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

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Abstract

We study the role of victim reporting in the production of public safety. We examine the Secure Communities program, a crime-reduction policy that involved police in detecting unauthorized immigrants and increased deportation fears in immigrant communities. We find that the policy reduced the likelihood that Hispanic victims report crimes to police and increased offending against Hispanics. The number of reported crimes is unchanged, masking these opposing effects. We show that reduced reporting drives the offending increase and provide the first elasticity of offending to victim reporting in the literature, calculating that a 10% decline in reporting increases offending by 7.9%.

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Criminal activity imposes high social and economic costs, making the advancement of public safety a principal objective of government. In accordance with the canonical model of the economics of crime, which posits that offenders respond to the expected cost of offending (Becker, 1968), scholars and policymakers regard criminal enforcement as a key tool for reducing crime. Enforcement is primarily carried out by government employees, but it also hinges on civilian participation: the police only learn about crimes when victims and witnesses report incidents. Without victims reporting crimes to the police, enforcement policy could become less effective, and public safety may worsen.

While victim reporting is a central input into law enforcement, the willingness to report crimes may itself be influenced by the broader enforcement environment. Residents may be less likely to engage with the police when they perceive enforcement to be disproportionate or unfair (Owens and Ba, 2021), and mistrust can be exacerbated by events like high-profile instances of police violence (Ang et al., 2023; Zaiour and Mikdash, 2023). Immigration enforcement, in particular, is often viewed as a policy area that carries significant risks for civilian cooperation. Though the goal of much of immigration enforcement policy is the advancement of public safety, a competing concern is that it induces fear within immigrant communities (Alsan and Yang, 2022). Residents may, as a result, become less willing to report crimes. Despite the theoretical significance of victim reporting, there is a dearth of empirical evidence examining the causal impact of victim behavior on public safety,[1] limiting our understanding of the tradeoff between enforcement and community engagement.

This paper studies the role of victim reporting in the production of public safety, and our central contribution is to provide the first estimate of the elasticity of offending with respect to victim reporting. We do so using a range of survey and administrative data combined with quasi-experimental variation in enforcement induced by U.S. immigration policy. Specifically, we study the Secure Communities (SC) program, a federal policy which significantly increased deportations of undocumented immigrants arrested for criminal offenses and whose stated goal was to reduce crime. During the program's implementation, law enforcement officials across the country expressed skepticism about the program's crime-reduction potential because of the possibility that it would deter victims from engaging with the police.[2] Guided by these competing predictions about the program's impact on public safety, our analysis proceeds by estimating three empirical objects: how victim reporting responded to changes in enforcement, how criminal offending responded to changes in enforcement, and

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

[2] See, e.g., William J. Bratton, "LAPD, not ICE," *Los Angeles Times*, 10/27/2009.

the causal relation between victim reporting and offending.

Studying the relationship between victim reporting and criminal offending requires overcoming two key empirical challenges. First, most administrative data only include crimes that have been reported to law enforcement, meaning that criminal offending and victim reporting are combined into a single measure of crime. This measurement issue implies that if a policy changes victim reporting, then examining its impact on *reported* crime will yield biased estimates of the true effect on offending. The first half of our paper tackles this central challenge. Second, policies or events that plausibly shift victim reporting are likely to have separate impacts on offending. This fact complicates the task of isolating the causal effect of victim reporting on offending behavior, and it is the focus of the second half of our paper.

We begin by estimating the impact of Secure Communities on victim reporting and offending. To overcome the aforementioned measurement challenge, we utilize 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.[3] The NCVS also provides information on respondent ethnicity, allowing us to separately estimate effects for Hispanics. This feature is a key advantage relative to most administrative crime data, which consist of counts of total reported crimes with no demographic information, and it is crucial in a setting where over 90% of deported individuals are Hispanic. We focus on effects for all 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 causal effects, we leverage the differential timing of the Secure Communities program's implementation, which was staggered across counties due to resource constraints. This program increased information sharing between county jails and federal immigration authorities to facilitate the identification and deportation of undocumented immigrants arrested for criminal offenses. 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 confirm that the program led to sharp increases in immigrant detentions and deportations: detentions by Immigration and Customs Enforcement (ICE) rose by 54% following the program's introduction.

[3] Because our primary measure of criminal behavior comes from a victim survey, we use the terms "victimization," "crime," and "offending" interchangeably throughout the paper.

Our first main finding is that Secure Communities reduced the likelihood that Hispanic victims reported crime to the police. Hispanics reduced 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 occurred relatively quickly and appears more pronounced among property offenses. In contrast, we find no changes in the reporting behavior of non-Hispanics. Because police rely on victim reporting to apprehend offenders, this decline in reporting implies a substantially lower likelihood that offenders are caught after committing a crime.

At the same time, the policy *increased* crime against Hispanics. Hispanic victimization (i.e., offending against Hispanics) increased by 0.15 percentage points, a 16% increase relative to the pre-period monthly victimization rate. These estimates imply that Secure Communities resulted in 1.3 million additional crimes against Hispanics in the two years following program activation. The increase in victimization mirrors the decline in reporting and appears to be largely concentrated among property crimes, which comprise the majority of victimizations. We also estimate similarly sized but less precise increases in violent crime.

As with reporting, 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), we rule out declines in victimization of more than 3.3%, indicating quite precisely that the policy did not achieve its goal of improving public safety through heightened enforcement. The findings of increased victimization and lower victim reporting are robust to accounting for survey attrition, ethnicity misreporting, and compositional changes among survey respondents and crime victims.

Our findings of increased victimization stand in stark contrast to prior studies analyzing Secure Communities, which concluded that the program had no impact on public safety (Miles and Cox, 2014; Treyger et al., 2014; Hines and Peri, 2019). To reconcile these opposing conclusions, we combine our primary outcomes to construct a measure of *reported* crime, analogous to standard measures of crime in the literature. We find that reported crime remains unchanged after the launch of the program, as it masks the opposing changes in victimization and reporting rates. Hence, our analysis not only overturns the prevailing consensus on Secure Communities' public safety effects, but it also explains why prior studies have systematically failed to detect its true impact. Our findings highlight the need to disentangle victimization from reporting behavior to accurately understand changes in public safety.

In the second half of the paper, we argue that the decline in victim reporting is the primary driver of increased victimization. As we highlight above, an empirical challenge to

isolating the effect of victim reporting on offending is that the Secure Communities program may have impacted offending through channels other than reporting. To address this challenge, we conduct four complementary exercises that draw on survey data, administrative crime records, and a novel collection of administrative micro-data from over 45 medium and large police departments. These micro-data were hand-collected through records requests to individual police departments, and each record is geocoded to a Census tract in order to link the outcomes to neighborhood resident demographics.

We first estimate victimization and reporting effects separately by cohort of program implementation using the NCVS sample. We show that cohorts with larger declines in reporting also experience larger increases in victimization, establishing a clear link between these two outcomes.

Second, we consider changes in police behavior in response to Secure Communities as an alternative driver of changes in offending. Although the program operated through the jail system — almost always managed by county sheriffs rather than municipal police departments — we examine the possibility that SC impacted police resources or behaviors. Using both the NCVS and the FBI's Uniform Crime Reports, we show that clearance rates, or the likelihood that a crime which is known to police is solved, remain unchanged after the program's implementation. We supplement this analysis with 911 call records from 16 cities in our novel data collection; using this data, we show that police response times did not change under SC. These results suggest that neither changes in police effectiveness nor police "pulling back" are responsible for the increase in crime.

Third, we examine how the policy affected the ethnic composition of *offenders*. If a deterioration in Hispanics' economic outcomes were the primary driver of the increase in crime, we would expect the arrestee population to become *more* Hispanic. We measure offender demographics using administrative arrest records from 37 cities in our data collection that include consistent data on Hispanic arrestee ethnicity. Our findings indicate that the policy actually *reduced* the Hispanic share of arrestees, providing evidence against economic distress as a driver of increased victimization.

Finally, we conduct a mediation analysis that explicitly accounts for the possible impacts of deportations and other social and economic changes as alternative drivers of the increase in offending. Specifically, we use data on immigration enforcement to estimate SC's impact on deportations and Census data to estimate the program's impact on unemployment, wages, and the share of female-headed households. Leveraging elasticities of crime with respect to these factors from the literature, we demonstrate that the increase in victimization cannot be jointly explained by these factors. In fact, the "residualized" change

in victimization after accounting for all measured mediators is *larger* than our baseline estimate, ruling out that changes in deportations and socioeconomic factors drove the observed increase in crime. As further evidence of the central importance of victim reporting, we estimate the predicted change in victimization from the observed decline in reporting, using the implied elasticity of offending to reporting from our cross-cohort analysis. We find that the predicted increase in victimization is strikingly similar to the "residualized" effect from our mediation analysis. Combining our findings from this exercise, our preferred estimate of the elasticity of offending to reporting is -0.79, meaning that a 10% decline in victim reporting increases offending by 7.9%. This sizable magnitude reinforces that community engagement is central to public safety. As reference, this figure is comparable to estimates of the elasticity of offending to *police force size* (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019), which is consistently cited as a key lever for improving public safety (Chalfin, 2025).

Our primary contribution is to show that victim reporting is fundamental for the production of public safety and to provide the first estimate of the elasticity of offending with respect to victim reporting. Criminal justice scholars have long studied the relationship between trust in law enforcement and willingness to call the police,[4] 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 could directly harm public safety, existing evidence on this relationship has been limited to qualitative ethnographies and cross-sectional correlations[5] or has focused on specific settings such as domestic violence (Miller and Segal, 2019; Golestani, 2021) and workplace mistreatment (Grittner and Johnson, 2022), in which the offender and victim have an established relationship. 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 changes in enforcement with little attention paid to victim behavior. 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.

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.

[4] 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.

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

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.[6] 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 findings of this study also have implications for the broader design of criminal justice policy. Scholars, law enforcement officials, and activists have warned that various policing practices — such as zero-tolerance policing, "stop-and-frisk" policies, mandatory arrest laws for domestic violence, no-knock warrants, and physical restraint techniques like chokeholds — may reduce victim reporting, compromise police legitimacy, and damage police-community relations (e.g., Fagan and Davies, 2000; Fratello et al., 2013; Iyengar, 2009; Reisig and Kane, 2014; U.S. Department of Justice, 2021). Building public trust has thus emerged as a key area for reform (Ramsey and Robinson, 2015). Our findings expand this debate by showing how certain policies may not only damage victim reporting, but also, as a result, may worsen public safety. Stated differently, we provide direct evidence of a tradeoff between enforcement and community engagement in the provision of public safety.

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 and maps our program estimates to our target elasticity. 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 mechanisms underlying the rise in crime, concludes that the reporting decline is the primary cause, and reports our estimate of the elasticity of offending to reporting.

[6] 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 program, an information-sharing initiative intended to promote public safety.[7] 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),[8] 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).[9,10]

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 re-

[7] 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).

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

[9] 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).

[10] 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.

source 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.[11] These concerns are consistent 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

[11] 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.

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).[12] 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.[13,14]

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).

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 Appendix B, we provide more details on this framework and discuss various extensions.

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

[12] 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.

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

[14] 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).

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 is drawn from a cumulative distribution function $G(c)$ 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 arrest 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: $M - rax - rap_DD - c > 0$. The number of offenses is thus $O = G(M - rax - rap_DD)$.

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 drawn from the cumulative distribution function $F(h)$ across victims. There is an additional cost of reporting the incident related to immigration enforcement: this cost is a function of the probability that an individual is referred to immigration officials δp_D and the cost of deportation D.[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:

$$\frac{\partial r}{\partial p_D} < 0$$

[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).

[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.

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, and is an empirical question. 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 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.

Our primary interest is in estimating how offending responds *directly* to a change in victim reporting, summarized by the elasticity $\epsilon_{O,r} = \frac{\partial O}{\partial r} \times \frac{r}{O}$. As we show in Appendix B, we can calculate an upper bound for this (negative) expression using the ratio of the program's impacts on offending and victim reporting:

$$\frac{dO/O}{dp_D} \Bigg/ \frac{dr/r}{dp_D} = \underbrace{\epsilon_{O,r}}_{\leq 0} + \underbrace{\frac{\epsilon_{O,p_D}}{\epsilon_{r,p_D}}}_{\geq 0}, \tag{1}$$

where $\epsilon_{x,y}$ generically reflects the partial derivative elasticity of x with respect to y. Because the direct effect of the change in deportation probability on offending is negative (given deterrence), as is the direct effect on victim reporting, the second term in the expression above is positive. Hence, our empirical estimate of this ratio is a biased-upward estimate of $\epsilon_{O,r}$.[17]

With this conceptual framework in mind, we now turn to estimating the program's impacts on victimization and victim reporting using the National Crime Victimization Survey, which we now discuss in greater detail.

[17] In Appendix B, we also consider an extension where we allow the distribution of costs to offending $G(c)$ to respond to the policy. In this case, the ratio of the policy's impacts is no longer necessarily an upper bound on our estimand of interest, since the policy may directly induce more offending if the costs to offending decline (through, e.g., worsening of labor market opportunities). We provide evidence in Section 8 against the importance of changing economic conditions as a channel for the program's effects.

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 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.[18]

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 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

[18] ICE detainer requests do not necessarily lead to a deportation. Unfortunately, information from TRAC on detentions cannot be linked to the removals data. Nevertheless, consistent with prior research (Alsan and Yang, 2022; Medina-Cortina, 2022), Figure A.5 shows a strong, positive correlation (0.86) between county-level SC removals and honored detainers in the post-SC period.

implementation, separately for Hispanic and non-Hispanic individuals. Panel (a) of Figure 1 shows that the reporting rate of Hispanic respondents declines following the launch of the program. At the same time, panel (a) of Figure 2 shows that the victimization rate of Hispanic respondents rises after the program implementation. Non-Hispanic individuals do not appear to have any post-treatment change in either outcome. Finally, panel (a) of Figure 3 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:

$$Y_{ict} = \beta_{\text{Post}} \text{SC}_{ct} + \mu_c + \delta_t + \epsilon_{ict} \tag{2}$$

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.[19]

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

$$Y_{ict} = \sum_{\tau=-8}^{\tau=7} \beta_\tau \times \text{SC}_{ct}^{\tau} + \mu_c + \delta_t + \epsilon_{ict} \tag{3}$$

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.[20] We omit the quarter before the program introduction, so that each β_τ coefficient measures the difference in outcome Y

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

[20] 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.

