy whose stated objective was to reduce crime. This program increased information sharing between local law enforcement and federal immigration authorities to facilitate the identification and deportation of undocumented immigrants arrested for criminal offenses. By involving local police in the detection of undocumented offenders, the program raised the expected cost of committing crime and thus offered the promise of improved public safety. However, a countervailing concern was that the policy would induce fear of deportation within immigrant communities and reduce victims' willingness to report crimes to the police. The effect of Secure Communities on public safety is thus ex-ante ambiguous. Indeed, the trade-off between deterrence and community engagement, central to this policy's net impact, is pervasive in broader debates about the efficacy of policing.[1]

[1] Scholars and policymakers have increasingly warned that traditional enforcement strategies, such as "broken windows" and "stop-and-frisk" policies, may reduce civilian cooperation with police and harm police effectiveness (see, e.g., Tyler et al., 2015). As a prominent example, the stated objective of President Obama's Task Force on 21st Century Policing was to "strengthen community policing and trust among law enforcement officers and the communities they serve" (Ramsey and Robinson, 2015).

The main empirical challenge to quantifying the impact of Secure Communities on public safety is that victim reporting directly impacts the measurement of crime. Most administrative data only include crimes that have been reported to law enforcement. Because Secure Communities plausibly affected victim reporting, examining the program's impact on *reported* crime is likely to yield biased estimates of the policy's impact on offending. This measurement concern also arises in other settings where victim reporting may respond to policies or events, such as high-profile police scandals (Ang et al., 2023) or domestic violence (Miller and Segal, 2019).

We overcome this measurement challenge by utilizing the National Crime Victimization Survey (NCVS), a nationally representative survey that 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 Secure Communities on the incidence of crime and on crime reporting behavior. Further, most administrative data consists of counts of total reported crimes with no demographic information, a substantial limitation in a setting where 90% of deported individuals are Hispanic. The NCVS includes the ethnicity of respondents, allowing us to separately estimate effects for Hispanics. We focus on effects for Hispanic individuals — including both citizens and non-citizens — consistent with prior work showing that Hispanic citizens also respond to enforcement policies due to fear that a family member or neighbor may be deported (Watson, 2014; Alsan and Yang, 2022).

To estimate the causal impact of Secure Communities, we leverage the differential timing of program implementation, which was staggered across counties due to resource constraints. We implement a difference-in-differences design and estimate effects separately for Hispanic and non-Hispanic respondents, comparing individuals of the same ethnicity before and after SC adoption across counties with different program status. We begin by showing that the program indeed led to sharp increases in immigrant detentions and deportations; detentions by Immigration and Customs Enforcement (ICE) rose by 54% following the program's introduction.

Nevertheless, contrary to the policy goal that heightened enforcement would improve public safety, we find that Secure Communities did not reduce overall offending and, in fact, *increased* crime against 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 estimate similarly sized but less precise increases in violent crime. In contrast, there is no change in the overall victimization of non-Hispanic individuals. There is, however, an increase in the victimization of non-Hispanics who live in areas with a high share of Hispanic residents. Across the full population (pooling all respondents), the estimates rule out declines in victimization of more than 3.3%, indicating quite precisely that the policy did not improve aggregate public safety.

At the same time, the policy reduced the likelihood that Hispanic victims report crime to the police. Hispanics reduce their reporting rate by 9 percentage points, a significant and sizable 30% decline relative to the pre-period reporting rate of 33 percent. The 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 program 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. The findings of increased victimization, lower victim reporting, and no change in reported crime are robust to accounting for survey attrition, ethnicity misreporting, and compositional changes among survey respondents and crime victims.

In the final part of the paper, we argue that the decline in victim reporting is the key driver of increased victimization. We first estimate victimization and reporting effects separately by cohort of program implementation. We show that cohorts with larger declines in reporting also experienced larger increases in victimization, indicating a clear link between these two outcomes.

We then examine how the policy impacted the ethnic composition of *offenders*. The policy induced a deterioration in Hispanics' economic outcomes (East et al., 2023), and an economic explanation for the increase in victimization would imply a relative increase in the share of Hispanic offenders. In contrast, a victim reporting explanation would imply a relative increase in *non-Hispanic* offenders, who face reduced apprehension probability but minimal change in deportation risk. To answer this question, we construct a novel dataset of administrative police records from 75 medium and large cities, which contain micro-data on individual arrests including information on arrestee demographics. We find that the policy reduced the share of arrested individuals that are Hispanic, consistent with the reporting decline being a key driver of the observed increase in crime.

Finally, we conduct a mediation analysis 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 on unemployment, wages, the share of female-headed households, and the male immigrant share of the population. The findings imply an elasticity of victimization-to-reporting of -0.5, highlighting that community engagement is central to public safety.

Our primary contribution is providing evidence that victim reporting is fundamental for the production of public safety. Criminal justice scholars have long studied the relationship between trust in law enforcement and willingness to call the police,[2] and recent empirical work has documented changes in victim reporting following events that alter perceptions of law enforcement legitimacy, like police violence (Ang et al., 2023; Zaiour and Mikdash, 2023) and immigration policies (Comino et al., 2020; Jácome, 2022). While prior work has posited that reduced victim reporting can directly harm public safety, existing evidence on this relationship has been limited to qualitative ethnographies and cross-sectional correlations[3] or has focused on domestic violence, in which the offender and victim have an intimate relationship (Miller and Segal, 2019; Golestani, 2021). Our paper advances this literature by providing causal evidence linking a decline in victim reporting with an increase in offending. This finding is an important contribution to the economics of crime literature, which has predominantly focused on how offenders respond to the enforcement regime with

[2] See Tyler and Huo (2002) and Xie and Baumer (2019) for reviews on the relationship between trust in law enforcement and calling the police within sociology and criminology.

[3] See, e.g., Wright et al. (1996) and Kirk and Papachristos (2011), respectively.

little attention paid to victim behavior.[4] Indeed, our setting is noteworthy in that we study a policy that was meant to reduce criminal offending but instead increased it, precisely because of an unintended impact on victim reporting. These findings thus provide novel evidence on the trade-off between deterrence and community engagement in the design of criminal justice policy.

More broadly, this paper speaks to key measurement challenges in the evaluation of criminal justice policy and shows that these issues can generate misleading conclusions. First, a small number of prior studies have emphasized the importance of accounting for crime reporting when measuring public safety (Carr and Doleac, 2016, 2018; Miller et al., 2022). However, due to data limitations, research studying the effect of enforcement policies has typically relied on administrative data on *reported* crime, primarily the FBI's Uniform Crime Reporting data.[5] We show that victim reporting can respond to an enforcement policy as much as offender behavior, so that changes in reported crime can differ significantly from changes in underlying crime. Second, our findings build on Harvey and Mattia (2022), illustrating that crime data without victim demographics can obscure group-specific effects in contexts where racial or ethnic minorities are disproportionately affected by law enforcement policies. Both of these measurement issues are crucial in our setting: our estimates would not have detected victimization impacts on Hispanics if we had relied on reported crime or ignored victim ethnicity.

The rest of the paper is structured as follows. Section 1 provides details on the Secure Communities program. Section 2 outlines a simple conceptual framework that builds on Becker (1968) but incorporates victim reporting decisions. Sections 3 and 4 introduce the data and empirical strategy. Sections 5, 6, and 7 discuss the main results, robustness checks, and heterogeneity exercises, respectively. Section 8 investigates the mechanisms underlying the rise in crime and concludes that the reporting decline is the primary cause.

[4] As an illustrative example, two recent reviews of the economics of crime literature do not mention victim behavior (Nagin, 2013; Chalfin and McCrary, 2017).

[5] For papers studying the effect of immigration enforcement on reported crime, see Miles and Cox (2014), Treyger et al. (2014), Pinotti (2015), Hines and Peri (2019), Chalfin and Deza (2020), Amuedo-Dorantes and Deza (2022), and Amuedo-Dorantes and Arenas-Arroyo (2022).

1 Institutional Background

In 2008, the United States launched the Secure Communities (SC) program, an information-sharing initiative intended to promote public safety.[6] This policy expanded the federal government's 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 expansion of local involvement in immigration enforcement (Cox and Miles, 2013).[8,9]

[6] See "ICE Unveils Sweeping New Plan to Target Criminal Aliens in Jails Nationwide," ICE News Release, Department of Homeland Security, 3/28/2008. This initiative aligns with the mission of the Immigration and Customs Enforcement (ICE) agency: to "Protect America through criminal investigations and enforcing immigration laws to preserve national security and public safety" (`https://www.ice.gov/mission`).

[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.

[8] Prior programs involving local participation in immigration enforcement included the Section 287(g) program and Criminal Alien Program (CAP). By the start of SC, 287(g) agreements were only in place in around 75 jurisdictions, and 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.

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., 2023; 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 scholars and policymakers warned that increasing local involvement in immigration enforcement would compromise engagement with police among immigrant communities, with adverse consequences for public safety (see, e.g., Kirk et al., 2012). In particular, police chiefs voiced concerns about decreased victim and witness willingness to report crimes because these civilian groups might fear increased risk of deportation.[10] These concerns are consistent

[10] In a 2009 opinion piece, LAPD chief William Bratton argued, "Every day our effectiveness is diminished because immigrants living and working in our communities are afraid to have any contact with the police. A person reporting a crime should never fear being deported, but such fears are real and palpable for many of our immigrant neighbors." "LAPD, not ICE," *Los Angeles Times*, 10/27/2009.

with qualitative interview-based research of offenders, which finds that offenders target Hispanic victims in part because these victims are less likely to contact police after they are victimized (Caraballo and Topalli, 2023).

Why would victims of crime fear interactions with police? While the SC program targeted individuals arrested for a criminal offense, there was a strong community perception that local police were now serving as immigration agents who could ask civilians about their own or others' immigration status as well as detain any unauthorized individuals (Kohli et al., 2011). 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). The patterns of actual ICE enforcement did not alleviate these concerns; around 20% of individuals transferred from local jails to ICE custody were not charged or convicted of a crime (see Figure A.6).[11] 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, a majority of surveyed Latinos reported feeling *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).

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

[12] E.g., "The Teens Trapped between a Gang and the Law," *The New Yorker*, 12/25/2017.

[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 in the investigation or prosecution of crime. 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*, 1/12/2023).

2 Conceptual Framework

We outline here a simple conceptual framework to illustrate the predicted impact of Secure Communities on both crime reporting and criminal victimizations. We build on the canonical Becker (1968) framework and extend it to incorporate the decision of victims to report crimes to the police. We operationalize the Secure Communities program as a change in the probability of detention, which can impact both offenders' and victims' decisions. 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 Supplemental Appendix B, we provide more details on this framework and discuss various extensions.[14]

Potential offenders have a single choice of whether to commit an offense, which they make by weighing the associated costs and benefits. 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.[15] 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

[14] Supplemental Appendix: https://elisajacome.github.io/Jacome/SupplementalAppendix.pdf

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

officials δp_D and the cost of deportation D.[16] 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 reporting r among victims also lowers this same cost by decreasing the likelihood that crimes are detected by police. The impact of the SC program on public safety thus depends on which of these effects dominates. While the overall effect on public safety is ambiguously signed, we would expect offending to increase for *non-Hispanic* offenders, who face a lower apprehension probability but minimal change in deportation risk (see Supplemental Appendix B for more detail). Finally, the sign of SC's impact on *reported* crime, $C = r \cdot O$, is also ambiguously signed and, crucially, may differ from the program's impact on criminal offending, O.

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.

[16] 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.

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) 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, or 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. For more details on the survey and its sample design, see Supplemental Appendix C.

Given the retrospective nature of the survey, we build a dataset at the person $\times$ year $\times$ month level corresponding to the years and months for which a respondent is answering. To construct the 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.

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 national population), 24.3 million Hispanic individuals (69% of the Hispanic population), and 7.4 million non-citizen Hispanic individuals (73% of the non-citizen Hispanic population) based on population counts from the 2000 Census (Manson et al., 2022). In robustness checks in Section 6, we consider the sensitivity of 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 enforcement intensity is honored detainers because it is available both before and after treatment and is more closely linked to deportation actions, as it only includes individuals who were transferred to ICE custody.[17]

We augment the analysis with local area characteristics. We use the 2000 Census and American Community Survey via IPUMS for 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

[17] 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.5 shows a strong, positive correlation (0.86) between county-level SC removals and honored detainers in the post-SC period.