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-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 (3). 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 Enforcement: Immigrant Detentions

We first establish that the Secure Communities program did in fact increase enforcement intensity. Panel (a) of Figure A.7 plots the average logged number of honored immigration detainer requests around the implementation of Secure Communities. Panel (b) then estimates equation (3) 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 (2) 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 Willingness to Report Crimes to Police

Having established that SC increased immigration enforcement, we now study the likelihood that a crime victim reports their incident to the police. Table 2 displays the estimates of equation (2) for both Hispanic and non-Hispanic respondents, and panel (b) of Figure 1 plots the dynamic event-study estimates. Recall that the unit of observation is a criminal incident, so the sample is restricted to individuals who were victimized (i.e., a significantly smaller sample). These results show that Hispanic individuals in treated counties had comparable trends in reporting behavior prior to SC's implementation relative

to Hispanics in control counties, thereby providing support for the parallel trends assumption. 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. In contrast, we find that SC had no impact on non-Hispanic reporting.

Victim reporting directly impacts the likelihood of offender apprehension. Put differently, the total likelihood of apprehension for offenders is the product of the probability that a victim reports the incident r and the probability that an arrest is made for a reported offense a. In the NCVS, the probability of an arrest among reported offenses is 9%. Our estimates thus imply that for offenders targeting Hispanic victims, the likelihood of apprehension, ra, decreases from 3% to 2.1% after Secure Communities due to changes in reporting.[21]

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).

5.3 Victimization

We now turn to our main outcome of interest: changes in public safety. Panel (b) of Figure 2 plots the dynamic event-study estimates for criminal victimizations. 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. 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. Conversely, 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

[21] In Appendix D, we provide a discussion and analysis of the apprehension risk. Our point estimates indicate that the percent decline in the likelihood of apprehension is similar in magnitude to the decline in reporting, suggesting that reporting is the primary driver of overall changes in apprehension risk.

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.[22]

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 emphasize 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.

5.4 *Reported* Crime Rates

Our main finding — that the Secure Communities program increased crime against Hispanics — stands in stark contrast to a substantial body of literature analyzing the program using data on crimes *reported to the police* (Miles and Cox, 2014; Treyger et al., 2014; Hines and Peri, 2019). To reconcile this discrepancy, we combine our two primary outcomes and estimate the program's impact on the likelihood of being victimized *and* reporting a crime.

Figure 3 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 arises because the reported crime rate masks two opposing effects of Secure Communities: an increase in victimization and a decrease in reporting. These null effects on the rate of reported victimization mirror previous findings that concluded SC had no meaningful impact on public safety.[23] However, our results underscore that mis-measuring crime — by relying on data of *reported* crime — can lead to incorrect conclusions about the program's true impact on offending.

In sum, our findings overturn the prevailing consensus on the impact of Secure Com-

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

[23] 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).

munities, while providing a rationale for why the existing literature has failed to detect a crime-related effect from this policy. More broadly, these results highlight the importance of accounting for reporting behavior when estimating changes in public safety, particularly when studying policies that may influence both an offender's incentives to commit crimes and a victim's incentives to report them.

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 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.[24]

6 Robustness

6.1 Sample Construction and Empirical Specification

We consider here the robustness of the main results: the decline in reporting and the increase in victimization 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.8. 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.[25] The results are comparable, although

[24] 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.

[25] 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.

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 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)).[26] 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)).[27] Across these strategies, we continue to find a significant decline in reporting and increase in victimization among Hispanic respondents. Figure A.10 displays the event-study coefficients from these alternative strategies, showing that the dynamic evolution of outcomes is comparable across these specifications.

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

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

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 reporting a crime to the police or on the likelihood of being victimized.

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.11 shows the main outcomes by calendar time, separately by Hispanic ethnicity, and we denote the first activation month in 2008 with a vertical line. We do not see any evidence of a change in outcomes for Hispanics around the first activation date relative to non-Hispanics. There is, however, evidence that Hispanic victimization increases relative to non-Hispanic victimization beginning in 2010 through 2012, consistent with our main estimates. These plots provide reassurance that 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 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 (2), 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.[28]

Despite the reassuring evidence of minimal attrition, we use the point estimates from these regressions to conduct a back-of-the-envelope calculation of how much bias could be induced from changes in response rates, similar in spirit to Lee (2009). We conduct these calculations in rows (2)–(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.8).[29] 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 not driven by sample attrition.

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

[29] 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.

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.8). 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 (2) 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.[30]

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

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

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 (2) using predicted victimization as the dependent variable. Figure A.12 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.[31]

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.[32] To test for this concern, we conduct an analogous exercise to the one above, by constructing *predicted reporting* measures using pre-SC data for crime incidents. The results are presented in Figure A.13, 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.

[31] Standard NCVS weights include a survey attrition adjustment; Figure A.12 also uses alternative weights that do not adjust for attrition (results are nearly identical).

[32] 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.

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 (2) 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.14 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 reporting crimes to police decreases and the likelihood of being victimized generally increases as neighborhoods become more Hispanic (or non-citizen Hispanic). The reporting effects suggest that non-Hispanic victims in Hispanic neighborhoods may decrease their willingness to report crimes, potentially due to concerns about the rising threat of deportation for their neighbors. Further, the victimization effects are consistent with offenders targeting Hispanic neighborhoods after the policy — potentially because the apprehension risk has declined in these places given the reduction in crime reporting — thereby increasing victimizations of non-Hispanics in these areas.

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.9). 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 (2) 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.15 displays the distribution of treatment effects. Among 31 cohorts, 30 have negative reporting treatment effects and 28 have positive victimization treatment effects. The distribution of reporting effects is relatively narrow, and clustered around a 10 p.p. decline. These patterns imply that the main results are not driven by a few select counties or cohorts, but rather that Hispanics across areas are exhibiting declines in reporting and experiencing increases in victimization.[33]

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 (2), 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.[34] 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.15).

8 Reduced Reporting as the Key Driver of Higher 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

[33] In Appendix E, 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.

[34] 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 (or increase less) when serious offenders are targeted.

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 an increase in deportation risk for immigrant arrestees, as well as 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 provide several pieces of evidence pointing to the reporting decline as the main driver of increased victimization. We conclude by deriving and discussing our estimate of the elasticity of offending to victim reporting.

8.1 Relationship between Victimization and Reporting Effects

We begin by showing that cohorts with larger reporting declines experienced larger victimization increases, using the cohort-level treatment effects discussed in Section 7.2. Figure 4 shows a pronounced negative relationship between these outcomes, and 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: that the victimization and reporting effects are estimated in the same sample, which could induce a spurious correlation between the effects, and that 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 4. 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 Changes in Police Behavior

A competing hypothesis for the increase in victimization is that Secure Communities influenced police behavior in ways that contributed to higher crime rates. Recall that the program primarily operated through fingerprinting in county jails, which are almost always managed by county sheriffs rather than municipal police departments. Nevertheless, if police effectiveness declined or if officers reduced their patrols in response to the program, these

shifts could have led to an increase in crime rates independent of the decline in reporting. In this subsection, we examine this possibility using a range of data sources.

We start by examining clearance rates, which reflect the likelihood that a reported crime is solved by police (typically resulting in an arrest) and thus serve as a proxy for the arrest rate of reported crimes, a. Police effectiveness in solving crimes could decline with SC if, for example, witness cooperation decreases or officers become more detached from Hispanic neighborhoods. Conversely, police effectiveness could improve if officers have more time and resources to focus on crime-solving due to a potential reduction in the number of reported crimes requiring investigation.

We investigate clearance rates in two ways in Table A.9. First, we use the NCVS to calculate the share of crime incidents reported to police that result in an arrest. We find no significant change in the likelihood of arrest for reported incidents. The point estimate for Hispanic victims implies a 22% reduction; however, this estimate is quite noisy and imprecise (p-value = 0.48). This finding is important because it implies that police effectiveness did not decline for cases with Hispanic victims or respond differentially by victim ethnicity. Second, we use UCR data to examine whether SC affected clearance rates for serious crimes. Again, we find no evidence that SC changed police crime-solving effectiveness.

To further explore this possibility, we manually collected and harmonized administrative data of 911 call records from municipal police departments across the country for the period 2006–2011; we provide information on these data and our data cleaning and sample selection choices in Appendix F. For the subset of cities for which we observe time stamps in the 911 call data, we calculate the log of the average time (in seconds) for a call to be assigned to a police officer. This measure serves as a proxy for the efficiency of police responses and the availability of patrol officers to be assigned to calls. Importantly, we geocoded all records in this data, allowing us to examine changes in police response in Census tracts with differing Hispanic resident populations. We designate tracts as "Hispanic tracts" if the share of the population that is Hispanic is above the 90th percentile of the tract-level Hispanic share distribution. Using this data, we estimate a version of equation (2) at the Census tract $\times$ year-month level. We find no changes in police response times, overall or in Hispanic neighborhoods, suggesting that patrol patterns and officer availability remain largely unchanged after the policy.

Taken together, these findings provide no evidence that police are pulling back or becoming less effective in response to SC, thus ruling out changes in police behavior as a

main driver of the observed increase in victimization.[35]

8.3 Economic Distress: 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 changes in economic conditions. Specifically, 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.

To consider changes in the offender population, we augment the analysis using our 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.[36] Indeed, lack of information on offender characteristics is a pervasive challenge in the economics of crime literature (Doleac, 2023).

We circumvent this challenge by collecting micro-data on arrest records from municipal police departments for 2006–2011, data which is similar to the 911 call data used above to measure police response times. Each arrest observation records the date and time of the arrest as well as basic demographic information on the arrested individual. Crucially for this analysis, we observe Hispanic arrestee ethnicity in 37 cities, as well as the location of each incident, allowing us to investigate program impacts on offender composition separately by neighborhood type.

Our goal is to understand the impact of SC on the ethnic composition of arrestees. We therefore use equation (2) to measure changes in the overall arrest rate as well as the Hispanic

[35] Our results also point against racial animus as an alternative channel for the victimization increase. Chiefly, we find an increase in *non-Hispanic* victimization in counties and neighborhoods with larger Hispanic populations (Table A.6, Figures A.9 and A.14), suggesting that the increase in offending is not strictly determined by ethnicity, and instead could be related to location-level changes in the likelihood of offender apprehension. Further, we do not find evidence of larger increases in Hispanic victimization in counties with a higher Republican vote share (Table A.8), where anti-immigrant sentiments are likely more common (see, for example, Afrouzi et al. (2024) and "Americans Still Value Immigration, but Have Concerns," *Gallup*, 7/13/2023.)

[36] 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.

and non-Hispanic arrest rates. In these regressions, the unit of observation is a Census tract in a given year-month. Because we do not observe victim ethnicity in these data, we again split our analysis by neighborhood ethnic composition. Examining effects *within* ethnic neighborhoods allows us to investigate changes in arrestee composition, holding fixed the characteristics of potential victims.

The top panel of Table 3 presents changes in per capita arrest rates for the full sample as well as separately for Hispanic and non-Hispanic tracts. Panel (b) then uses these estimates to calculate the change in the share of arrested individuals that are Hispanic. The outcome mean shows that the Hispanic share of arrestees is lower than the overall tract composition, perhaps due to immigrants' lower criminality relative to the US-born (Abramitzky et al., 2024). Across all tract types, the Hispanic share declines after SC. In Hispanic neighborhoods, there is a 2.3 p.p decline off a base of 47%, or a 5% decline.

In sum, these results indicate that the composition of offenders changed after the launch of SC to become *less* Hispanic. These findings are inconsistent with changes in economic conditions driving the increase in crime.

8.4 Deportations and Other Economic/Social Changes

Next, we use a decomposition to account for SC's impacts on deportations and on other economic and social outcomes. To do so, we estimate SC's impact on these outcomes and 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? We summarize the approach and results here, and point the reader to Appendix G for more detail.

Framework — We consider a set of outcomes that, while parsimonious, capture the fact that Secure Communities impacted both social and economic outcomes in Hispanic communities. Specifically, we consider deportations stemming from increased enforcement; the employment-to-population ratio and logged hourly wage of low-educated foreign-born Hispanics (following East et al., 2023); and the share of Hispanic household heads that are female. 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:

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

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 (4). For the effect of SC on deportations, we use our estimate of the increase in honored ICE detainers (or transfers to ICE custody), which is shown in Table A.2. For the remaining mediators, we estimate SC impacts using equation (2) 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 and wages 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 Johnson and Raphael (2012), Gould et al. (2002), and Glaeser and Sacerdote (1999) 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, shown in Figure 5 and the final column of Table A.10, offer three main takeaways. First, the negative bar corresponding to deportations suggests that the removal of individuals would be expected to reduce crime. Indeed, 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 and incapacitate offenders.[37] Second, although Secure Communities impacted

[37] To estimate the effect of deportations on crime, we use Johnson and Raphael's (2012) estimate of

other labor market and demographic outcomes that are related to crime, their predicted effect on victimization — based on elasticities from prior work — is negligible. Put differently, the elasticity of crime with respect to these mediators would need to be significantly larger than the elasticities from prior work in order for these mediators to account for the increase in victimization. We thus conclude that demographic and economic responses to SC, while important outcomes in their own right, are not the primary drivers of the observed increase in Hispanic victimization.

Third, the decomposition reveals a notable positive "residual" effect. Since we have accounted for SC's impact on deportation, demographic, and economic responses, we interpret this positive residual as capturing the expected increase in victimization due to the decline in reporting. As further validation, the final bar of the figure then compares the residual effect to the change in victimization we would have predicted based on the observed decline in reporting (Table 2) and the implied elasticity of offending to reporting from the cross-cohort analysis (Figure 4). The similarity in magnitudes between our baseline estimate of increased victimization (0.152), the mediator-adjusted residual effect (0.203), and the implied increase from the reporting decline (0.263) all indicate that the decline in victim reporting is the primary driver of the increase in victimization.

Finally, in Appendix Figure A.16 we compare our predicted effect on victimization to what would be expected using estimates from the literature. Note that this comparison is imperfect, as existing estimates pertain to specific offender populations (e.g., perpetrators of domestic violence). Nonetheless, it underscores that, if anything, our observed decline in reporting, combined with prior estimates, would have predicted an even larger effect on victimization.

8.5 Elasticity of Offending to Victim Reporting

Using the findings from this section, we can now calculate the implied elasticity of offending to reporting in three ways. First, assuming that victimization increased *solely* because of reduced reporting, our baseline estimates imply an elasticity of -0.59. Second, if we use the mediation estimates to "residualize" the victimization effect of the components explained by deportation and social/economic changes, we estimate an elasticity of -0.79.[38]

the crime impact of incarceration. Their estimand of interest is the treatment effect of changing the stock of prisoners, which jointly captures the incapacitation and deterrence impacts of incarceration. Their estimate is thus particularly suitable for our setting, where SC incapacitated offenders through deportation and possibly deterred potential offenders.

[38] 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 four measured mediators.

Third, we can use our cohort-level estimates from Section 8.1 and conclude that the elasticity is -1.02.[39] Since deportations are expected to reduce crime, it is unsurprising that our first elasticity estimate, which does not account for the program's deportation impact, is less negative than the residualized elasticity and the cohort-level elasticity. This finding accords with equation (1), which shows that an elasticity constructed from our baseline estimates provides an upper bound on the (negative) elasticity of offending to victim reporting.

Overall, our findings suggest that changes in victim reporting can substantially impact public safety. We estimate the elasticity of offending to victim reporting to be similar in magnitude to the elasticity of offending to police force size (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019), a commonly cited policy tool for crime reduction (Chalfin, 2025).

9 Conclusion

Our study provides evidence that reduced crime reporting can negatively impact public safety. To examine the relationship between victim reporting and crime, we study an increase in immigration enforcement through the implementation of the Secure Communities program between 2008 and 2013. We observe a significant reduction in the crime reporting rate of Hispanic victims, consistent with heightened fear of interacting with law enforcement. We also find that Hispanic residents are significantly more likely to become victims of crime, a result that sharply contrasts with the program's crime-reduction objectives. We argue that the decline in reporting is the primary driver of the increased victimization of Hispanic individuals.

Unlike prior studies, we conclude that Secure Communities worsened public safety. To reconcile our findings with previous work, we show that reported crime rates — the standard measure of crime in the literature — remains constant because the decline in victim reporting obscures the true increase in victimization. These findings overturn the existing empirical work on the Secure Communities program and offer an explanation for why previous studies have consistently found null effects. More broadly, these findings demonstrate that the tracking and measurement of public safety outcomes can be distorted by changes in victim reporting.

The divergence between the public safety goals of the Secure Communities program

[39] These elasticities each rely on different assumptions. The first elasticity assumes that SC impacted victimization solely through victim reporting. The second elasticity assumes that our measured mediators account for all non-reporting impacts of SC on victimization. The third elasticity assumes that cohort-level program impacts on victim reporting are uncorrelated with cohort-level program impacts on other channels that may impact victimization.

and its actual effects has significant policy implications. The public debate surrounding immigration policy often centers on concerns related to immigrant criminality, and views about immigration policy can have wide-ranging political consequences (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 targeting immigrant offenders are frequently promoted as a means to reduce crime rates. Our findings indicate that these policies may be ineffective, if not counterproductive, due to their potential impacts on community engagement with the police.

Our results also underscore the importance of collecting and utilizing data sources with granular demographic information to obtain a more comprehensive understanding of the impact of policies targeting certain subgroups. The analysis shows that estimating the impact of Secure Communities on the full population masks its effects on Hispanic individuals, a group that constitutes 15% of the U.S. population.

The effectiveness of government, including law enforcement, can be undermined when policies erode 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 induce fear among victims and witnesses. Confidence in law enforcement in the U.S. is particularly low in low-income communities (Frydl and Skogan, 2004) and has decreased in recent years (Kennedy et al., 2022). Future work should explore ways to improve trust and foster greater community engagement.

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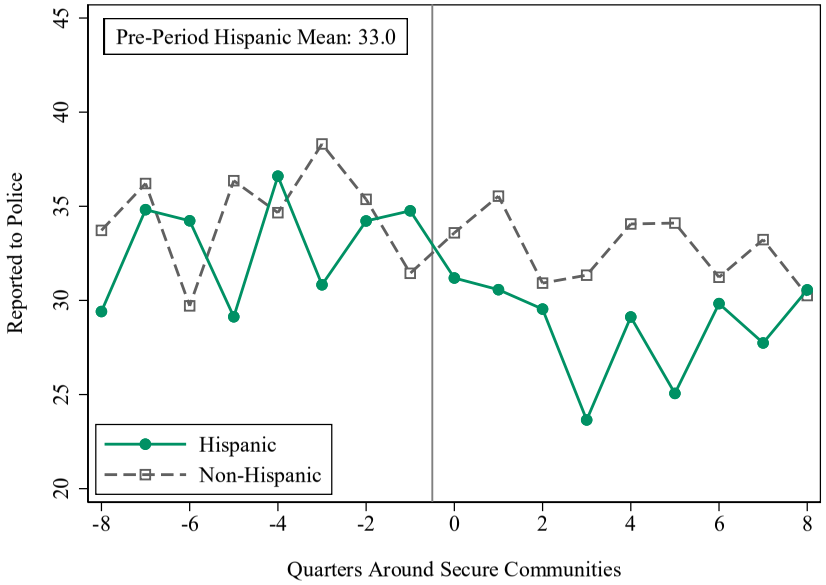
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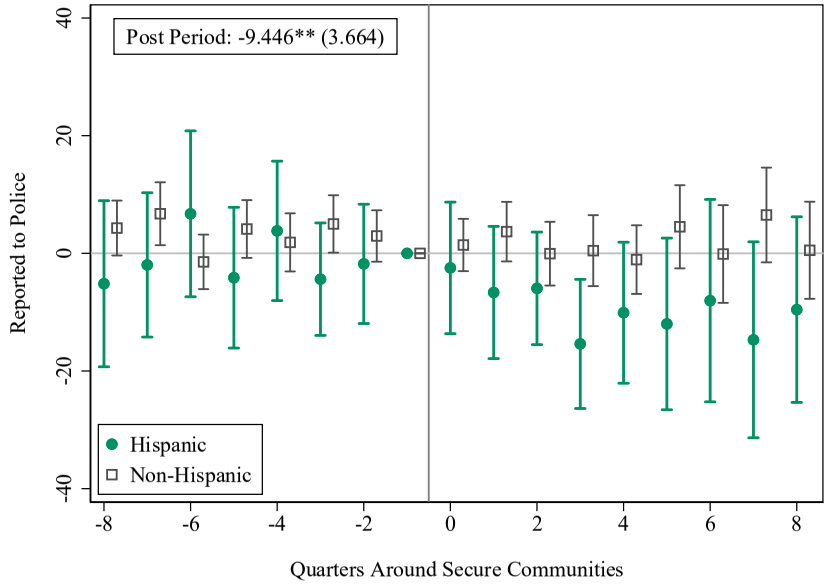
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Figure 1: Share of Crimes Reported to Police around Secure Communities (SC), by Hispanic Ethnicity

(a) Raw Data



(b) Regression Estimates

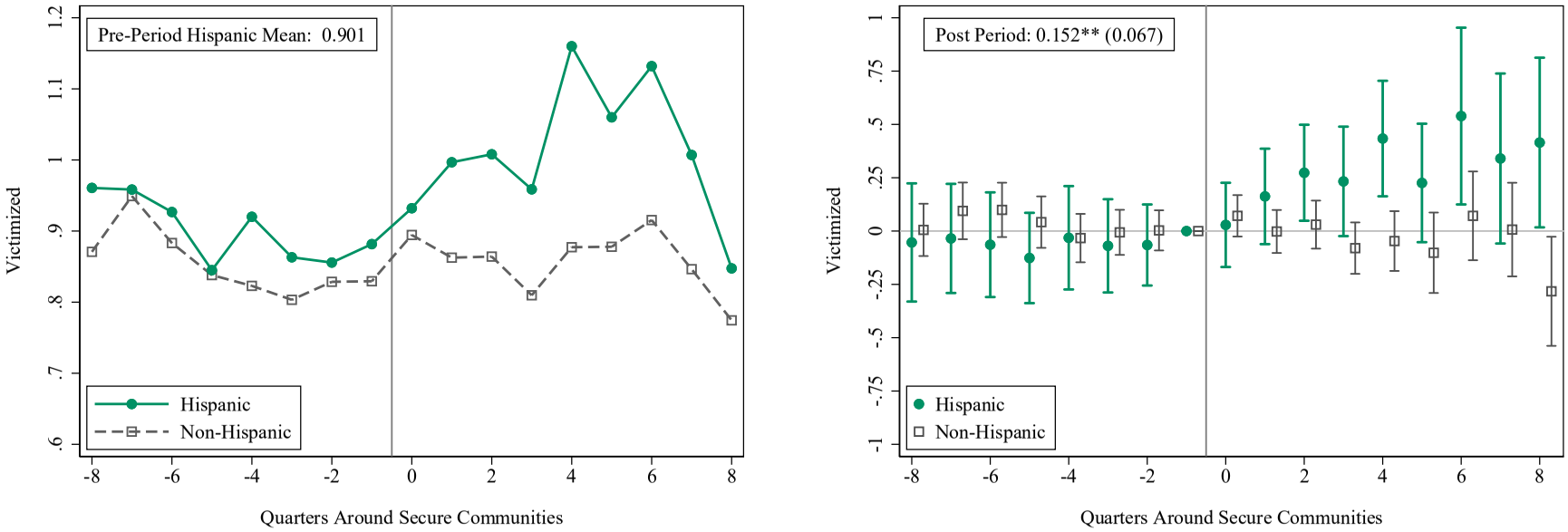


NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS crime incidents for which a victim reported the crime to police for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 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 (3) and reports the β_{Post} estimate and corresponding standard error from equation (2). 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 2: Share of Persons Victimized around Secure Communities (SC), by Hispanic Ethnicity

(a) Raw Data

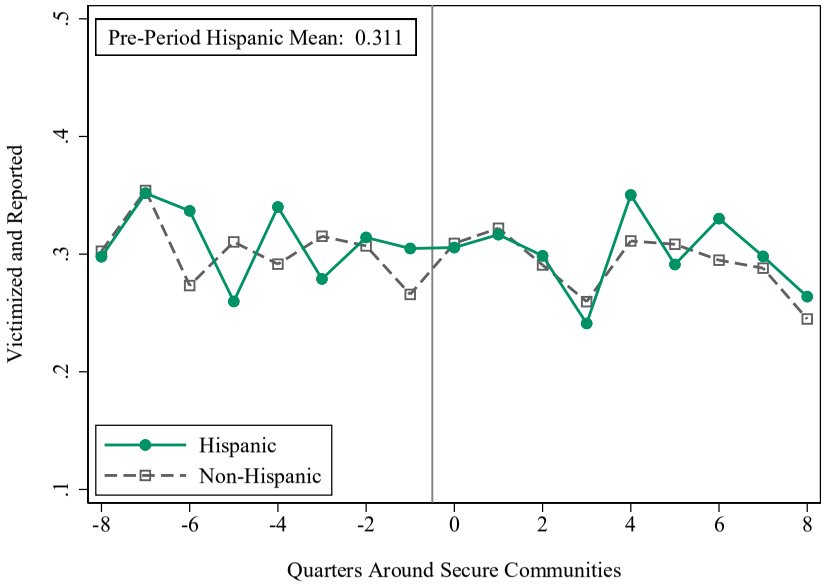
(b) Regression Estimates



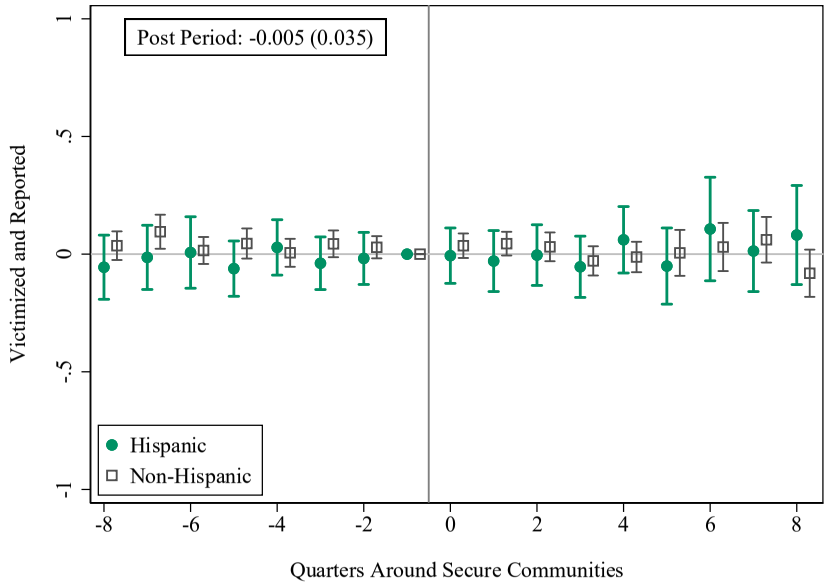
NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS respondents victimized for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (scale 0 to 100). The sample of counties follows the NCVS sample restrictions described in Section 3. Panel (b) plots the dynamic difference-in-differences estimates using equation (3) and reports the β_{Post} estimate and corresponding standard error from equation (2). 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 Persons Victimized who Reported Crimes to Police (*Reported* Crime Rate) around Secure Communities (SC), by Hispanic Ethnicity