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.

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 $\times$ 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 represents the program's impact on outcome Y in the two years after implementation.[18]

[18] To separately identify the two-year effect, we also include an additional indicator variable

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

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

where we denote $\tau = 0$ as the first quarter after SC activation for each county c, and we include event-time dummies SC_{ct}^{τ} to quantify the effect of the program for the eight quarters before and after the implementation of the program.[19] 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 is a binary variable denoting whether an individual is victimized at time t among all survey respondents, where the unit of observation is a person-month. The second outcome denotes whether a 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 that a person is victimized *and* reports the incident. This outcome is analogous to measures of reported crime rates available in administrative data (e.g., the FBI Uniform Crime Reporting program). For ease of exposition, we multiply outcome variables by 100.

Throughout the paper, we estimate regressions separately for Hispanic and non-Hispanic individuals. We report and interpret estimates for all Hispanic respondents — including citizens and non-citizens — in accordance with prior work showing that Hispanic citizens also respond to enforcement policies due to fear that a family member or neighbor may be deported (Watson, 2014; Alsan and Yang, 2022). We additionally consider non-Hispanic respondents in order to quantify any public safety impacts of SC on this group.

The identifying assumption for interpreting the estimates as the causal effect of the Secure Communities program is that individuals of a given ethnicity in earlier-

that is equal to one for all time periods beyond the two years after the program's launch.

[19] 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.

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).

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 using already-treated units to estimate the effect for 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., 2023) 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 the baseline sample (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. (2024) and Callaway and Sant'Anna (2021). While the concerns highlighted above are important, in practice the results are also quite similar using the standard TWFE model.

5 Results

5.1 First Stage: Impact on Detentions

We first establish that the Secure Communities program did in fact increase enforcement intensity. 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 immigrant detentions, consistent with findings in prior work (Alsan and Yang, 2022; Medina-Cortina, 2022). 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

Table 2 displays the estimates of 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 enforcement. A back-of-the-envelope calculation suggests that SC resulted in 875,000 additional crimes against Hispanics in the two years after program implementation among our sample of counties. Under the assumption that the results apply nationwide, the estimated effect translates to 1.3 million additional crimes.[20]

What does the increase in victimizations among Hispanic respondents imply for changes 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 full population 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

We now study 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 significantly 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 is consistent with a "chilling effect": individuals are afraid of interacting with law enforcement and are thus less likely to report victimizations. These results are consistent with Comino et al. (2020) and Jácome (2022), which find increases in Hispanic crime reporting (of 9% and 4%, respectively) following reforms that made the policy environment friendlier toward immigrants. These results also complement previous work documenting changes in Hispanic individuals' behaviors including lower participation in public assistance programs (Watson, 2014; Alsan and Yang, 2022) and fewer workplace complaints (Grittner and Johnson, 2022).

[20] We calculate this number by multiplying the monthly victimization effect by 24 (months) and by the Hispanic population (35.3 million).

5.4 *Reported* Crime Rates

Next, we consider an outcome that incorporates changes in victimizations as well as changes in reporting behavior: *reported* crime rates. 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 result is the consequence of reported crimes masking two opposing effects of Secure Communities, the increase in victimization and the decrease in reporting. For both groups, we find precisely estimated null effects, which are consistent with Miles and Cox (2014), Treyger et al. (2014), and Hines and Peri (2019), all of which rule out any meaningful effects on reported crime rates using administrative data.[21] This result stresses the importance of accounting for reporting behavior when estimating changes in public safety, especially when studying policies that may change both an offender's incentives to commit crimes as well as a victim's incentives to report crimes.

5.5 Effects by Crime Type

Finally, we separate victimizations into violent and property crimes. Table A.3 shows that the decline in reporting is primarily driven by a large, significant (34%) decline in Hispanics' willingness to report a property offense, with no analogous significant decline in the likelihood of reporting a violent offense. These results suggest that heightened enforcement dissuaded Hispanics from contacting police over non-violent incidents. We similarly find that the increase in victimization is driven by an increase (15%) in property crimes against Hispanics. To the extent that offenders respond strategically to changes in the probability of apprehension, then it is consistent to

[21] Table A.4 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).

see an increase in *property crime* victimizations. Finally, we also find a 15% increase in violent crimes, though we are under-powered to identify a statistically significant effect on this relatively rare outcome.[22]

6 Robustness & Alternative Hypotheses

6.1 Sample Construction and Empirical Specification

We consider here the robustness of the main results: the increase in victimization and the decline in reporting among Hispanic individuals. We first test the sensitivity of the results to sample construction choices, and report the findings for Hispanic individuals in Table A.5 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.5, we include these three states and shift the county population threshold from 100,000 to 75,000.[23] 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 (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, thus extending the sample window to March 2012 (row (4)). We also estimate a standard

[22] A concurrent paper in criminology (Baumer and Xie, 2023) uses logistic regressions in the NCVS to simultaneously estimate the effect of Secure Communities, the 287(g) program, and sanctuary policies on violent victimizations, controlling for time-varying and time-invariant individual, neighborhood, and county attributes. That paper finds that SC program activation and 287(g) agreements are associated with an 86% and 111% increase in violent victimizations of Latinos, respectively. We attribute the difference in our findings to the differing empirical strategies employed.

[23] We are unable to separately add these 3 states and lower the threshold given Census guidelines that prohibit the presentation of estimates for samples that only differ slightly.

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) (rows (5) and (6)). In all of these checks, we find that the results are robust.

Next, we add individual-level demographic characteristics as control variables to our baseline specification (row (7)). This model 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 changing during the program's rollout due to the Great Recession, and that these changes could simultaneously alter criminal behavior. We thus include a county's time-varying unemployment rate as an additional control variable (row (8)).[24] These results are likewise highly similar to the baseline findings.

Further, we test the sensitivity of the results to alternative difference-in-difference models following Borusyak et al. (2024) and Callaway and Sant'Anna (2021) (rows (9) and (10)). 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 (row (11)).[25] Across 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 coefficients from these alternative strategies, showing that the dynamic evolution of outcomes is comparable across these specifications.

Table A.6 and Figure A.6 show the results of analogous checks for non-Hispanic respondents. Across specifications, we continue to find no impact of SC on the likelihood of being victimized or on the likelihood of reporting a crime to the police.

Finally, one concern with the empirical strategy is that we focus on the county-specific rollout of the program, so the 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 the main outcomes by calen-

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

[25] To preserve statistical power, this specification controls for "cohort" groups of counties based on the year-month of SC activation instead of individual counties (i.e., using Hispanic×time, cohort×time, and cohort×Hispanic fixed effects).

dar 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 the 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 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 Supplemental Appendix C, the NCVS contacts a fixed 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 the top panel of Table A.7. Because we do not know a household's Hispanic composition if they do not respond to the survey, we use the address to perform versions of this analysis in Census tracts with increasingly larger Hispanic population shares. 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.[26]

Despite the reassuring evidence of minimal attrition, we use the point estimates

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

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)–(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 p.p. (or 3%, assuming 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 0.028 p.p. increase in victimization, 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 that were present at each of their interviews and thus do not leave the survey (row (12) of Table A.5 and Figure A.7).[27] We also estimate an OLS model with person-level 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 similar to our baseline effects, though the victimization effects lose statistical significance from reduced precision. The stability of the point estimates using subgroups of individuals who do not leave the survey further corroborates the conclusion that the results are

[27] The NCVS samples addresses for 3.5 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 households that responded to every survey and 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.

not driven by sample attrition.

Finally, a related concern could be that Hispanic individuals may be less likely to self-identify as Hispanic in the NCVS survey after the program's implementation, an effect that has been found in other contexts (e.g., Duncan and Trejo, 2011). We conduct two exercises to consider ethnic re-classification. First, we estimate the victimization and reporting effects on *all* survey respondents — regardless of self-reported ethnicity — that live in areas with high shares of Hispanic residents (i.e., Census tracts above the 75th percentile of the tract-level Hispanic share distribution) (row (14) of Table A.5 and Figure A.7). We continue to find a 0.12 percentage point increase in victimization (p-value= 0.05) and 6 percentage point decline in reporting (p-value= 0.03) among these respondents, indicating that changes in self-reported ethnicity are not driving the results. Second, we estimate a regression similar to equation (1) to quantify post-SC changes in the outcome of whether a respondent self-identifies as Hispanic in the NCVS. We find a 3% decline in the likelihood that respondents self-identify as Hispanic (Table A.7). A similar back-of-the-envelope calculation as the one for household response rates — which considers the worst-case scenario that all ethnic re-classification occurred among Hispanic respondents that were *not* victimized — indicates that this change can explain at most 20% of our victimization effect.

These findings in some ways contrast with prior work documenting the "chilling" effects of immigration enforcement on participation in public programs (Alsan and Yang, 2022; Santillano et al., 2020; Watson, 2014). We note a few features of NCVS interviews that may reconcile the high NCVS response rates and the minimal survey attrition we observe among Hispanic respondents following SC: NCVS interviews may be conducted in Spanish; field representatives (FRs) are instructed to make home visits if phone interviews are not possible and to do so when respondents are more likely to be home; and FRs send thank-you notes after every interview (Bureau of Justice Statistics, 2019). FRs also stress the confidentiality of the NCVS survey, which likely reduces the perceived risk of responding to this instrument.[28]

[28] https://bjs.ojp.gov/media/video/68736.

6.3 Respondent, Victim, and Crime Composition

We next address a concern closely related to sample attrition. If there are changes within 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. (2023) and Medina-Cortina (2022) find that SC induced an out-migration of low-educated foreign-born men after a county's implementation.

First, as noted above and shown in row (7) of Table A.5, we re-estimate the main specification including respondent 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 these 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 the 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 effects, implying that the victimization increase is not due to a changing pool of survey respondents.[29]

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.[30] To test for this concern, we conduct an analogous exercise to the one above,

[29] Standard NCVS weights include a survey attrition adjustment; Figure A.11 also uses alternative weights that do not adjust for attrition (results are nearly identical).

[30] Note that we find large, significant reporting declines when focusing on "always-responders" and when using individual-fixed effects (Table A.5), ruling out that the effect is driven by respondents with lower reporting rates entering the survey after SC.

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 whether the decline in reporting translates into a decline in the probability of an arrest. Without a victim report, it is unlikely that the police will be able to identify and apprehend an offender. Conversely, if the crimes for which reporting is reduced have arrest rates that are already quite low, then a change in reporting may not meaningfully alter arrest rates.

In Table A.9, we estimate 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. 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% decline. 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 focuses on 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% decline. 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 effects provides suggestive evidence that the decline in reporting translated to a lower arrest rate overall, but not necessarily among reported incidents.

7 Heterogeneity

7.1 Neighborhood Characteristics

We first consider whether individuals who live in neighborhoods with high shares of Hispanic residents have differing treatment effects. Specifically, we use a respondent's Census tract to estimate equation (1) 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). Figure A.13 plots treatment effects according to these characteristics separately for Hispanic and non-Hispanic respondents.

Panels (a) and (b) show results for Hispanics: because most Hispanic respondents live in areas with high shares of Hispanic residents, victimization and reporting effects are relatively constant as the neighborhood resident share of Hispanics (or non-citizen Hispanics) increases. However, SC's impact on victimization appears higher — a 25% increase — in neighborhoods with the highest shares of non-citizen Hispanics.

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 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 (row (8) in Table A.6 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 also experienced an increase in victimization.

7.2 County Characteristics

We first document overall variation in impacts by estimating equation (1) separately for each earlier-treated cohort of counties (i.e., counties that activated the program in the same year-month), using Hispanic respondents in later-treated counties as the comparison group. Figure A.14 displays the distribution of treatment effects. Among 31 cohorts, 28 have positive victimization treatment effects and 30 have negative reporting treatment effects. The distribution of reporting effects is relatively narrow, and clustered around a 10 p.p. decline. This pattern implies that the main results are not driven by a few select counties or cohorts, but rather that Hispanics across areas are experiencing increases in victimization and exhibiting declines in reporting.[31]

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 enforcement varied across counties with different characteristics, using ICE removals (deportations) in the two-year post-period to measure enforcement. Column (1) shows that counties with a higher share of non-citizen Hispanic residents have higher removals per capita, or greater *total* 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 enforcement — have larger victimization effects.[32] For reporting, we find minimal evidence that county characteristics predict differences in the treatment effects among Hispanic individuals. These findings are perhaps unsurprising given the limited variation in the decline in Hispanic reporting across cohorts of counties (Figure A.14).

[31] In Supplemental Appendix D, we conduct a similar exercise at the county level using a deconvolution procedure to address noise from estimation error. We find that 79% and 68% of counties have negative reporting and positive victimization effects, respectively.

[32] 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 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.

8 Is the Reporting Decline Driving the Increase in Victimization?

We have presented evidence that Secure Communities both 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 implicitly tied to victim reporting. However, in addition to affecting crime reporting behavior, the Secure Communities program led to multiple economic and social changes within Hispanic communities (e.g., East et al., 2023; Medina-Cortina, 2022; Amuedo-Dorantes et al., 2018), each of which may have had an impact on public safety. In this section, we present several pieces of evidence that suggest that the reporting decline we observe is a key driver of increased victimization.

8.1 Relationship between Victimization and Reporting Effects

We first ask whether cohorts with larger reporting declines experienced larger victimization increases, using the cohort-level treatment effects discussed in Section 7.2. Figure 5 shows a pronounced negative relationship between these outcomes, highlighting that larger reporting declines co-occur with larger victimization increases.

We estimate the slope of this negative relationship in two ways. First, we estimate a bivariate regression of estimated cohort-level victimization treatment effects on reporting treatment effects. This regression yields a coefficient of -0.008, significant at the 1% level. Next, we randomly split the baseline sample into two partitions and instrument for the reporting effect in one partition using the analogous estimate from the second partition. This split-sample approach addresses two potential concerns in the simple OLS regression: first, the victimization and reporting effects are estimated in the same sample, which could induce a spurious correlation between the effects, and second, the cohort-level reporting effects are measured with error, which could attenuate the estimated coefficient. Using this IV strategy, we find a larger negative coefficient of -0.017, significant at the 10% level, suggesting that this approach corrects for measurement error in the OLS estimation. We overlay the regression line from this preferred split-sample approach in Figure 5. The slope of the line suggests that for a 10 p.p. decline in reporting, we would expect to see a 0.17 p.p. increase in the victimization rate, very similar to the baseline findings. This robust negative relationship is consistent with victim reporting being a key input into public safety.

8.2 Hispanic Composition of Arrestees

Prior work has documented that Secure Communities affected the labor supply and wages of immigrant workers (East et al., 2023; Ali et al., 2024), a change which could have plausibly led to an increase in criminal activity. In this subsection, we consider the importance of victim reporting vis-à-vis changes in economic conditions.

In particular, the specific mechanism underlying the rise in criminal behavior has different implications for how the composition of offenders would change around the introduction of SC. On the one hand, if the increase in crime stems from Hispanic individuals having worse economic conditions, then we would expect the composition of offenders to be relatively *more* Hispanic after SC. On the other hand, if changes in reporting behavior are driving the increase in crime, then we would expect the composition of offenders to be *less* Hispanic. Reduced reporting promotes higher offending, but Hispanic offenders also face a higher expected cost of committing crime given the possibility of being detained and deported (see Supplemental Appendix B). The composition of offenders should thus change to have relatively fewer Hispanics.

To consider changes in the offender population, we augment the analysis using a novel data collection from individual U.S. police departments. While the NCVS is uniquely able to disentangle impacts on victimization and crime reporting separately by respondent ethnicity, it does not provide thorough offender information. Most victims in the survey are unable to provide information about the offender, and ethnicity information about offenders is limited.[33] Indeed, lack of information on offender characteristics is a pervasive challenge in the economics of crime literature (Doleac, 2023).

We circumvent this challenge by utilizing hand-collected micro-data on arrests from 75 municipal police departments for 2006–2013. Each arrest observation records the date and time when the arrest occurred as well as basic demographic information on the arrested individual, including Hispanic ethnicity. Importantly, these data include the address of the incident so that we can investigate differences by neighbor-

[33] 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.

hood type. We provide information on these data and our data cleaning and sample selection choices in Supplemental Appendix E.

Our goal is to understand the impact of SC on the ethnic composition of arrestees. We use the same specification as equation (1) to measure changes in the share of arrested individuals that are Hispanic. In these regressions, the unit of observation is a Census 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 August 2011 as the control group. Because we do not observe victim ethnicity in these data, we split our analysis by neighborhood ethnic composition; we designate the 25% of tracts in the sample with the highest Hispanic 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 in these tracts being 68%. The average Hispanic share for non-Hispanic neighborhoods is 14%.

Table 3 presents the findings for the full sample as well as separately for Hispanic and non-Hispanic neighborhoods. 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.[34]

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 Supplemental Appendix G, 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 ethnicity of marginal offenders. 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.

In sum, these results indicate that the composition of offenders changed after

[34] Appendix Table A.11 shows additional results for the impact of SC on the volume of 911 calls and arrests. These results align closely with the findings using NCVS data that SC did not change reported crime rates or arrest rates conditional on a reported crime.

the launch of SC to become *less* Hispanic. These findings are inconsistent with changes in economic conditions driving the increase in crime and instead suggest that changes in reporting behavior drove the rise in victimization.

8.3 Decline in Reporting vs. Other Economic and Social Changes

Next, we use a decomposition to assess the importance of the SC program's economic and social effects — relative to its impact on victim 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 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, and point the reader to Supplemental Appendix F for more detail.

Framework — In addition to the reporting rate, 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 employment-to-population ratio and logged hourly wage of low-educated foreign-born Hispanics (following East et al., 2023); 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 SC 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. The 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 yearly level and our baseline set of counties in the 2005–2014 annual American Community Surveys (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 choose to borrow estimates of α^j from the literature, rather than estimate them from our own data, because 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. Column 2 displays the implied effect of each mediator on victimization given these elasticities.

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 elasticities from the literature would have predicted an even *larger* victimization effect than the one we find. In Figure A.15 and Supplemental Appendix F, we show that the results from this decomposition are similar when using alternative estimates for the elasticity of victimization with respect to victim reporting. 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, the decomposition shows a notable negative "residual" effect. This finding is the converse of the 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.

Elasticity of Victimization to Reporting — The results from the mediation exercise lead us to conclude that the decline in victim reporting is the primary driver of increased victimization. We thus use the estimates to calculate the implied elasticity of victimization to victim reporting. Assuming that victimization increased *solely* because of reduced reporting, we estimate an elasticity of -0.59. Alternatively, if we use the mediation estimates to "residualize" the victimization effect of the components explained by social and economic changes, we estimate an elasticity of -0.49.[35] These calculations require that SC did not affect victimization through channels beyond reporting or the measured social and economic factors, and a violation of this assumption would be the deterrence effect of deportations. However, because the deterrence effect should promote lower victimization, we consider these calculations to be conservative estimates of the true victimization-to-reporting elasticity.

8.4 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.

Several pieces of suggestive evidence point against animus being the key driver of the 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

[35] Our residualized estimate of the victimization effect is $\mathrm{E}[V_1 - V_0] - \sum_{j \in \tilde{\mathcal{J}}_p} \alpha^j \mathrm{E}[\theta_1^j - \theta_0^j]$, where $\tilde{\mathcal{J}}$ consists of the social and economic mediators.

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.[36] Finally, we find an increase in non-Hispanic victimization in counties (and neighborhoods) with larger Hispanic populations (Table A.6, 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 animus is unlikely to be driving the policy's impact on victimizations.

9 Conclusion

We study increases in criminal enforcement resulting from the Secure Communities program, a large-scale federal policy implemented piecemeal at the county level between 2008 and 2013. In sharp contrast to the crime-reduction goals of the program, we find that Hispanic residents are significantly more likely to be victims of crime after its implementation. We also find a significant reduction in the crime reporting rate of Hispanic victims, consistent with heightened fear of interacting with law enforcement, and we argue that the decline in reporting is a key driver of increased Hispanic victimization.

The divergence between the public safety goals of the program and its actual effects is notable. Views about immigration policy can have broad political ramifications, and the public debate about immigration policy often centers on concerns related to immigrant criminality (e.g., Afrouzi et al., 2024; Ajzenman et al., 2023; Alesina and Tabellini, 2022; Couttenier et al., 2021; Dustmann et al., 2019). Within this debate, criminal enforcement policies which target immigrant offenders are often promoted as a way to lower crime rates. Our findings indicate that these policies may be ineffective, if not counterproductive, given their potential impacts on community engagement with the police.

More broadly, our study provides evidence that lower levels of trust in law

[36] See, for example, Afrouzi et al. (2024) and "Americans Still Value Immigration, but Have Concerns," *Gallup*, 7/13/2023.

enforcement, which can manifest via reduced reporting of crimes, can harm public safety by lowering the probability of offender apprehension. This finding highlights an important trade-off between the ability of enforcement to decrease crime through deterrence or incapacitation, and the potential for excessive enforcement to reduce the willingness of civilians to engage with law enforcement.

Furthermore, 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 remains constant after an increase in enforcement because the decline in victim reporting masks the increase in true victimization. Further, the 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.

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 explore how to design effective public safety 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 and community engagement can be improved.

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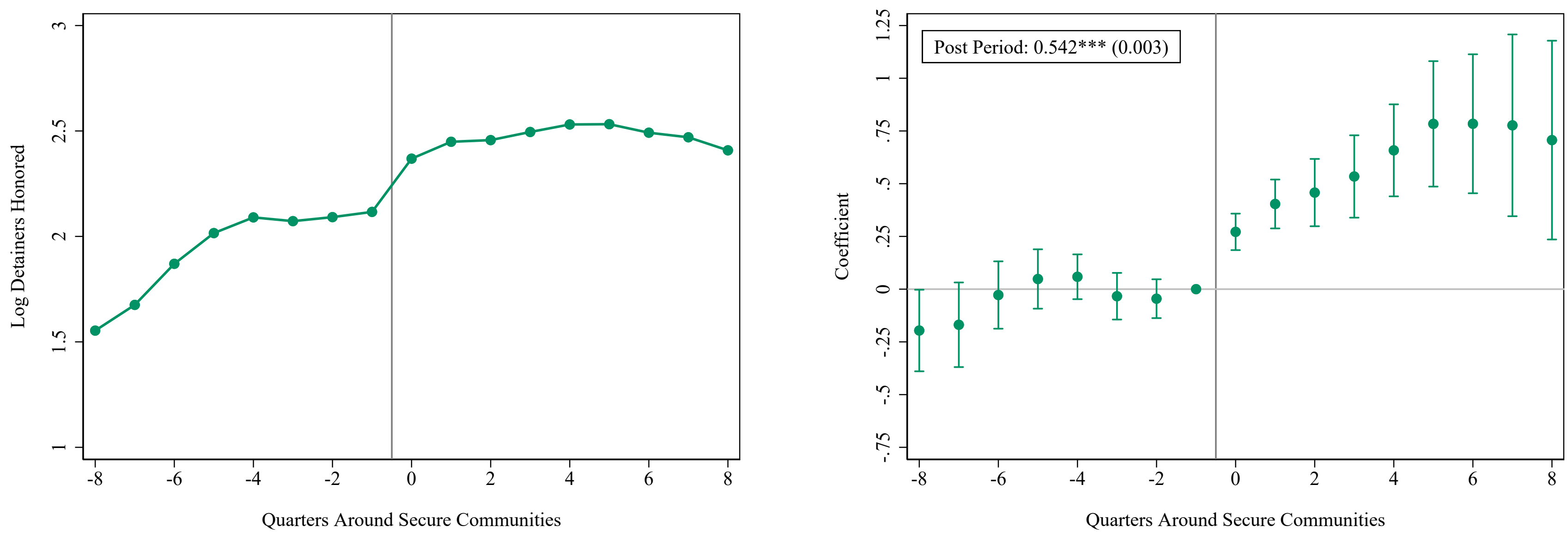
Figure 1: Logged Number of Honored ICE Detainer Requests around Secure Communities (SC) Implementation