(a) Raw Data

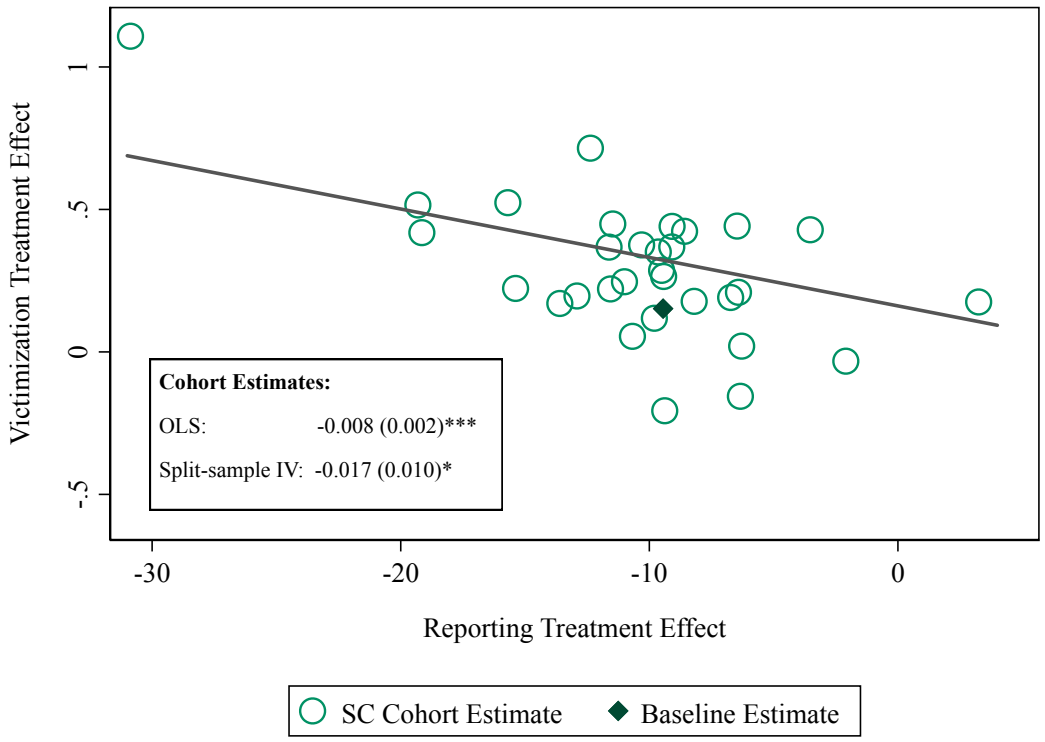


(b) Regression Estimates



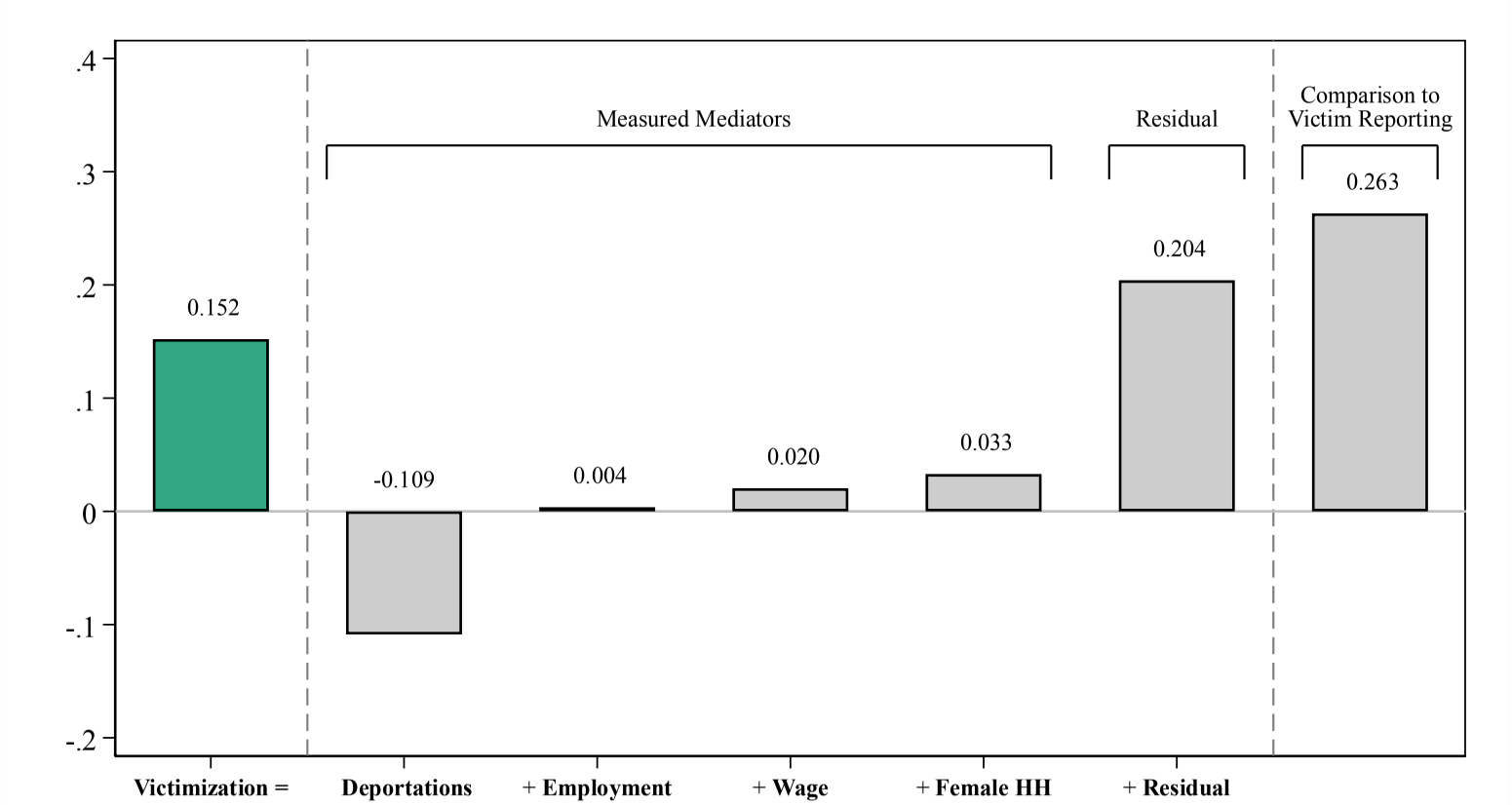
NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the share of NCVS respondents who were both victimized and reported the crime to police for the eight quarters before and after the implementation of the Secure Communities program. The outcome is multiplied by 100 for ease of exposition (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 (3) and reports the β_{Post} estimate and corresponding standard error from equation (2). 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 4: 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 (2) 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 5: Decomposing Victimization Increase into Various Components



Notes: This figure presents estimates from the decomposition outlined in Section 8.4 and 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 effect of deportations on victimization uses estimates from Table A.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 four mediators. The final bar depicts the implied effect of reporting on victimization using our reporting effect from the NCVS (Table 2) and our cohort-level victimization-to-reporting relationship (Figure 4). For more details on these calculations, see Appendix G.

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

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

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

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

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

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (2). 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 Arrest Composition, Using Police Administrative Data

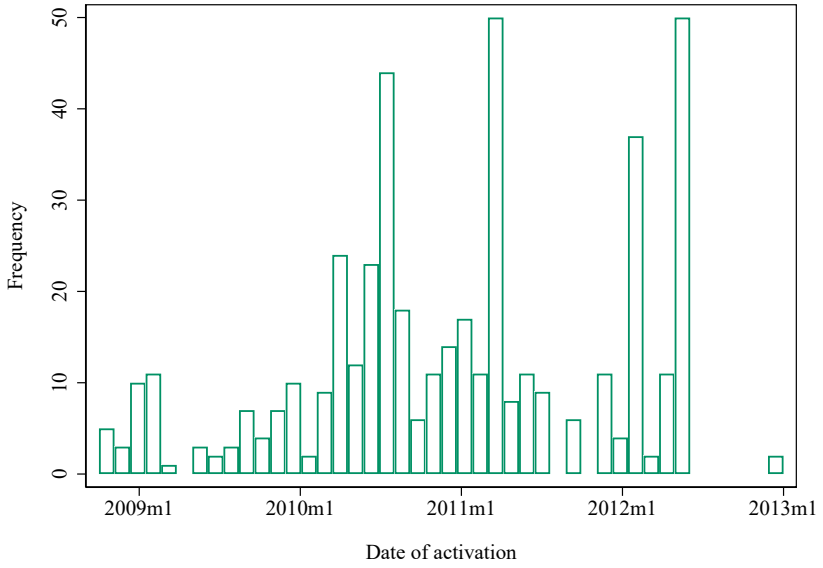
	All tracts			Hispanic tracts			Non-Hispanic tracts		
	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$	β_{Post}	(S.E.)	$\bar{Y}$
A. Arrests Per Capita:									
Hispanic	-0.033**	(0.016)	0.59	-0.088	(0.072)	1.37	-0.010	(0.009)	0.34
Non-Hispanic	0.020	(0.098)	2.67	0.018	(0.124)	1.85	0.016	(0.099)	2.93
Total	-0.013	(0.108)	3.26	-0.070	(0.188)	3.22	0.006	(0.106)	3.27
B. Implied Arrest Share:									
Hispanic Share	-0.013*	(0.007)	0.208	-0.023*	(0.013)	0.473	-0.005	(0.003)	0.123
Observations		187,738			49,914			137,824	
Number of Cities		37			20			37	
Tract Share Hispanic		0.30			0.74			0.15	

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (2) and micro-data from police administrative records, as described in Section 8.3. In panel (a), the outcome is arrests per 1,000 population. In panel (b), we use the estimates from panel (a) to calculate the change in the share of arrested individuals that are Hispanic. Standard errors are calculated using the Delta Method. The unit of observation is at the tract-year-month level, and regressions include city×county and year×month fixed effects. Tracts are weighted by their 2005–2009 population (Manson et al., 2022). In all regressions, the sample is restricted to meet the sampling criteria described in Section 3. The regressions use later-treated counties as the control group for estimating the treatment effects of SC on the outcomes of counties treated earlier in time (Sun and Abraham, 2021). Standard errors are clustered at the county level. $\bar{Y}$ refers to the pre-period average of the outcome variable in that specification. "Number of cities" refers to the number of cities represented in the regression. "Tract Share Hispanic" refers to the average Hispanic population share in the corresponding tracts.

FOR ONLINE PUBLICATION: APPENDICES

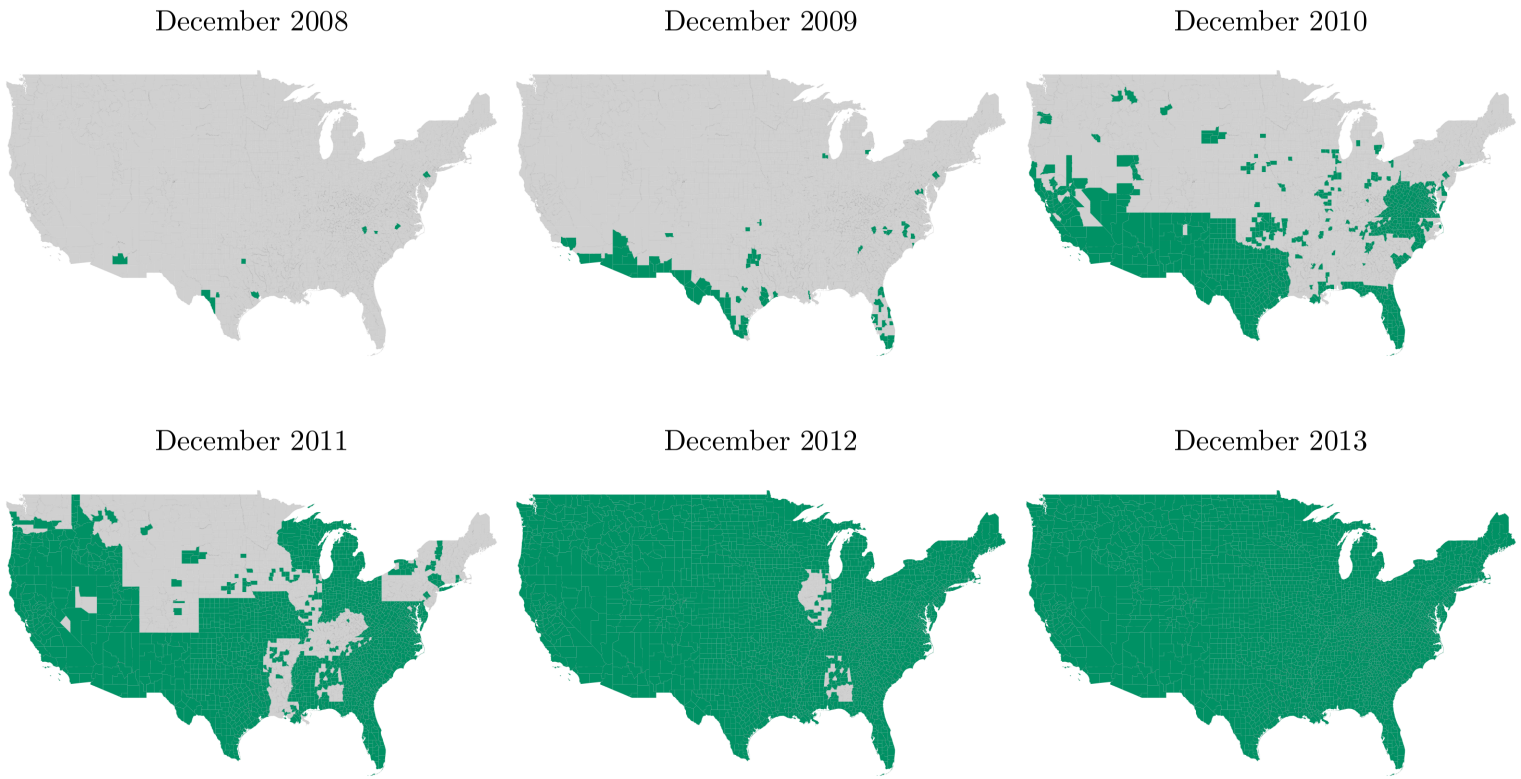
A Appendix Figures and Tables

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).

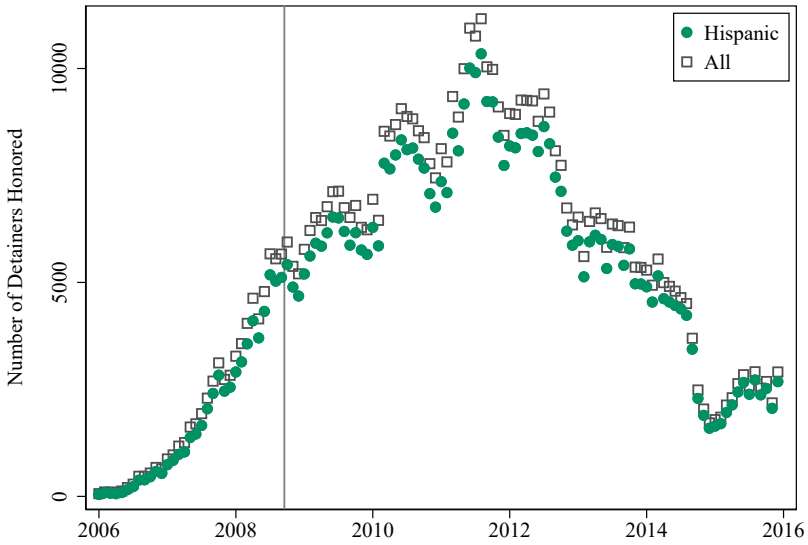
Figure A.2: Geography of Secure Communities (SC) Implementation



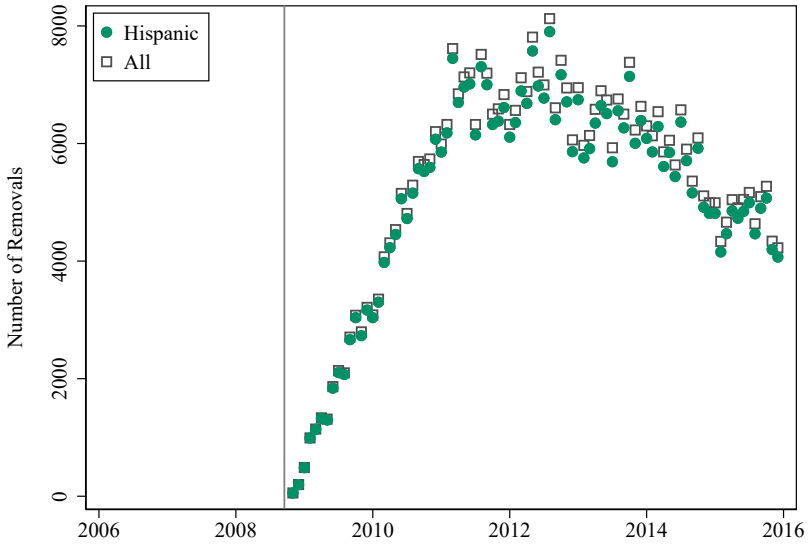
NOTE: This figure shows the county-level rollout of the Secure Communities Program over time, with counties that have implemented the program by each point in time highlighted in green.