(a) Raw Means

(b) Regression Estimates

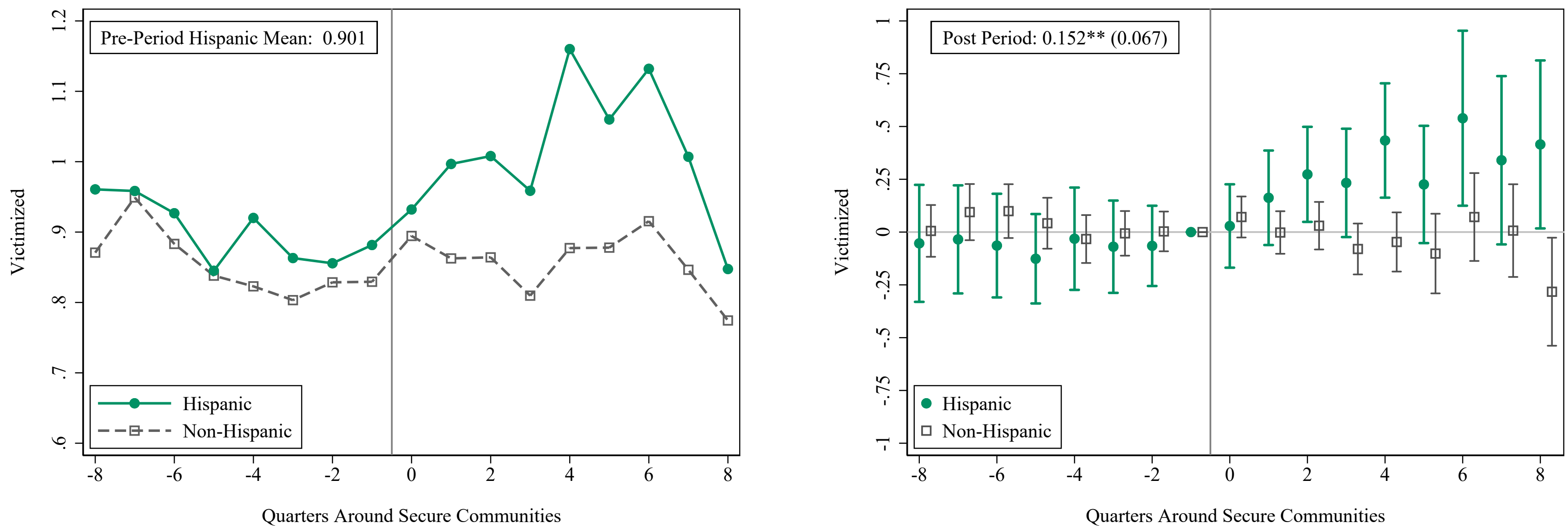


NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the logged number of honored ICE detainers for the eight quarters before and after the implementation of the Secure Communities program. An honored detainer request refers to an ICE detainer request record that indicates that an individual 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. 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

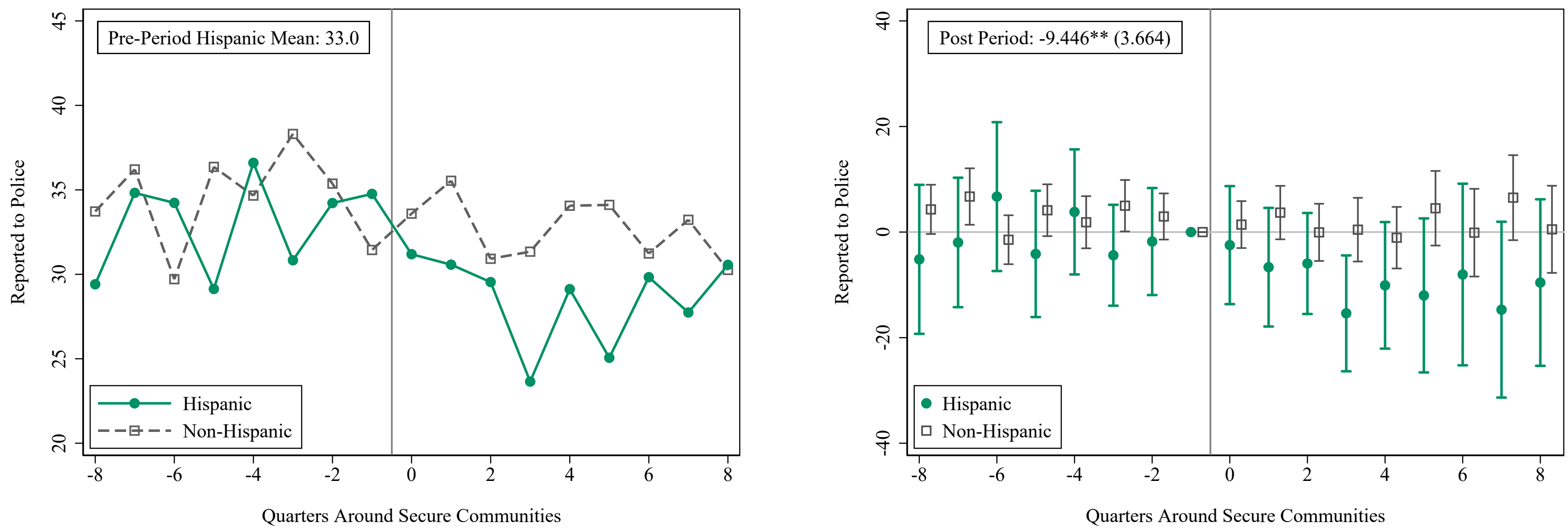


Note: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS respondents victimized for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3. 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

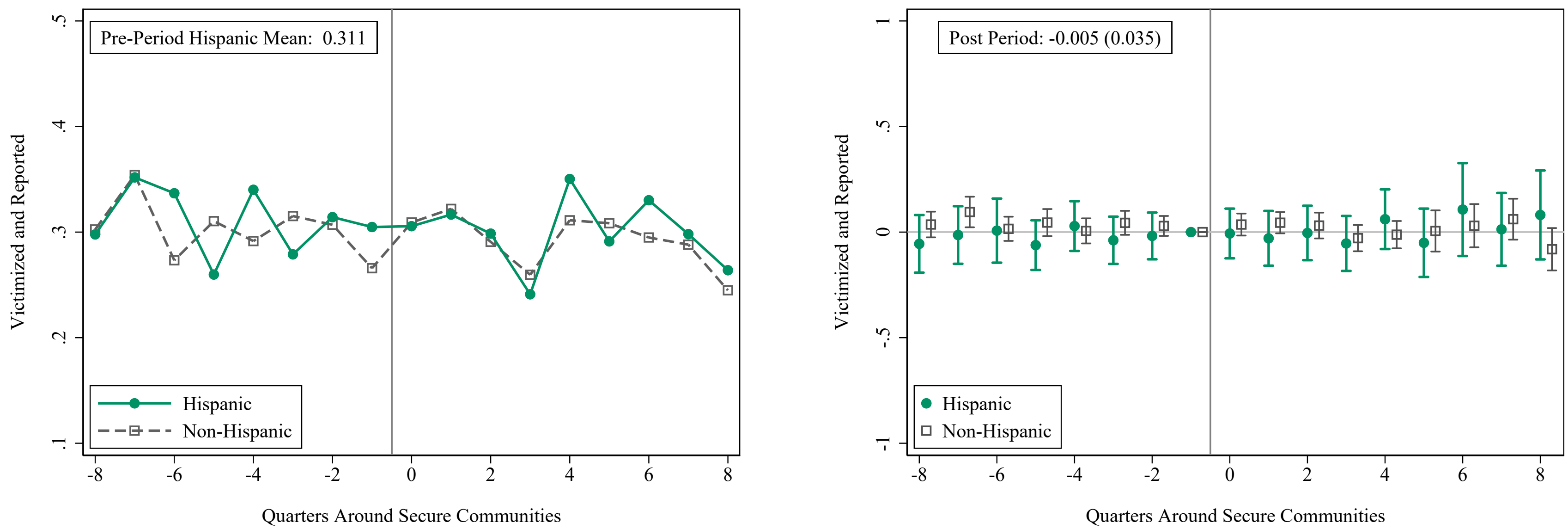


NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS crime incidents for which a victim reported the crime to police for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3. 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