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

(a) Number of ICE Detainers Honored

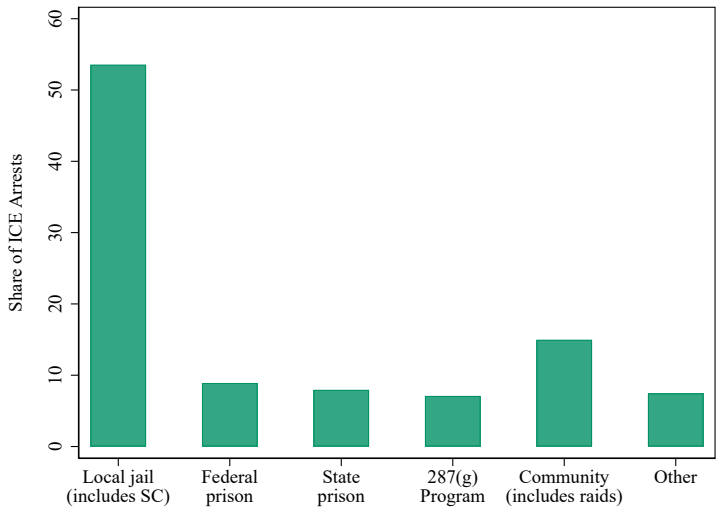


(b) Secure Communities (SC) Removals



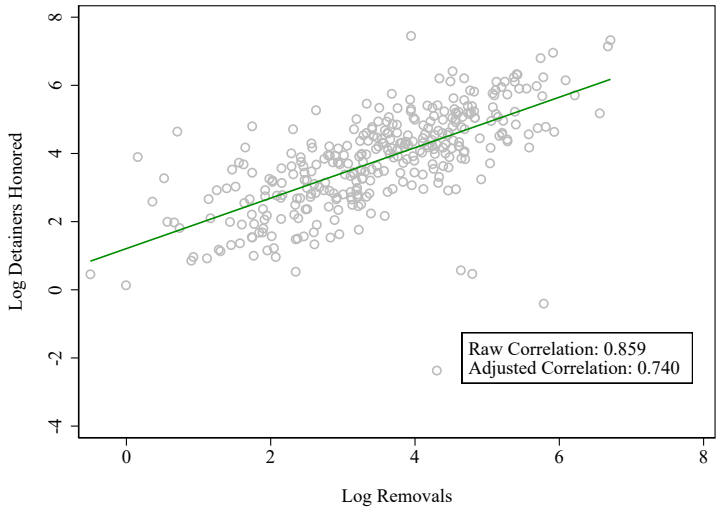
Notes: Panel (a) plots the number of monthly detainer requests honored by ICE using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention.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



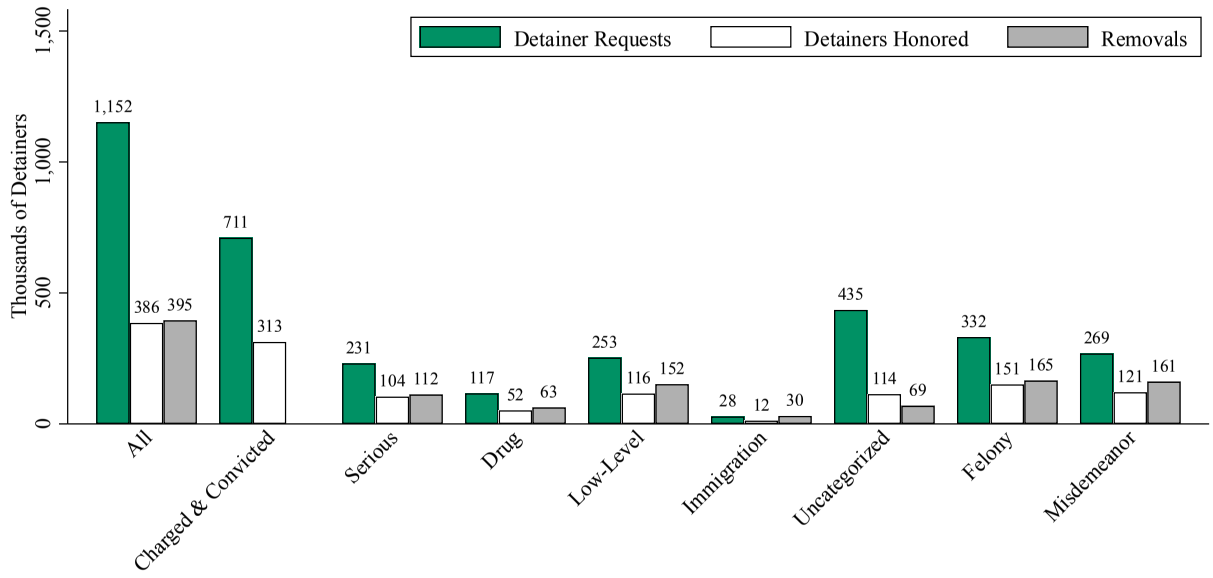
Notes: This figure plots the share of ICE interior arrests by source between 10/2008 and 8/2011 (TRAC, 2018). "Local" refers to individuals arrested by local police or sheriff's offices. "State" and "federal" refers to individuals who were transferred to ICE after being released from state and federal 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).

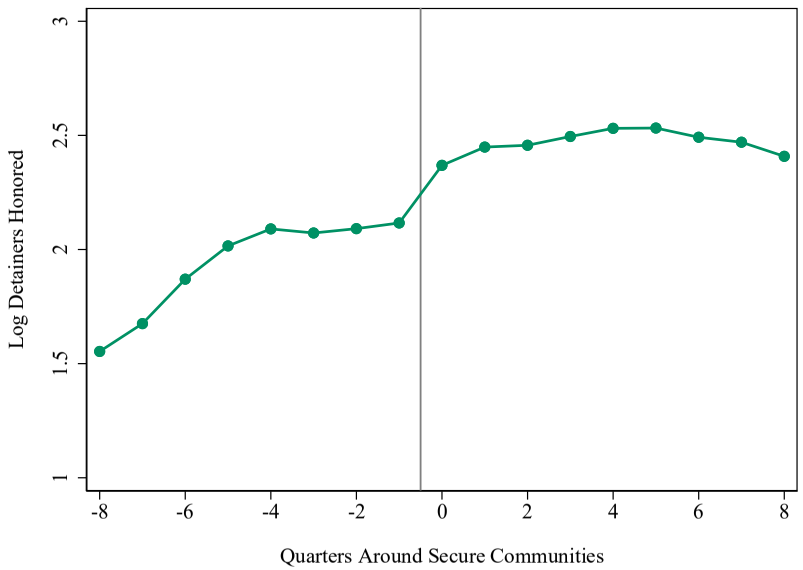
Figure A.6: ICE Detainer Requests, Detainers Honored, and Removals, by Offense Type



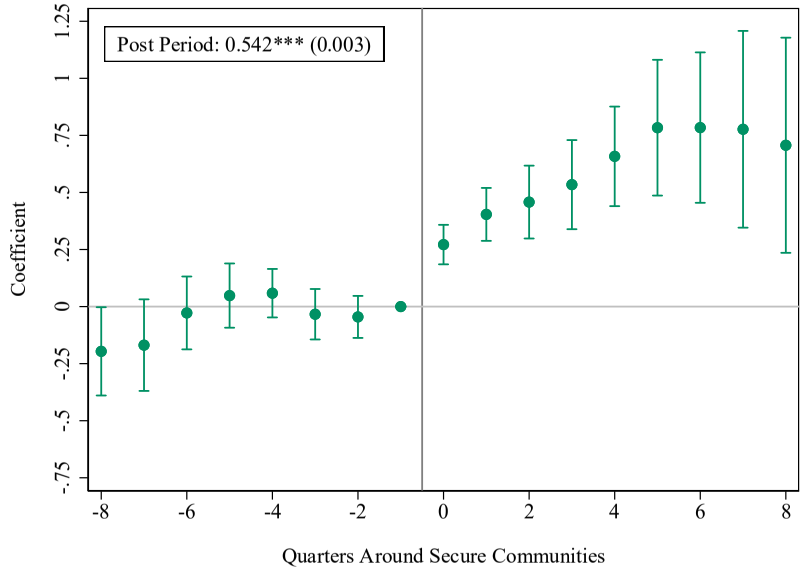
Notes: This figure shows the total number of ICE detainer actions by offense type using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. The data covers all actions after Secure Communities (SC) was implemented in each county from 10/2008 through 12/2014. A detainer 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: Logged Number of Honored ICE Detainer Requests around Secure Communities (SC) Implementation

(a) Raw Means

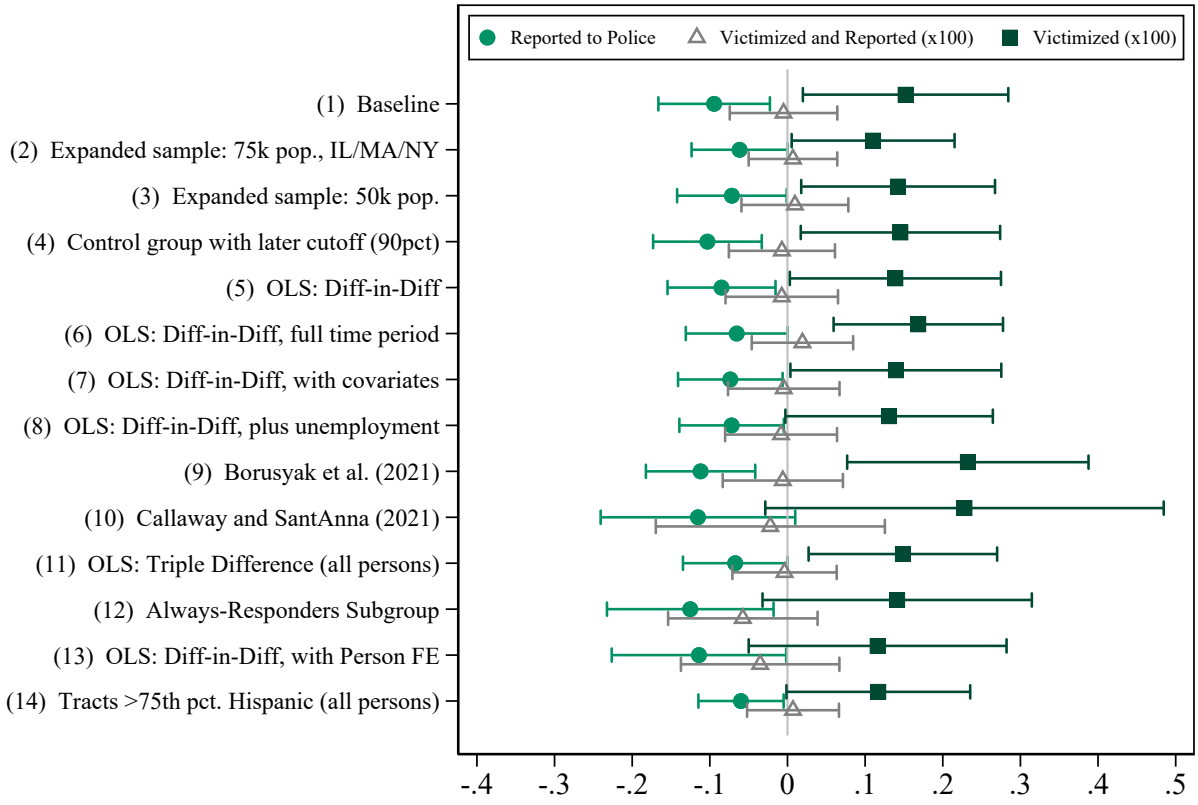


(b) Regression Estimates



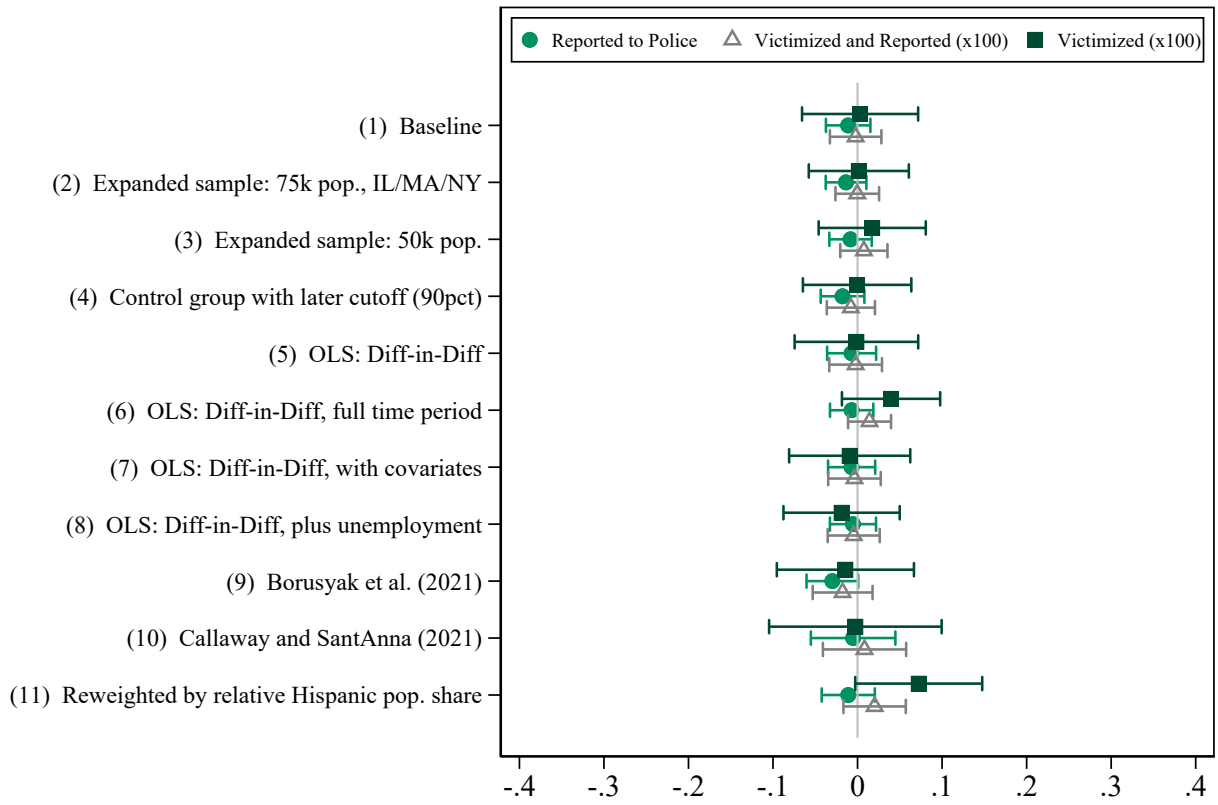
NOTE: *p<0.1, **p<0.05, ***p<0.01. Panel (a) plots the raw means of the logged number of honored ICE detainers around the implementation of the Secure Communities program. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. 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 (3) and reports the β_{Post} estimate and corresponding standard error from equation (2). In both panels, estimates are weighted by the county's population in that year (U.S. Census Bureau, 2022).