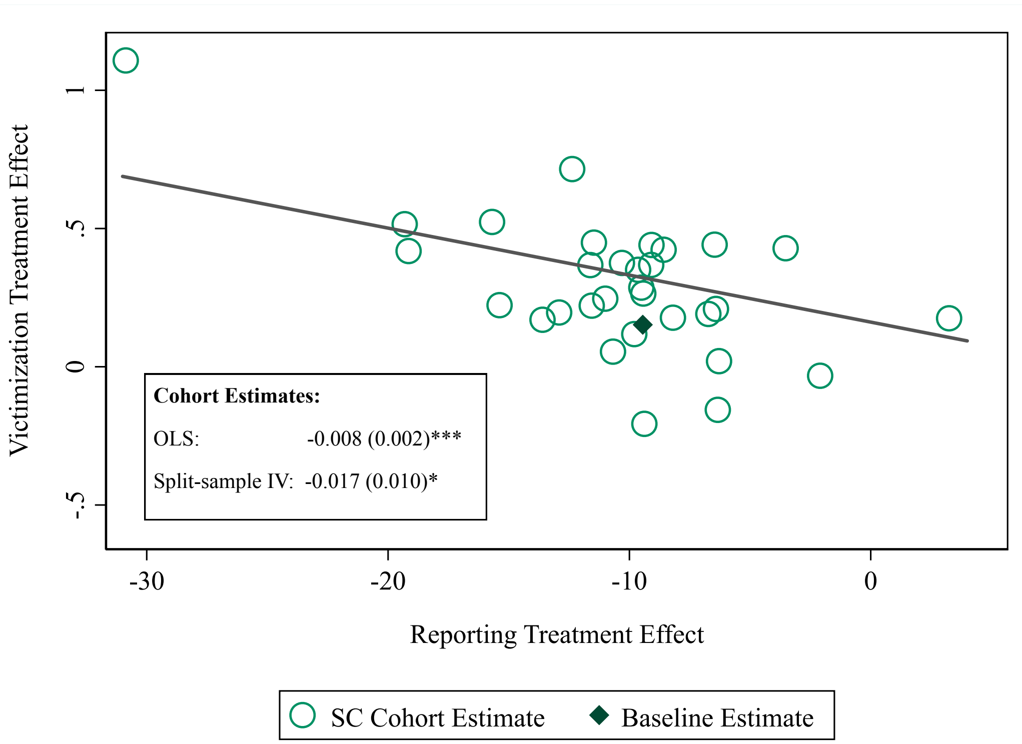
(a) Raw Data

(b) Regression Estimates



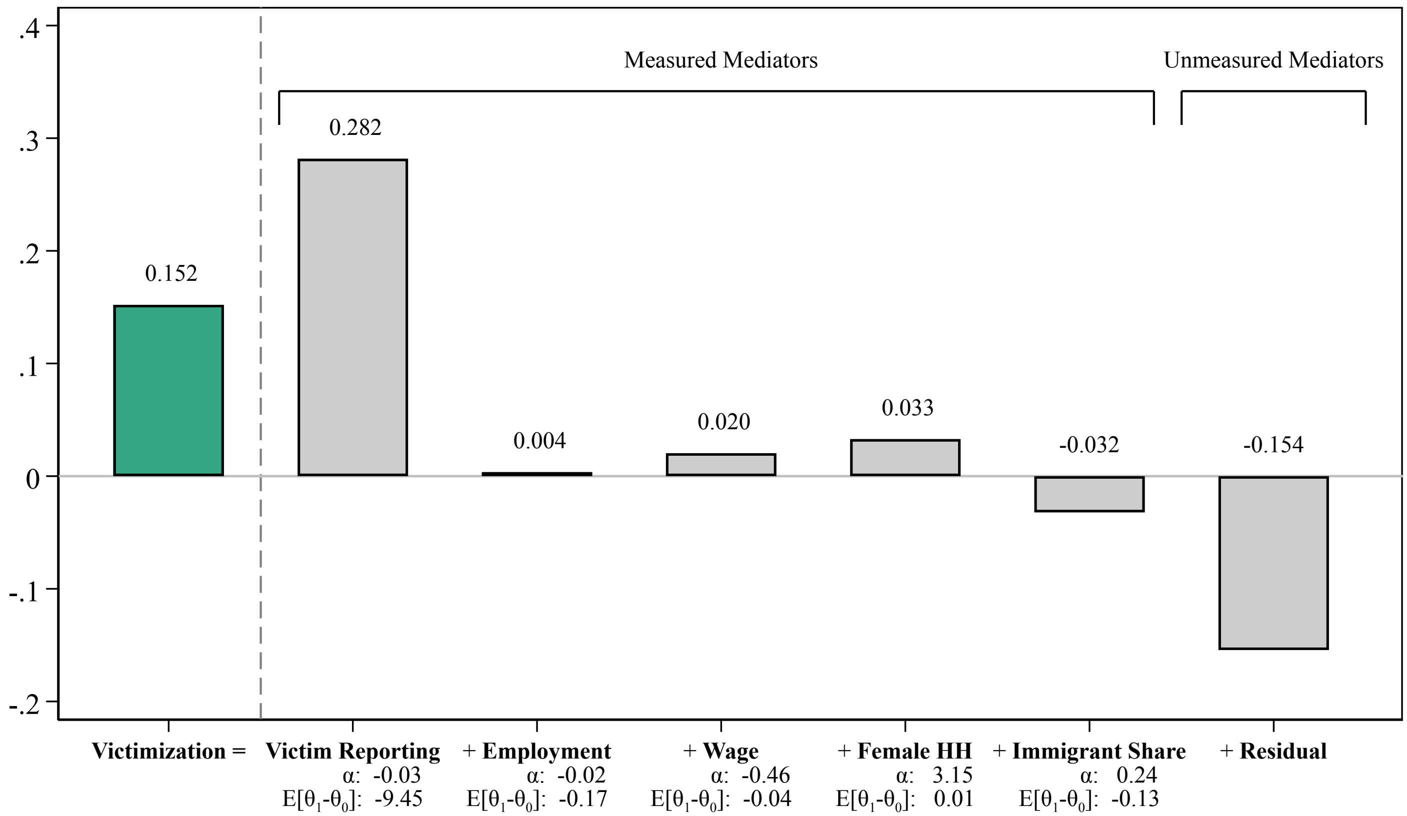
Note: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS respondents who were both victimized and reported the crime to police for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3. 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 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. "OLS" reports the coefficient of a regression of victimization cohort effects on the corresponding reporting cohort effects. Next, we randomly partition the sample in two parts and estimate cohort-specific reporting and victimization treatment effects in both sample partitions to address the joint determination of outcomes. "Split-sample IV" estimates a stacked regression of one partition's victimization cohort effects on that partition's own reporting cohort effects, instrumented by the other partition's reporting cohort effects. The plotted line corresponds to the "Split-Sample IV" estimates. Both of these models are weighted by the inverse square of the standard error of victimization effects.

Figure 6: Decomposing Victimization Increase into Various Components



Notes: This figure presents estimates from the decomposition outlined in Section 8.3 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 Supplemental Appendix F.

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) on the Hispanic Share of Arrests, Using Police Administrative Data

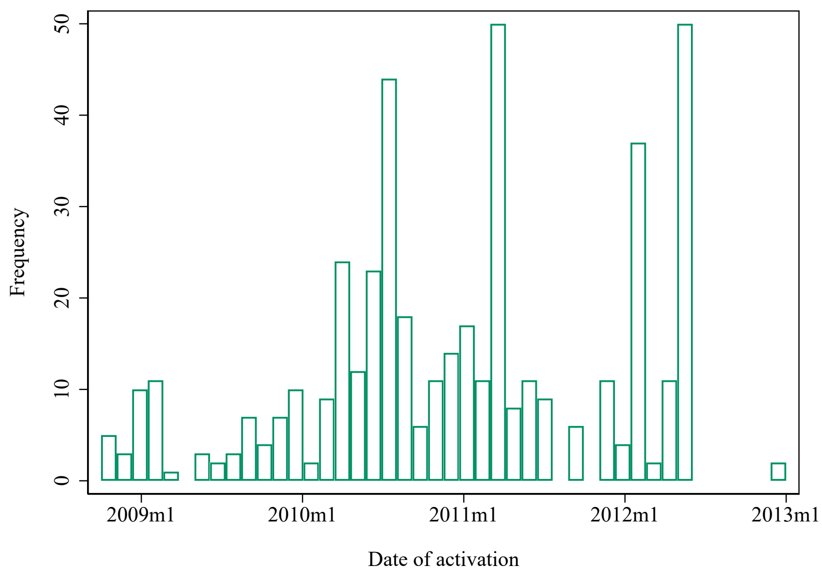
	All Tracts (1)	Hispanic Tracts (2)	Non-Hispanic Tracts (3)
β_{Post}	-0.006**	-0.015*	-0.004*
	(0.002)	(0.008)	(0.002)
Y Mean	0.280	0.539	0.147
Observations	81,892	27,848	54,044
Number of Cities	44	22	43
Tract Share Hispanic	0.328	0.677	0.147

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1). The main outcome — the share of arrested individuals that are Hispanic — is measured using micro-data from police administrative records, as described in Section 8.2. The unit of observation in each regression is a tract × year × month, and each regression includes city and time (year × month) 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).

FOR ONLINE PUBLICATION: APPENDICES

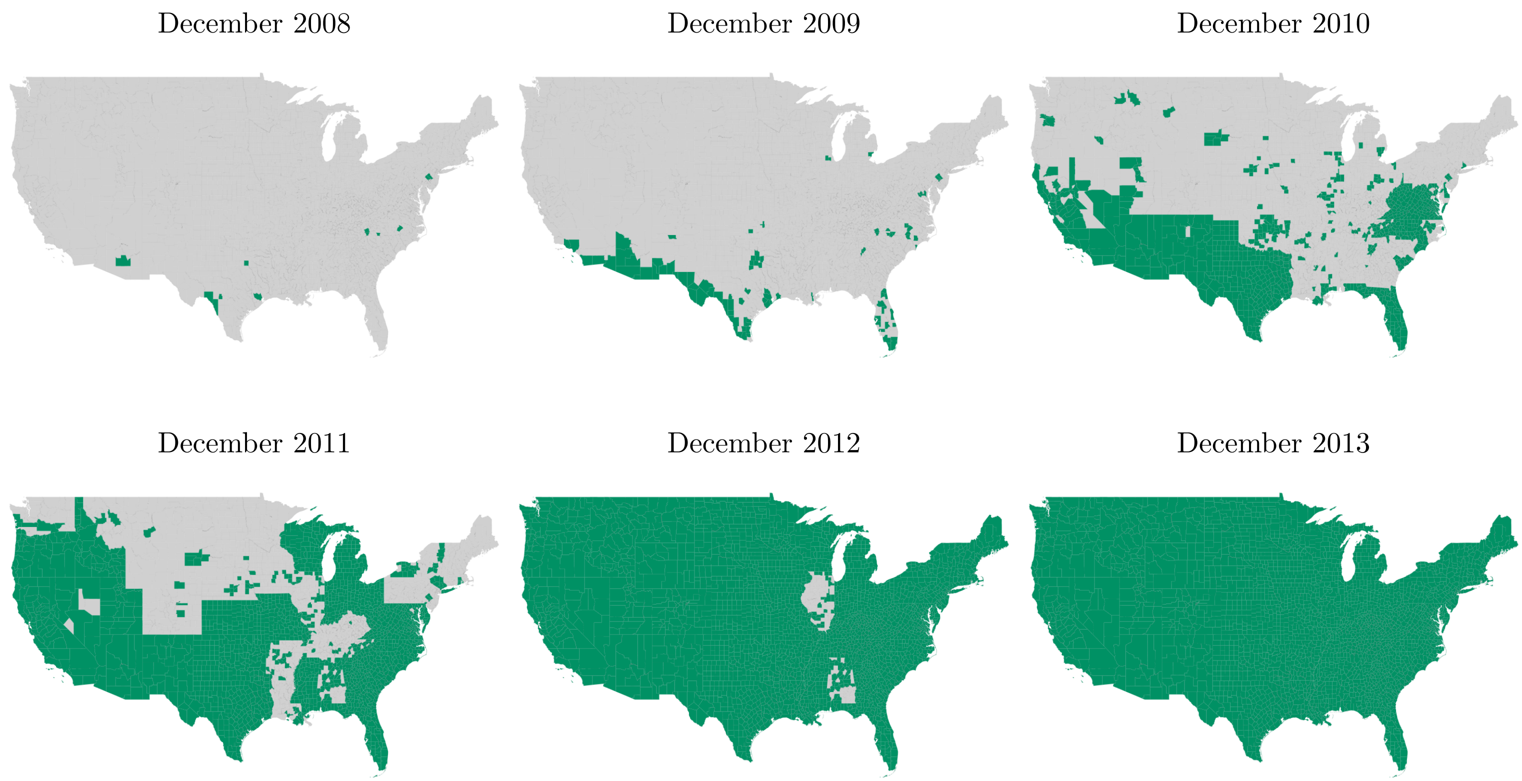
A Appendix Figures and Tables

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 (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).

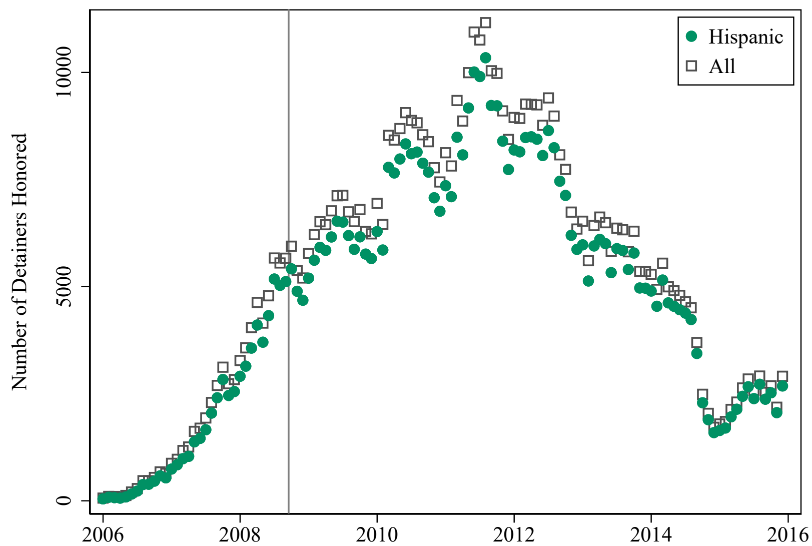
Figure A.2: Geography of Secure Communities (SC) Implementation



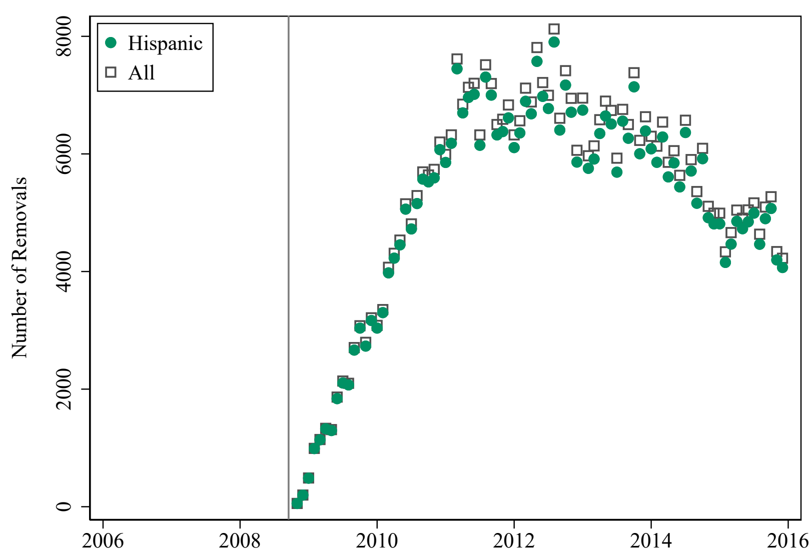
NOTE: This figure shows the county-level rollout of the Secure Communities Program over time, with counties that have implemented the program by each point in time highlighted in green.