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



NOTE: This figure reports variants of β_{Post} from equation (2) 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.

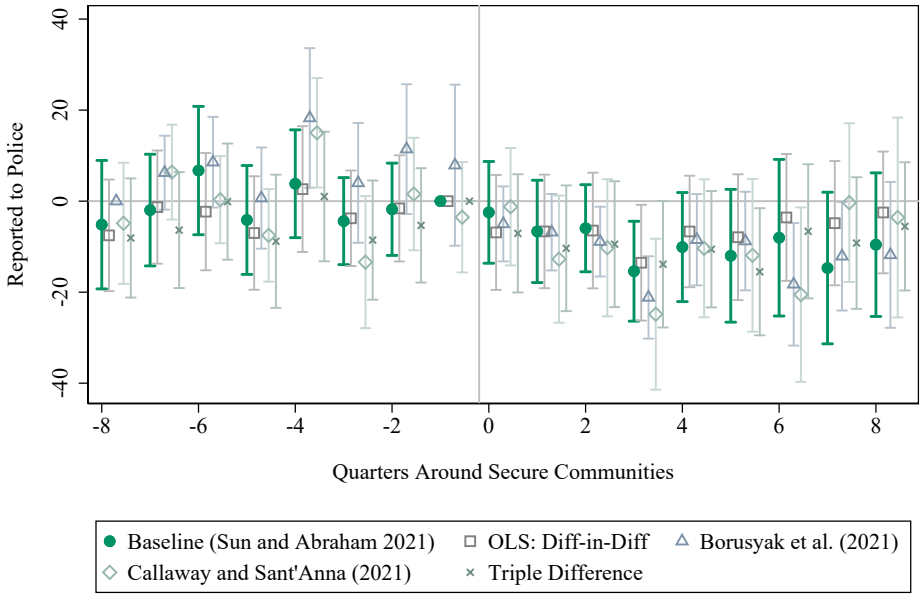
Figure A.9: Robustness of Main Results, Non-Hispanic Respondents



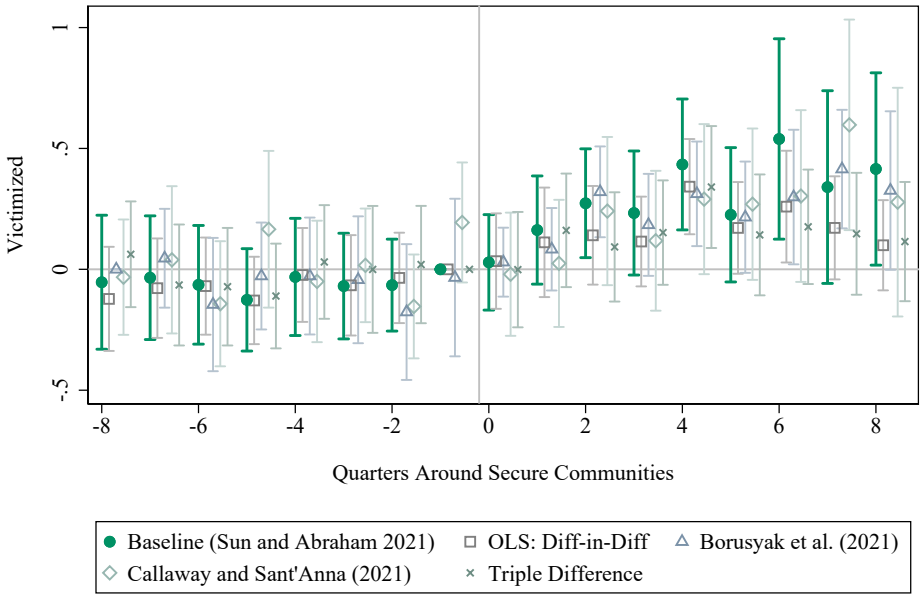
NOTE: This figure reports variants of β_{Post} from equation (2) for the non-Hispanic sample. The bars refer to the 95% confidence interval for the two-year post-period estimate of the Secure Communities (SC) program. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (3) include 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.10: Robustness of Event-Study Results, Hispanic Respondents

(a) Reported to Police



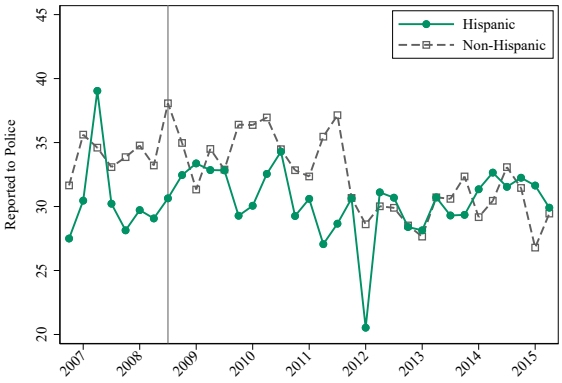
(b) Victimized



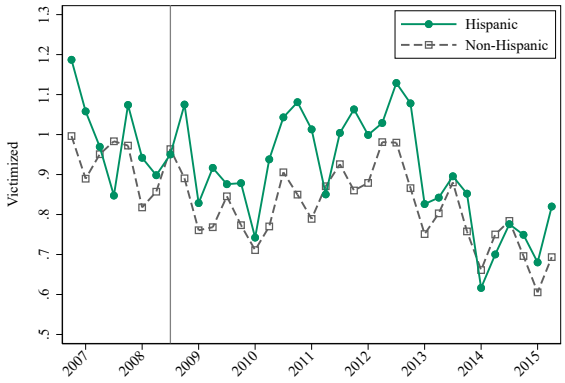
NOTE: This figure reports variants of β_{Post} from equation (3) for the Hispanic sample, with bars indicating 95% confidence intervals from standard errors clustered at the county level. The figures plot the baseline model estimates alongside the standard OLS difference-in-difference, a model following 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.11: Share of Crimes Reported to Police, Victimization Rates, and Reported Crime Rate In Calendar Time

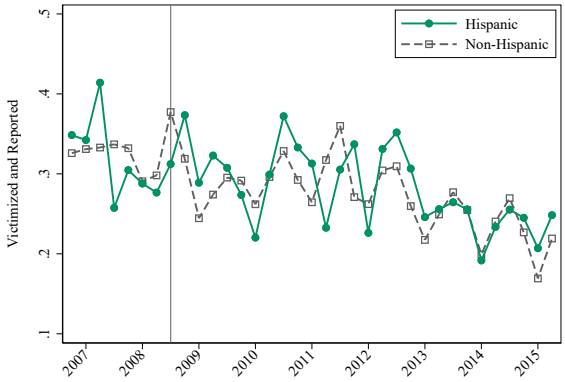
(a) Reported to Police



(b) Victimized

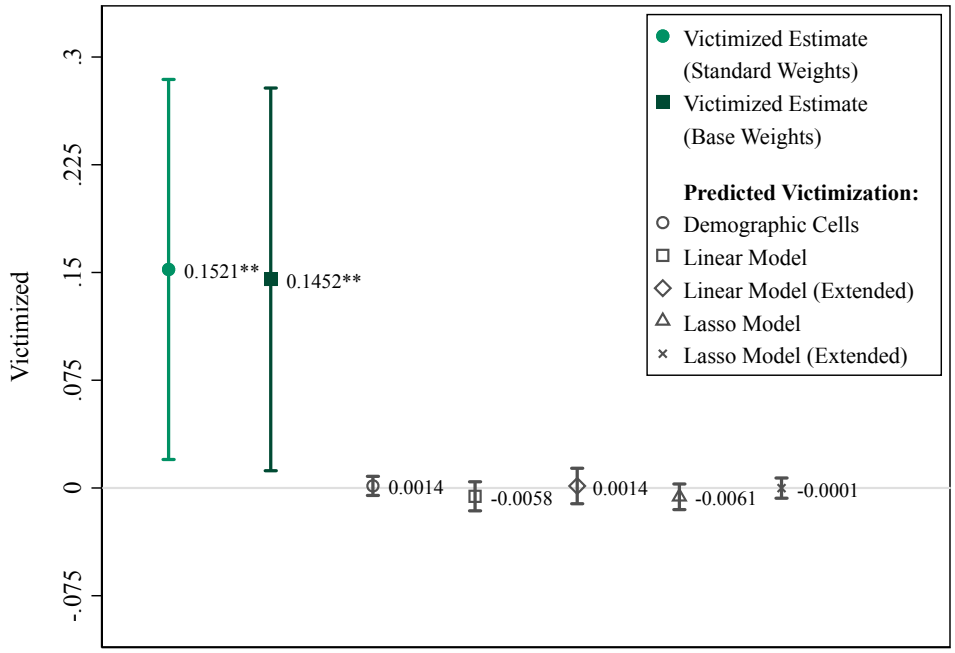


(c) Victimized and Reported to Police



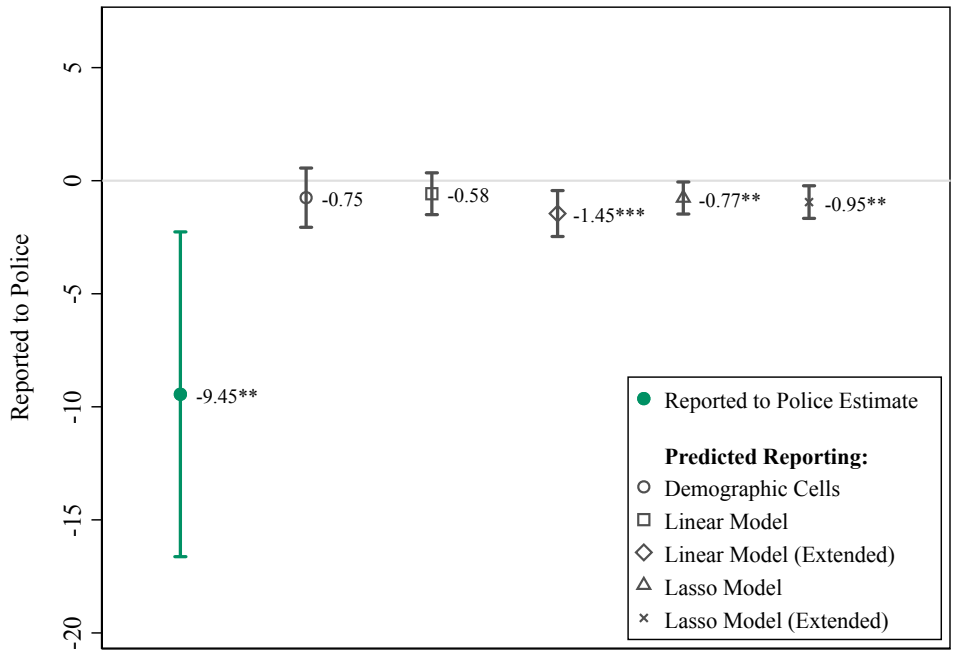
NOTE: These figures plot NCVS outcomes of the share of crime incidents reported to police, whether a person is victimized, and whether a person is both victimized and reported to police, separately for Hispanic and non-Hispanic respondents, between 2006 and 2015. Each outcome is multiplied by 100 for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

Figure A.12: 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.

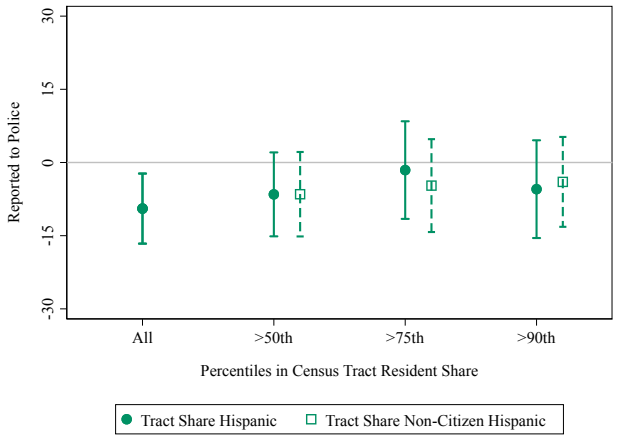
Figure A.13: Reported to Police Regression Estimate vs. *Predicted Reporting* given Observable Characteristics



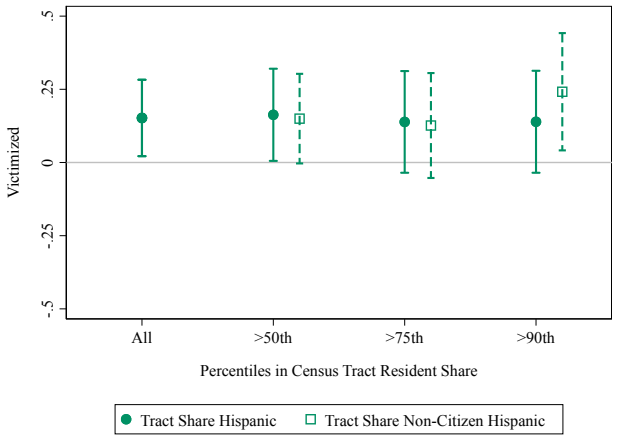
NOTE: *p<0.1, **p<0.05, ***p<0.01. The first estimate reproduces the baseline effect on the reporting behavior of Hispanic respondents following the implementation of Secure Communities (SC). The remaining estimates use pre-period data to generate various measures of predicted reporting based on victim 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.14: Results by Neighborhood Resident Characteristics