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

(a) Number of ICE Detainers Honored

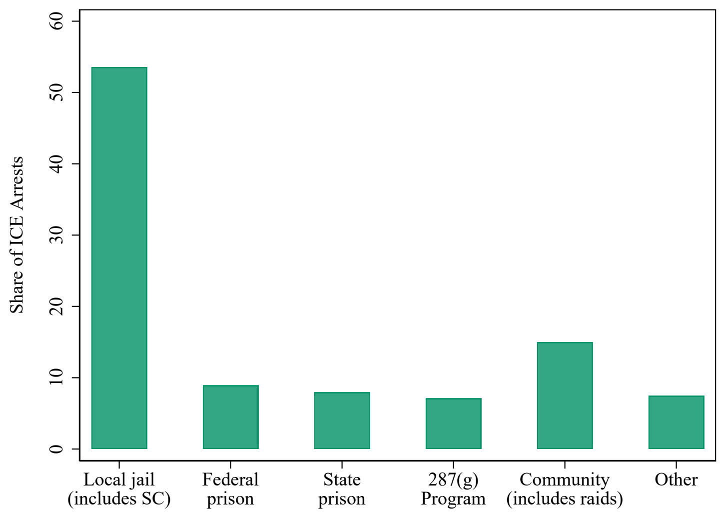


(b) Secure Communities (SC) Removals



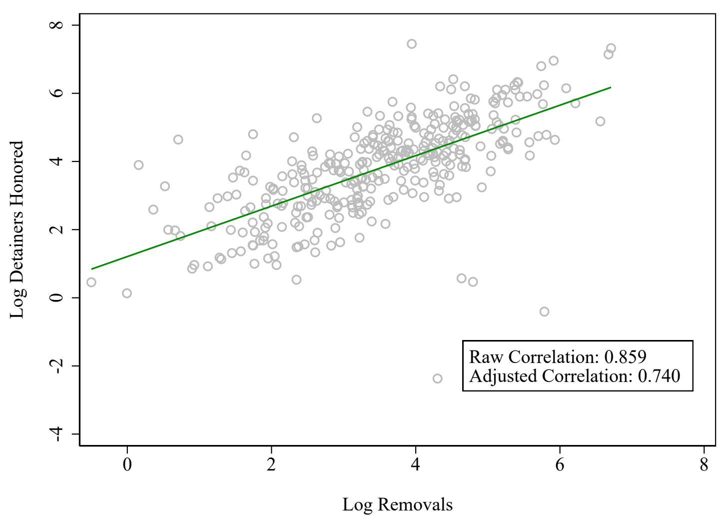
Notes: Panel (a) plots the number of monthly detainer requests honored by ICE using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. 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



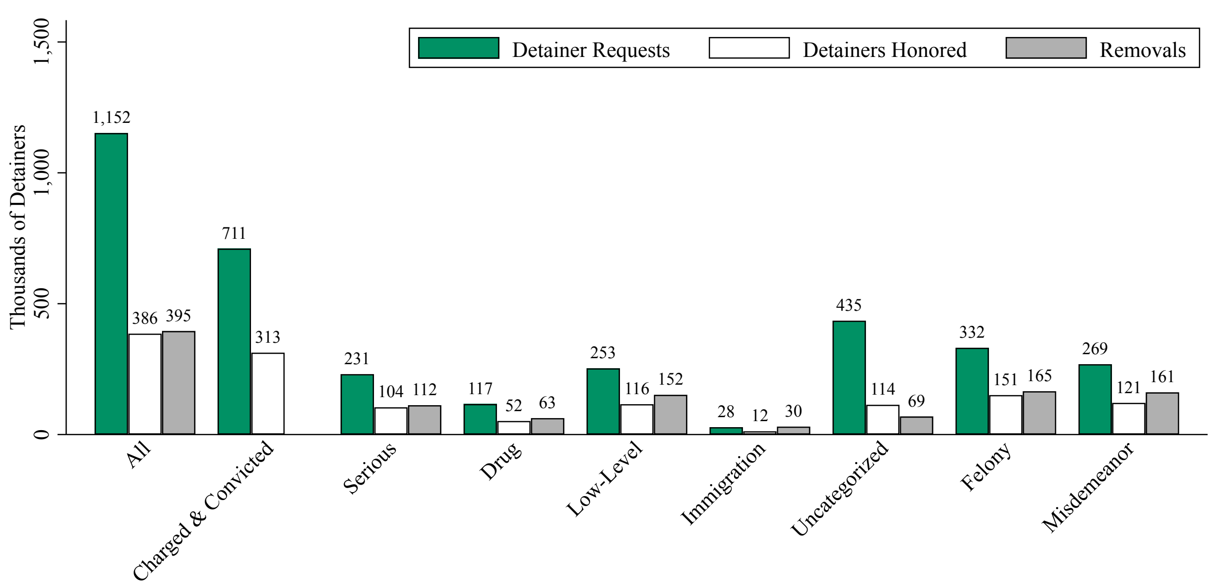
Notes: This figure plots the share of ICE interior arrests by source between 10/2008 and 8/2011 (TRAC, 2018). "Local" refers to individuals arrested by local police or sheriff's offices. "State" and "federal" refers to individuals who were transferred to ICE after being released from state and federal prison, respectively. "287(g) Program" refers to arrests that involve law enforcement agencies that had signed agreements through the 287(g) program. "Community" refers to individuals arrested at their homes, places of work, courthouses, etc.

Figure A.5: Relationship between ICE Detainers Honored and Removals



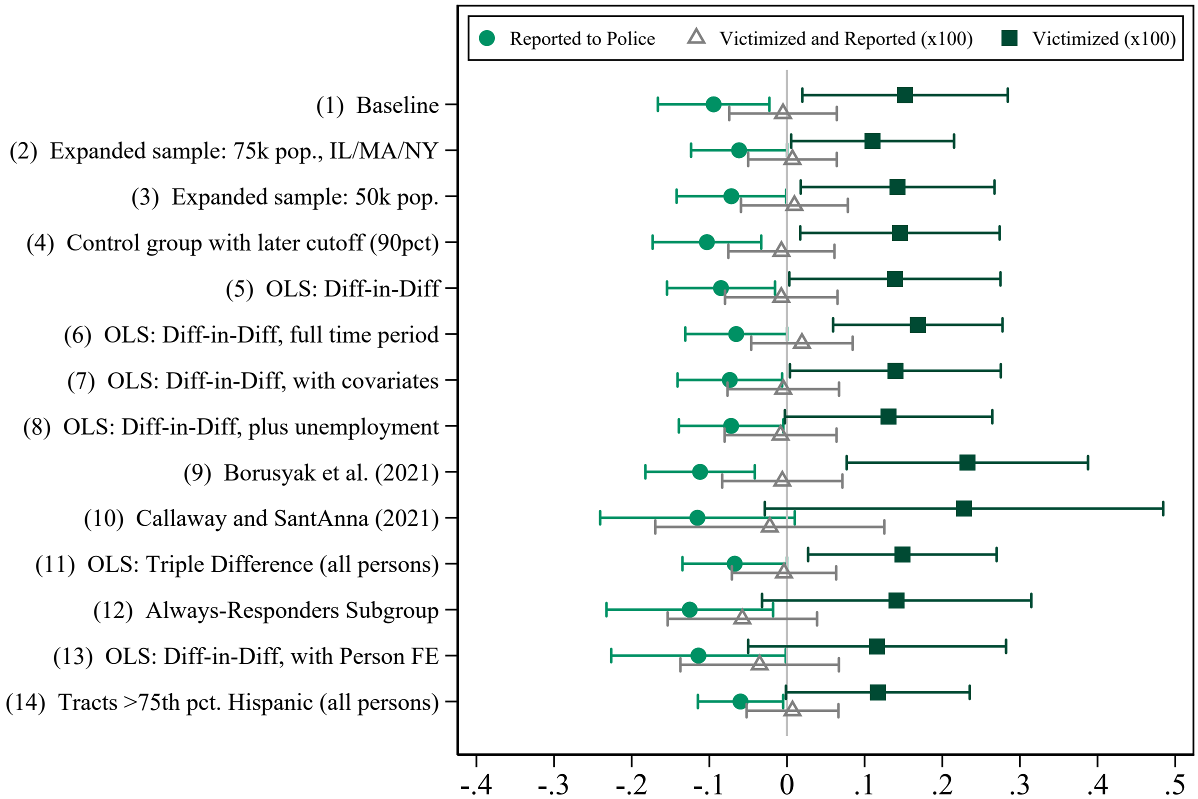
Notes: This figure plots the county-level relationship between the logged number of removals and the logged number of honored detainer requests during our sample window (8/2008-8/2011), adjusted for county-level log population, share Hispanic residents, and share non-citizen Hispanic residents (measured in 2000). Logged values are calculated as $\ln(Y + 1)$ to account for zero values. A removal is a record of an individual who was deported as a result of the SC program (records are only available after the program).

Figure A.6: ICE Detainer Requests, Detainers Honored, and Removals, by Offense Type



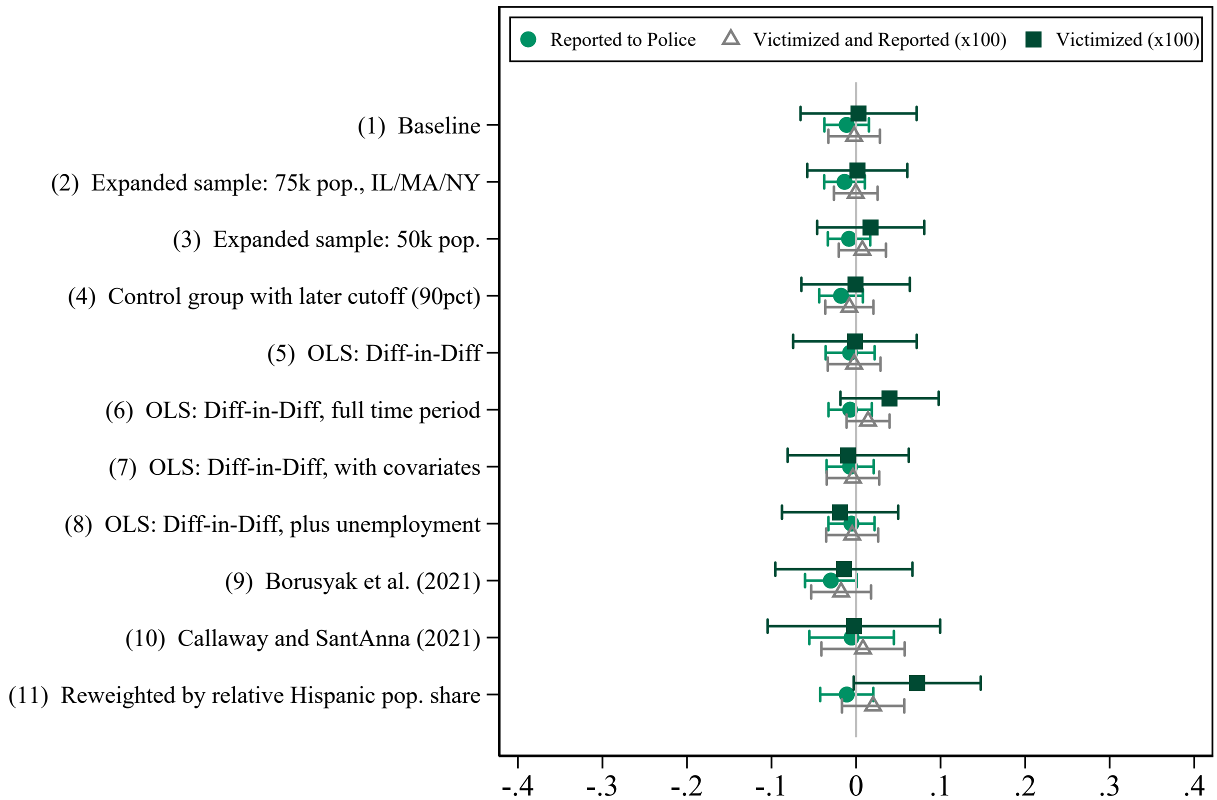
Notes: This figure shows the total number of ICE detainer actions by offense type using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. The data covers all actions after Secure Communities (SC) was implemented in each county from 10/2008 through 12/2014. A detainer 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.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) include additional states or lower 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 OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period through June 2015 (full sample period). (7) re-estimates (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. (2024) 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 ("always-responders"). (13) re-estimates (5), including person fixed effects. (14) produces a version of (1) using all respondents (regardless of self-reported ethnicity) living in tracts above the 75th percentile of the tract-level distribution of the share of the population that is Hispanic. "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. These estimates are also displayed in Table A.5.

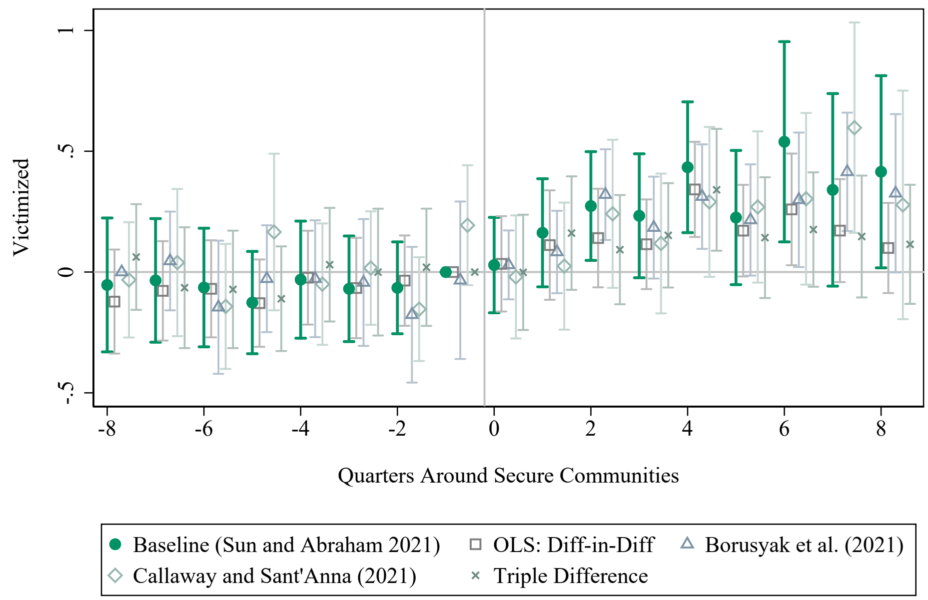
Figure A.8: Robustness of Main Results, Non-Hispanic Respondents



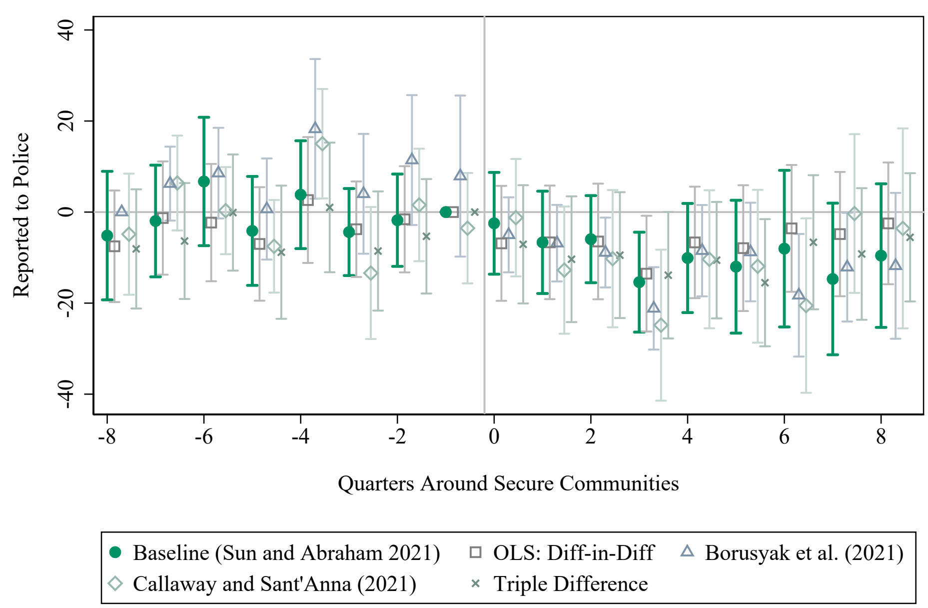
Note: This figure reports variants of β_{Post} from equation (1) for the non-Hispanic sample. The bars refer to the 95% confidence interval for the two-year post-period estimate of the Secure Communities (SC) program. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (3) include additional states or lower 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 OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period through June 2015 (full sample period). (7) re-estimates (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. (2024) 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. These estimates are also displayed in Table A.6.

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

(a) Victimized



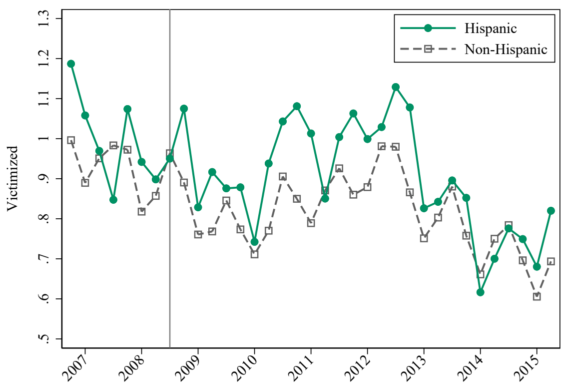
(b) Reported to Police



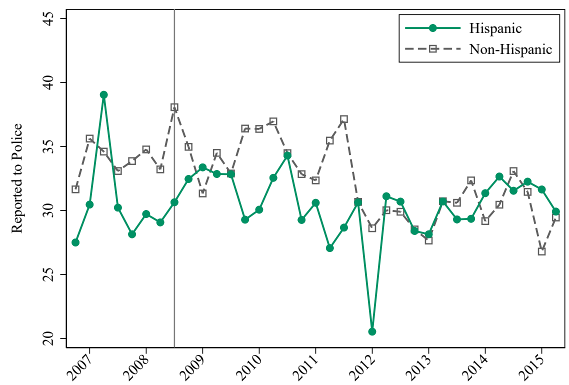
NOTE: This figure reports variants of β_{Post} from equation (2) for the Hispanic sample, with bars indicating 95% confidence intervals from standard errors clustered at the county level. The figures plot the baseline model estimates alongside the standard OLS difference-in-difference, a model following Borusyak et al. (2024), 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

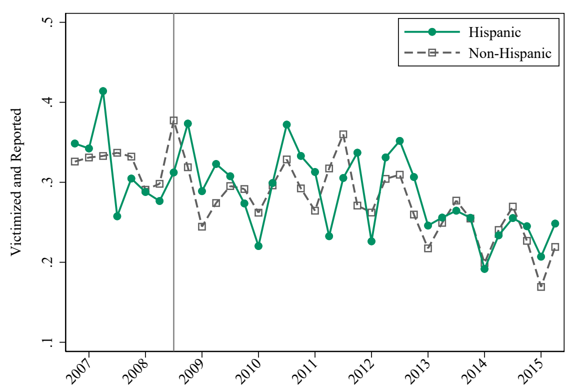
(a) Victimized



(b) Reported to Police

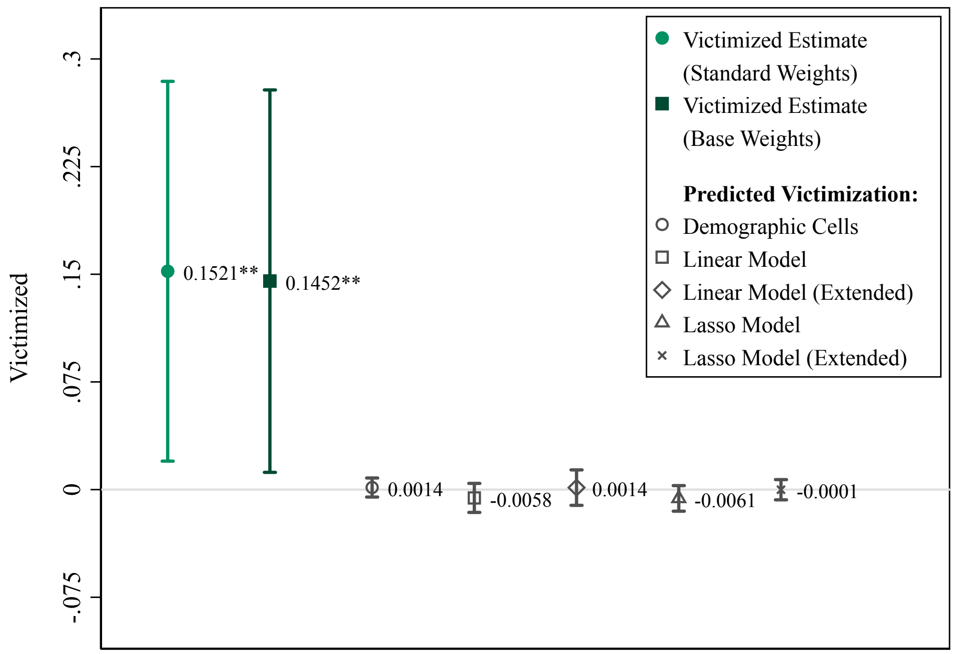


(c) Victimized and Reported to Police



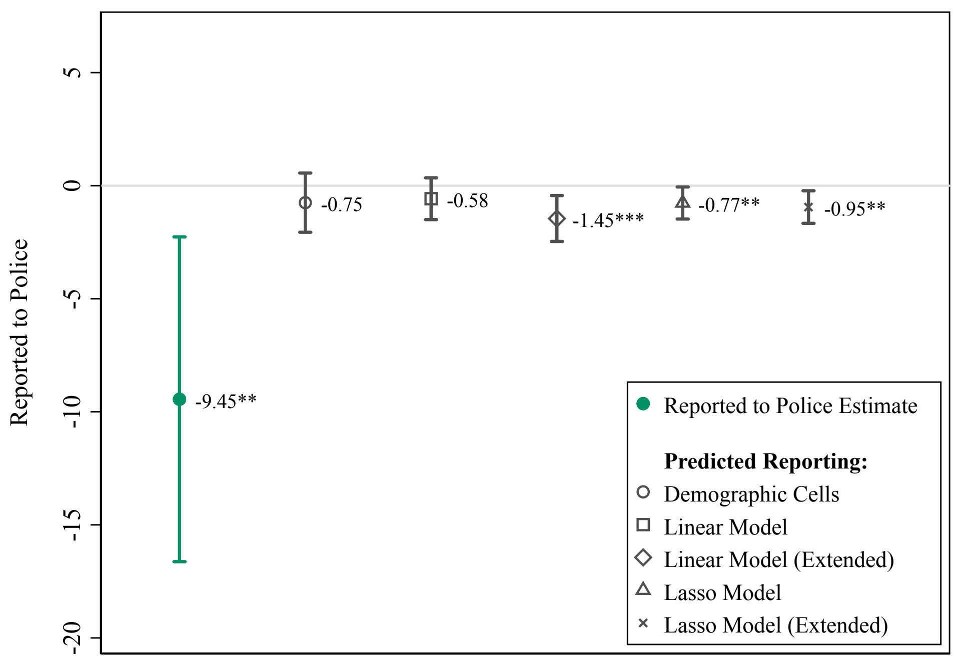
Note: These figures plot NCVS outcomes of whether a person is victimized, the share of crime incidents reported to police, and whether a person is both victimized and reported to police, separately for Hispanic and non-Hispanic respondents, between 2006 and 2015. Each outcome is multiplied by 100 for 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.