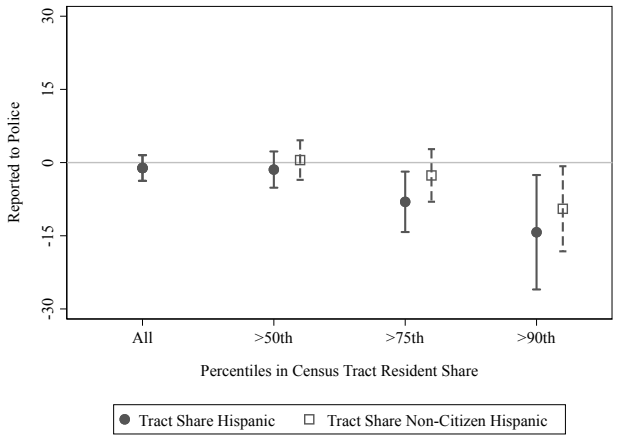
(a) Reported to Police, Hispanic



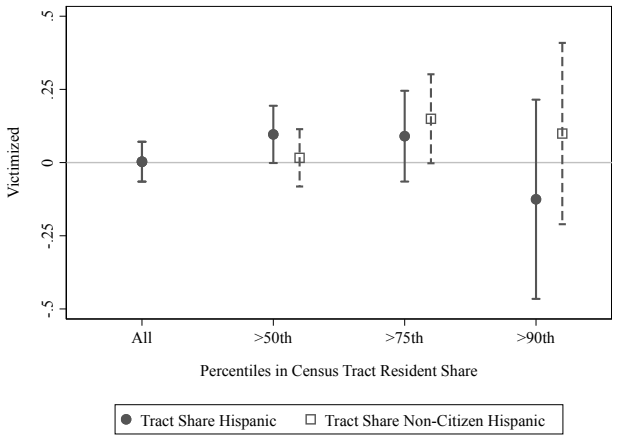
(b) Victimized, Hispanic



(c) Reported to Police, Non-Hispanic



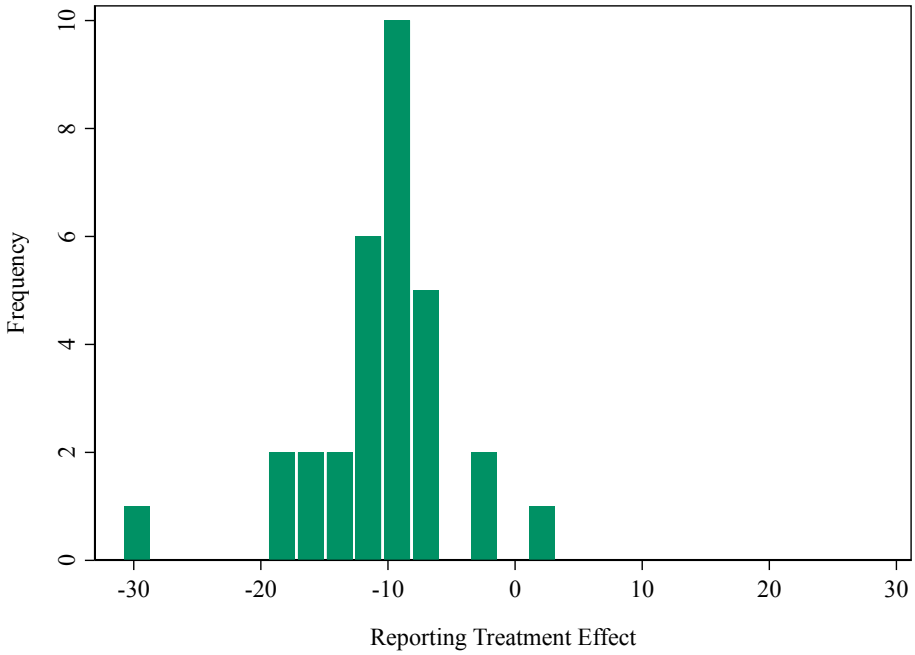
(d) Victimized, Non-Hispanic



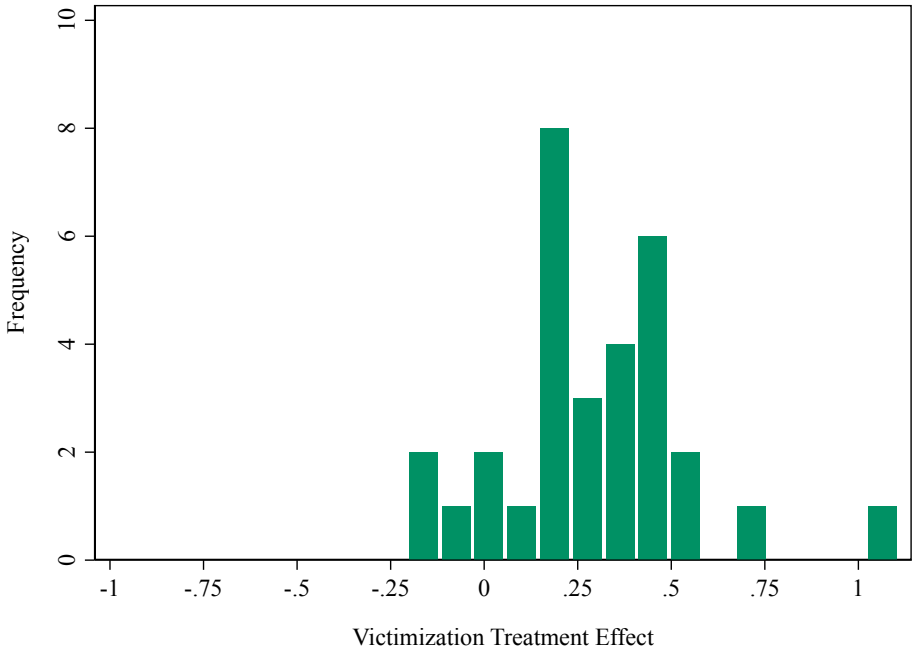
NOTE: This figure plots the estimates from equation (2) for subsamples of Hispanic and non-Hispanic survey respondents according to the share of their neighborhood that is Hispanic or non-citizen Hispanic. Data on neighborhood resident shares come from the 2000 Census and are linked to the NCVS based 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.15: Cohort-Level Effects of Secure Communities

(a) Reported to Police

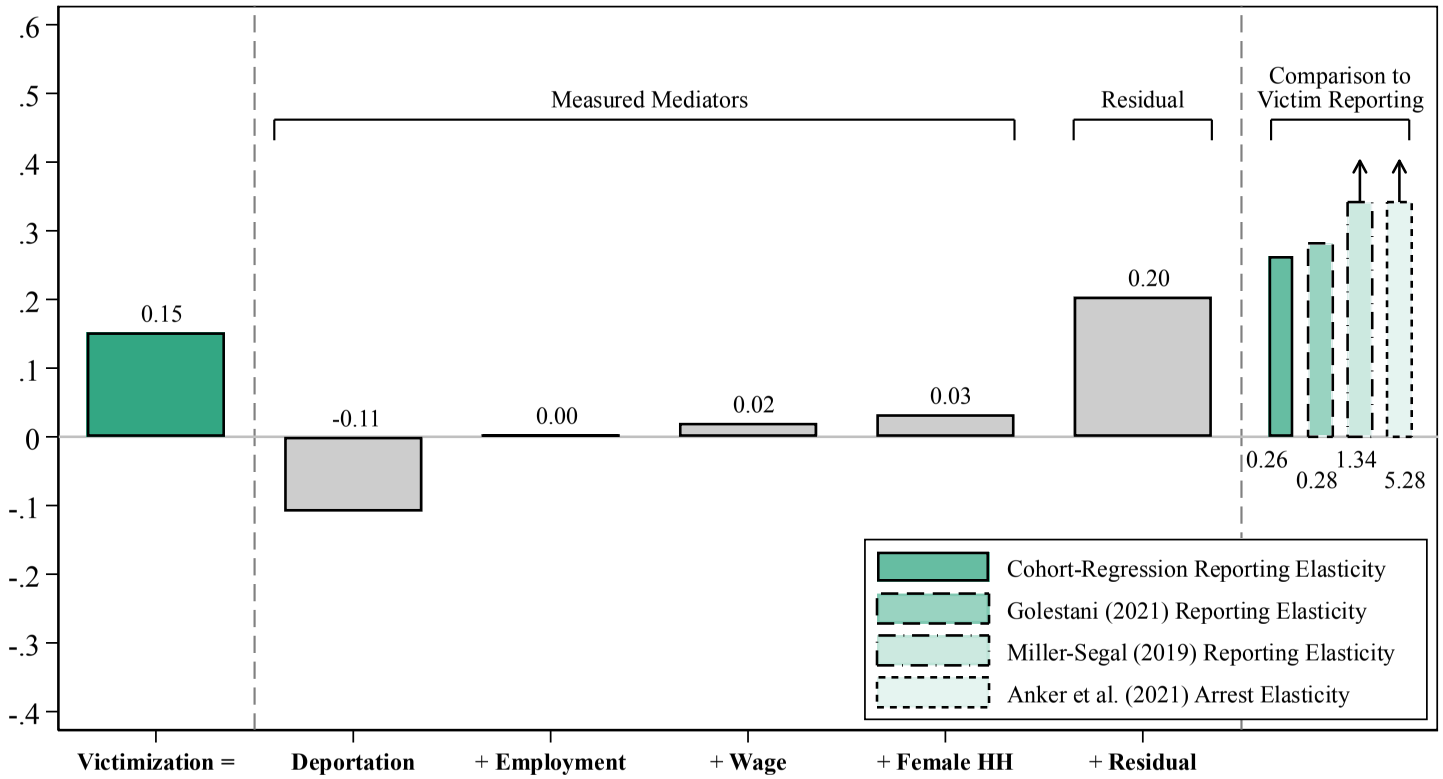


(b) Victimized



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 (2) separately for each activation cohort in the earlier-treated group and use the later-treated counties as the comparison group.

Figure A.16: Victimization Decomposition, Robustness to Alternative Elasticity Choices



Notes: This figure presents estimates from the decomposition outlined in Section 8.4. 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 four mediators. The final bar depicts the implied effect of reporting on victimization using our reporting effect from the NCVS (Table 2), with varying assumptions about the impact of victim reporting on offender behavior. For more details on these calculations, see Appendix G.6.

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

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

Table A.9: Effect of Secure Communities (SC) on Police Behavior

	β_{Post}	(S.E.)	Y Mean	N
A. Clearance Rate Among Reported Crimes				
(1) NCVS:				
All Victims	0.066	(1.224)	9.61	23,600
Hispanic Victims	-2.002	(2.812)	8.97	4,100
Non-Hispanic Victims	0.261	(1.436)	9.74	19,500
(2) UCR:				
Index Crimes	-0.052	(0.321)	20.00	13,098
Property Crimes	0.080	(0.277)	16.59	13,098
Violent Crimes	0.102	(0.918)	45.42	13,098
B. Police Response Time				
All Tracts	-0.144	(0.400)	4.94	23,836
Hispanic Tracts	0.090	(0.206)	3.68	3,540
Non-Hispanic Tracts	-0.150	(0.406)	5.16	20,296

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (2). Panel (a) reports changes in clearance rates using two data sets. The first set of results uses the baseline NCVS sample restricted to victimizations that are reported to the police. The outcome measures whether an arrest is made for a criminal victimization. The second set of results uses the FBI's Uniform Crime Reports (Kaplan, 2020). The sample and estimation are analogous to those in Table A.4 and use the share of crimes that were cleared as the outcome variable. Panel (b) uses administrative microdata collected from police departments across the country, as described in Appendix F. Police response time refers to the logged average seconds to dispatch. The unit of observation is at the tract-year-month level, and regressions include city × county and year × month fixed effects. Tracts are weighted by their 2005–2009 population (Manson et al., 2022). In all regressions, the sample is restricted to meet the sampling criteria described in Section 3. The regressions use later-treated counties as the control group for estimating the treatment effects of SC on the outcomes of counties treated earlier in time (Sun and Abraham, 2021). Standard errors are clustered at the county level.

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)
Deportation	-0.20	(Johnson & Raphael 2012)	-0.07	1.47 (61.55%)	-0.109
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
Residual					0.204
Comparison to Victim Reporting	-1.02	(Cohort-Level Regression)	-0.03	-9.45 (-28.62%)	0.263

NOTE: This table presents estimates from the decomposition outlined in Section 8.4 (also depicted in Figure 5). The top-right estimate corresponds to the effect of Secure Communities (SC) on victimization (Table 2). "Elasticity & Source" refers to the implied elasticity of offending 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. "Effect of SC" refers to the effect of the program on each of the mediators. The number in parentheses is the percent change in each mediator from its pre-period baseline value. The percent effect of SC on deportations comes from data on honored ICE detainers (row 3 of Table A.2). We estimate the effect of SC on employment, wages, and household composition 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" refers to the part of the total victimization effect that cannot be explained by the four mediators. "Comparison to Victim Reporting" refers to the effect of reporting on victimization using our reporting effect from the NCVS (Table 2) and our cohort-level victimization-to-reporting relationship (Figure 4). For more details on these calculations, see Appendix G.

B Conceptual Framework

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

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

They also face a cost for their crime, c, which is drawn from a cumulative distribution function $G(c)$ across offenders. This cost includes the psychic and opportunity cost of offending but not the punishment cost.

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

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

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

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

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

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

The Secure Communities program we consider is a shift in p_D. This can be thought of

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

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

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

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

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

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

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

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

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

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

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

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

Proposition 1 (Relationship between Crime and Reported Crime)**.** $\frac{dO}{dp_D} \leq 0 \Rightarrow \frac{dC}{dp_D} \leq 0$ and $\frac{dC}{dp_D} \geq 0 \Rightarrow \frac{dO}{dp_D} \geq 0$. But, $\frac{dO}{dp_D} \geq 0 \not\Rightarrow \frac{dC}{dp_D} \geq 0$ and $\frac{dC}{dp_D} \leq 0 \not\Rightarrow \frac{dO}{dp_D} \leq 0$.

We are also interested in how offending responds to a change in the victim reporting rate, $\frac{\partial O}{\partial r} = -G'(\cdot)(ax + ap_D D) \leq 0$. The framework shows how the ratio of the policy's impacts on offending and reporting provides an estimate of the effect of reporting on offending that is *biased upwards*:

$$\frac{dO}{dp_D} \Big/ \frac{\partial r}{\partial p_D} = \frac{\partial O}{\partial r} - G'(\cdot)\frac{ap_D D}{\epsilon_{r,p_D}} \geq \frac{\partial O}{\partial r},$$

where $\epsilon_{r,p_D} = \frac{\partial r}{\partial p_D} \Big/ \frac{r}{p_D} \leq 0$. We can further translate this expression into elasticities, as follows:

$$\frac{dO/O}{dp_D} \Bigg/ \frac{dr/r}{dp_D} \;=\; \epsilon_{O,r} + \frac{\epsilon_{O,p_D}}{\epsilon_{r,p_D}},$$

where $\epsilon_{x,y}$ generically reflects the partial derivative of x with respect to y. Because the direct effect of the change in deportation probability on offending is negative, as is the direct effect on victim reporting, the second term in the expression above is positive, so our empirical estimate of this ratio is a biased-upward estimate of $\epsilon_{O,r}$.

Extensions

We next outline various extensions that relax the simplifications in the basic framework outlined above. In particular, we extend the framework to incorporate citizens and allow for other features of the setting to respond to a change in enforcement intensity. A primary objective of this section is to consider how incorporating additional program impacts changes the conclusion from equation (1), that the ratio of program impacts on offending and reporting provides a biased-upwards estimate of the impact of reporting on offending.