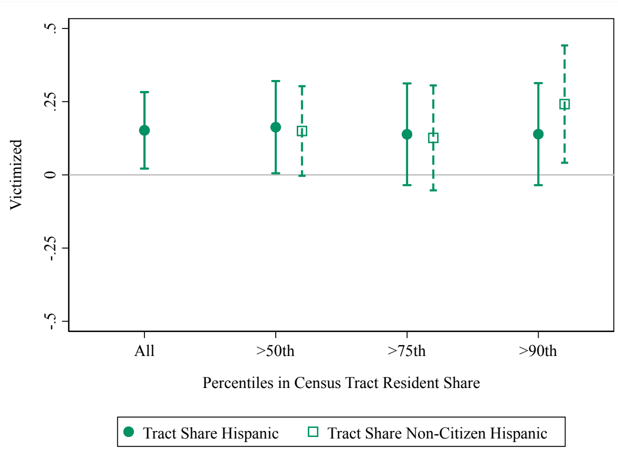
Figure A.12: Reported to Police Regression Estimate vs. *Predicted Reporting* given Observable Characteristics



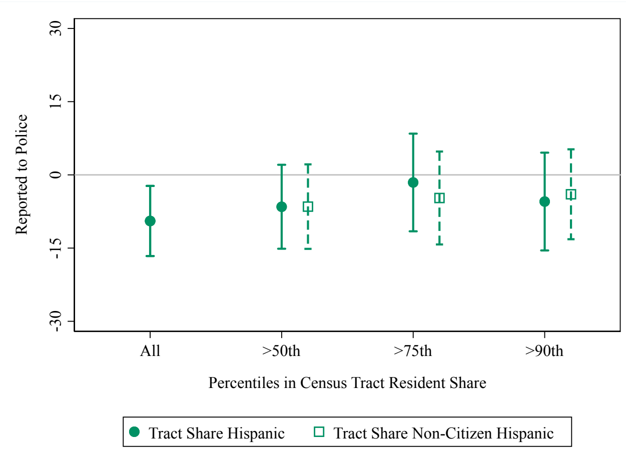
Note: *p<0.1, **p<0.05, ***p<0.01. The first estimate reproduces the baseline effect on the reporting behavior of Hispanic respondents following the implementation of Secure Communities (SC). The remaining estimates use pre-period data to generate various measures of predicted reporting based on victim 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

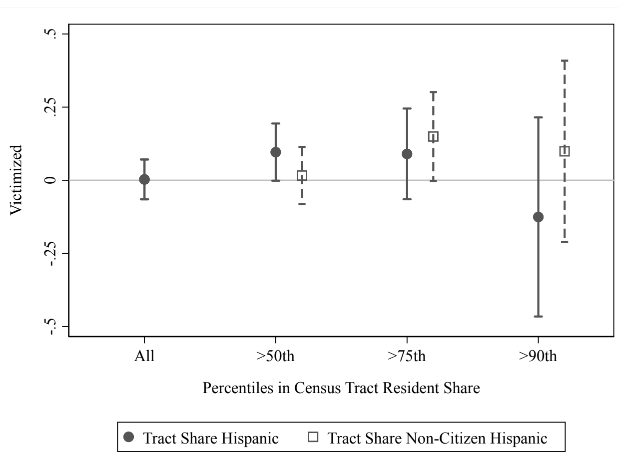
(a) Victimized, Hispanic



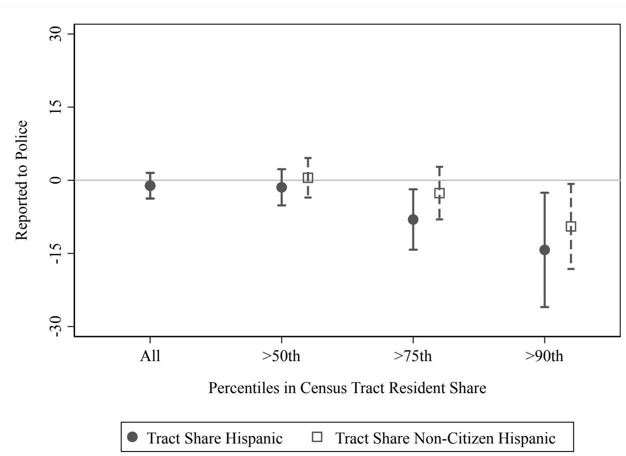
(b) Reported to Police, Hispanic



(c) Victimized, Non-Hispanic



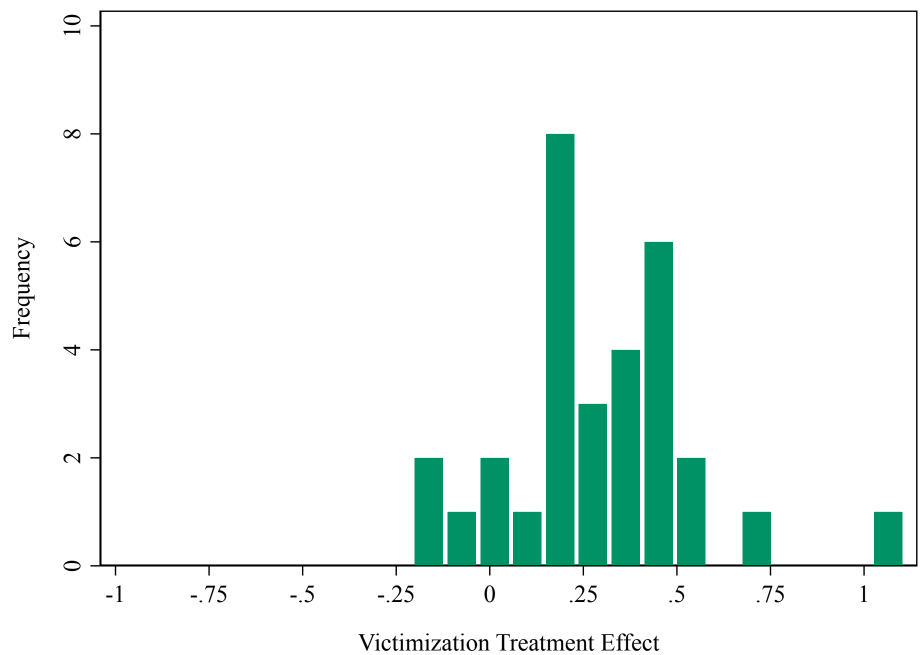
(d) Reported to Police, Non-Hispanic



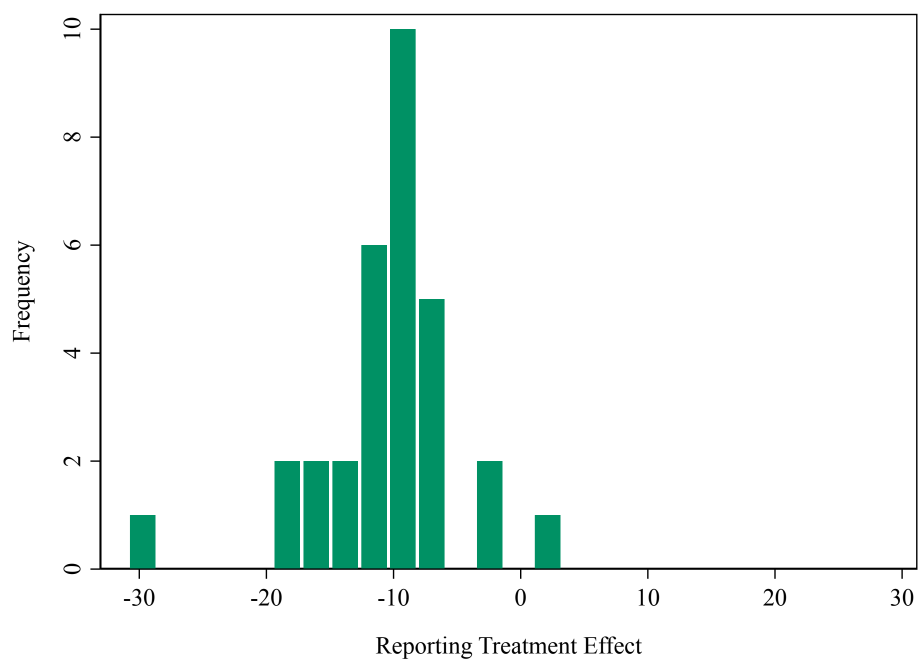
NOTE: This figure plots the estimates from equation (1) for subsamples of Hispanic and non-Hispanic survey respondents according to the share of their neighborhood that is Hispanic or non-citizen Hispanic. Data on neighborhood resident shares come from the 2000 Census and are linked to the NCVS based 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: Cohort-Level Effects of Secure Communities

(a) Victimized



(b) Reported to Police



Note: These figures plot the estimated distribution of Secure Communities program effects across cohorts of counties, according to the month of implementation. We estimate equation (1) separately for each activation cohort in the earlier-treated group and use the later-treated counties as the comparison group.

Figure A.15: Victimization Decomposition, Robustness to Alternative Elasticity Choices

Notes: This figure presents estimates from the decomposition outlined in Section 8.3 with varying assumptions about the impact of victim reporting on offender behavior. 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. "Residual" refers to the part of the total victimization effect that cannot be explained by the five mediators. This figure is analogous to Figure 6 but using different elasticities for the reporting/arrest mediator. For more details on these calculations, see Supplemental Appendix F.5.

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. (2022) 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 Enforcement Intensity (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. 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 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. "Violent crime" refers to rape and sexual assault, simple and aggravated assault, robbery, and verbal threats. "Property crime" refers to "burglary, theft, and larceny." "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 A.4: 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, and with local populations above 100,000 in 2000 using Manson et al. (2022). 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.5: 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 (all persons)	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
(14) Tracts >75th pct. Hispanic (all persons)	0.117*	(0.060)	1.06	-5.983**	(2.800)	33.45	0.007	(0.030)	0.37

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) include additional states or lower 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 OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period through June 2015 (full sample period). (7) re-estimates (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. (2024) 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 ("always-responders"). (13) re-estimates (5), including person fixed effects. (14) produces a version of (1) using all respondents (regardless of self-reported ethnicity) living in tracts above the 75th percentile of the tract-level distribution of the share of the population that is Hispanic. "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.6: 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) include additional states or lower 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 OLS two-way fixed effects using the baseline sample in (1). (6) expands the time period through June 2015 (full sample period). (7) re-estimates (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. (2024) 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.7: Effect of Secure Communities (SC) on Survey Response Rates and Self-Reported Ethnicity

	β_{Post}	(S.E.)	Y-Mean	Share Hispanic	Implied Hispanic Attrition	*Worst-Case* Victimization “Effect” (pp)	Percent of Estimated Effect
A. Survey Response Rate:							
(1) All Households	0.005	(0.004)	0.770	0.121	—	—	—
(2) Census Tracts >50th pct. Hispanic	-0.001	(0.006)	0.775	0.219	-0.005	0.006	4.0%
(3) Census Tracts >75th pct. Hispanic	-0.012	(0.008)	0.782	0.367	-0.033	0.040	26.1%
(4) Census Tracts >90th pct. Hispanic	-0.014	(0.012)	0.791	0.597	-0.024	0.028	18.3%
B. Identify as Hispanic:							
(5) All Respondents	-0.005*	(0.003)	0.154	—	—	0.030	19.5%

NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) reports difference-in-differences estimates of the effect of SC on household survey response rates. We use an analogous specification to equation (1) at the household level and with a binary variable indicating household response as the outcome variable. 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. 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). “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 panel (b), we estimate equation (1) using all respondents and use a binary variable indicating that the respondent self-identified as Hispanic as the outcome variable. In both panels, 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.” Standard errors are clustered at the county level. 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	Felony Share						
	(1)	(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.

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 8.3 (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 Supplemental Appendix F.

Table A.11: Effect of Secure Communities (SC) on 911 Calls and Arrests Using Police Administrative Data

	All Tracts (1)	Hispanic Tracts (2)	Non-Hispanic Tracts (3)
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

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (1). The two outcomes — total 911 calls and arrests — are measured using micro-data from police administrative records, as described in Section 8.2. Both outcomes are normalized using tract level populations from IPUMS (Manson et al., 2022). The unit of observation in each regression is a tract × year × month, and each regression includes city and time (year × month) 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.