Framework with Citizen Victims and Offenders — Until now, we have assumed that all victims and offenders are unauthorized. Here, we allow for a share of victims α_c to be citizens and for a share of offenders γ_c to be citizens.[40] For simplicity, we assume that offenders cannot choose whom to target, so they face a uniform reporting probability, and all victims face the same offending rate.

The same cost-benefit decision from the baseline framework applies to citizen and

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

non-citizen victims of crime, so that the share of reported incidents is:

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

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

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

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

Analogously, the number of offenses is:

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

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

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

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

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

Incorporating the presence of citizen offenders does not change the mapping from our program impacts to the desired elasticity of offending with respect to victim reporting, $\frac{dO/O}{dp_D} \Big/ \frac{dr/r}{dp_D} = \epsilon_{O,r} + \frac{\epsilon_{O,p_D}}{\epsilon_{r,p_D}}$. However, the direct effect of deportation risk on offending, $\frac{\partial O}{\partial p_D}$, will likely be smaller because only non-citizen offenders respond to deportation risk. Therefore, the empirical ratio of program impacts continues to be an upper bound on the (negative) elasticity of offending with respect to reporting, with a now-smaller bias term.

Distribution of Offender Costs — We also assumed that offenders' cost of offending c are unchanged by a change in p_D. The Secure Communities program intensified fears of participating in formal labor markets (East et al., 2023), so it may induce a leftward shift in the distribution of c, acting as another driver of more offenses O. To consider how this change may impact our model predictions, suppose that $G(\cdot)$ has mean 0, but we shift the distribution by μ_c, which will reflect the mean cost of offending. So the number of offenses is

now $O = G(M - rax - rap_D D - \mu_c)$. Allowing for p_D to impact the mean cost of offending, and assuming that an increase in deportation risk *reduces* the mean cost,

$$\frac{dO}{dp_D} = -G'(\cdot)\Big[\underbrace{\frac{\partial r}{\partial p_D}(ax + ap_D D)}_{\leq 0} + \underbrace{raD}_{>0} + \underbrace{\frac{\partial \mu_c}{\partial p_D}}_{\leq 0}\Big]$$

Written in elasticity form, we have

$$\frac{dO/O}{dp_D} \Big/ \frac{dr/r}{dp_D} = \epsilon_{O,r} + \frac{\epsilon_{O,p_D}}{\epsilon_{r,p_D}} + \frac{\epsilon_{O,\mu_c}\epsilon_{\mu_c,p_D}}{\epsilon_{r,p_D}}$$

Note that, with the possibility that deportation risk changes the mean cost of offending, the ratio of program impacts on offending and reporting, $\frac{dO/O}{dp_D} / \frac{dr/r}{dp_D}$, is no longer necessarily a biased-upwards estimate of the impact of reporting on offending, $\epsilon_{O,r}$. However, as we show in Section 8, multiple pieces of evidence point against changes in the costs/return to offending (e.g., economic conditions) as a driver of the increase in offending.

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

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

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

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

A related extension is one in which citizens, especially Hispanic citizens, worry about their *own* likelihood of being (unlawfully) detained because of heightened enforcement. In such a scenario, individuals might be less likely to report crimes to the police not because of empathy for their non-citizen neighbors, but because of increased fear of becoming ensnared

in the immigration system. Here, we would also expect a larger decline in the aggregate reporting rate relative to the baseline framework.

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

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

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

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

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

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

We can rewrite the above equation in elasticity form:

$$\frac{dO/O}{dp_D} \Big/ \frac{dr/r}{dp_D} \;=\; \epsilon_{O,r} + \frac{\epsilon_{O,p_D}}{\epsilon_{r,p_D}} + \frac{\epsilon_{O,a}\epsilon_{a,p_D}}{\epsilon_{r,p_D}}$$

As with the earlier extension allowing for an SC impact on the costs of offending,

allowing for impacts on police effectiveness changes equation (1) so that the ratio of program impacts on offending and reporting is no longer necessarily a biased-upwards estimate of the elasticity of offending with respect to reporting, $\epsilon_{O,r}$. However, as we discuss in Section 8, multiple pieces of evidence point against changes in police effectiveness as a driver of the increase in offending.

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

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

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

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

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

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

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

$$\text{Number of reported offenses:} \quad rNO = \frac{rO\lambda}{Orap_D(1-\theta)+\theta}$$

Incorporating the number of potential offenders adds an additional margin along which changes in p_D could impact overall offending, NO. This margin is summarized by $Orap_D$, which is the number of individuals who are deported in a given period. It is now no longer the case that the change in total offending always has the same direction of response as

the change in the "per capita" offending rate.[41] In particular, even if the offending rate is unchanged ($\frac{dO}{dp_D} = 0$), overall offending could still decline in response to the policy if the mass of potential offenders shrinks ($\frac{dOrap_D}{dp_D} > 0$).

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

C NCVS Sample Design

C.1 Overview of Survey

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

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

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

C.2 Survey Non-Response

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

The final type of non-response is a household nonresponse, which occurs when an interviewer arrives at the selected address but is not able to obtain an interview. This could occur — similarly to the person-level non-response — because the household is not home or is unwilling or unable to participate in the survey. However, this type of non-response could also occur for other reasons (e.g., if the living quarters are vacant or the address is no longer used as a residence). Interviews that do not occur despite the persons in the household being eligible for the interview are referred to by the Census Bureau as "Type A" interviews.[42]

[42] "Type B" interviews refer to those in which the sample household is no longer eligible for interview, but could become eligible later (e.g., a vacant address). "Type C" interviews refer to those in which the address should be permanently removed from the sample (e.g., the housing unit has been demolished).

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

Table A.7 shows that survey response rates are relatively high, at 77% when considering all households, and the survey response rates are equally high in Census tracts with high shares of Hispanic residents. Importantly, the NCVS introduced a Spanish language instrument in 2004, which allowed respondents to request that the interview be conducted in Spanish.

After the data collection, the Census Bureau creates weights to adjust the sample counts and correct for differences between the sample and population totals.[43] Throughout our baseline analysis, we use person-level weights to maintain sample representativeness. This weight incorporates a non-response weighting adjustment that allocates the sampling weights of both non-responding households and non-responding persons to respondents with similar characteristics.

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

D Impact of Secure Communities on Apprehension Risk

The importance of victim reporting stems from its role in affecting the likelihood that offenders are apprehended for crimes. As victims report fewer crimes, the risk of apprehension, ra, will decrease. A natural question is whether the decline in reporting translates into a substantial decline in the total probability of apprehension. Without a victim report, it is unlikely that the police will be able to identify and arrest an offender. Conversely, if the crimes for which reporting is reduced have arrest rates that are already quite low (i.e., a is small), then a change in reporting may not meaningfully alter apprehension rates.

Table A.9 shows that within the NCVS, the average likelihood of arrest among reported offenses is 9%. If we combine this rate with the estimates in Table 2, we conclude that the total apprehension risk for offenses committed against Hispanic victims declines from 3% to 2.1% after SC.

As another way to consider the overall change in apprehension risk, we estimate equation (2) using an outcome that denotes whether an arrest was made for a victimization incident. We estimate this regression on the sample of *all* victimizations. The first row of Table D.1 shows a negative point estimate 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 in apprehension risk is comparable to the decline in reporting, providing evidence that the decline in reporting translated to a lower overall apprehension rate.

Table D.1: Apprehension Risk Among All Victimizations

	β_{Post}	(S.E.)	Y Mean
Hispanic Victims	-1.625	(1.202)	4.36
Non-Hispanic Victims	-0.408	(0.632)	5.13
All Victims	-0.579	(0.547)	5.00

NOTE: *p<0.1, **p<0.05, ***p<0.01. This table reports estimates using equation (2). The outcome is whether an arrest is made for a criminal victimization. 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.

E County-Specific Treatment Effects

We estimate a series of county-specific treatment effects from the Secure Communities program. Specifically, we use equation (2) to estimate an effect for each earlier-treated county, using respondents in later-treated counties as a comparison group. We generate an estimate $\hat{\beta}_c$ and standard error $\hat{\sigma}_c$ for SC's impact in each county c for each outcome and ethnicity group. The empirical distribution of estimated effects includes noise from estimation error and thus may overstate the degree of variation in county-level impacts. We address this issue with a deconvolution procedure (Goncalves and Mello, 2021; Kline et al., 2022).

These estimated effects are a sum of the true county-level effect and estimation error:

$$\hat{\beta}_c = \beta_c + \epsilon_c$$

where ϵ_c is normally distributed and has a standard deviation that is estimated by $\hat{\sigma}_c$. Our goal is to identify the distribution of true effects, $f(\beta_c)$. Supposing that the effects can take on a finite set of values on a fine grid, $\beta_c \in \{\beta^k\}$, the likelihood of observing a county with a given estimated effect can be written as:

$$\Pr(\hat{\beta}_c) = \sum_{\{\beta^k\}} f(\beta^k) \cdot \Pr(\epsilon_c = \hat{\beta}_c - \beta^k)$$

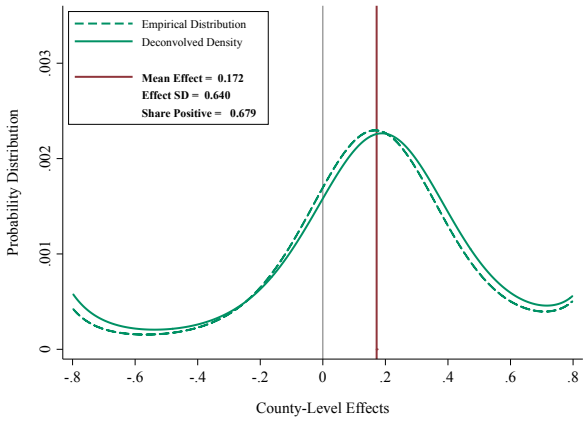
We estimate the distribution $f(\beta^k)$ using maximum likelihood, where we parametrize the effect distribution with a four-parameter exponential family distribution (Efron, 2016).[44] To produce the density of *estimated* effects, we conducted the same deconvolution procedure but divided all standard errors by 100. For Census disclosure purposes, we chose this procedure as an alternative to the standard kernel density approach, where each point in the distribution is estimated on a potentially small set of observations.

Figure E.1 displays the estimated densities of treatment effects, as well as the corresponding "deconvolved" density of treatment effects. The victimization (reporting) panels report the estimated share of counties with a positive (negative) effect. The distributions show that while the mean impacts align with our baseline results (red vertical lines), there is significant variation in effect sizes. The only outcome where effects are strongly concentrated in one direction is reporting among Hispanics: we estimate that 79% of counties have a negative treatment effect. Although the distribution of Hispanic victimization effects has more dispersion — a sizable share of counties have both large positive and negative impacts — we find that a high share (68%) of counties have a positive treatment effect. For non-Hispanic respondents, the outcome distributions are, unsurprisingly, centered around zero.

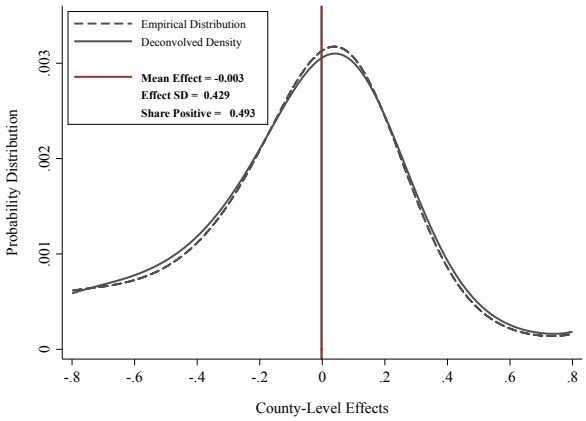
[44] We make the simplifying assumption here that the error terms ϵ_c are independent across counties. Although this assumption is likely to be violated (the same set of counties are used as controls for each treated county), we impose it for tractability, as it allows us to construct the log-likelihood as a simple summation of likelihoods across counties.

Figure E.1: County-Level Effects of Secure Communities with Deconvolution Procedure

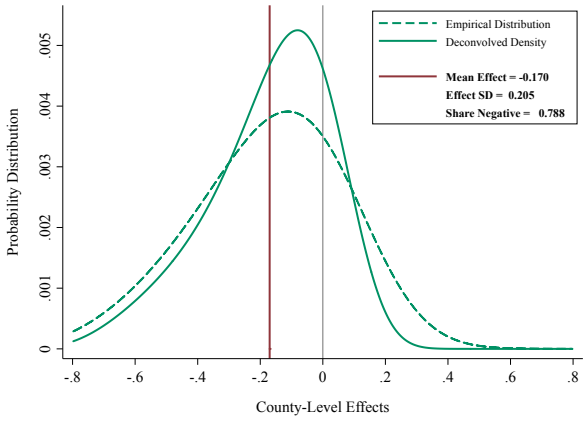
(a) Victimized, Hispanic



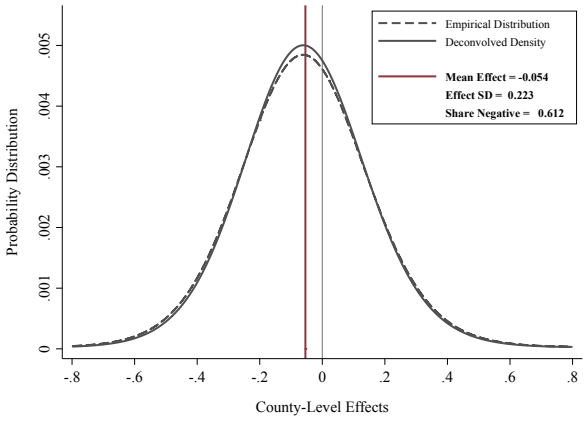
(b) Victimized, Non-Hispanic



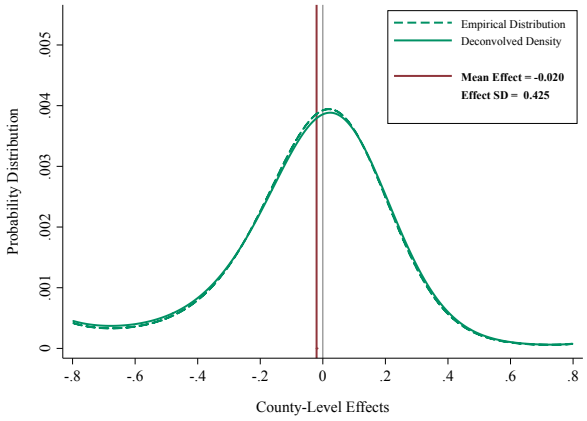
(c) Reported to Police, Hispanic



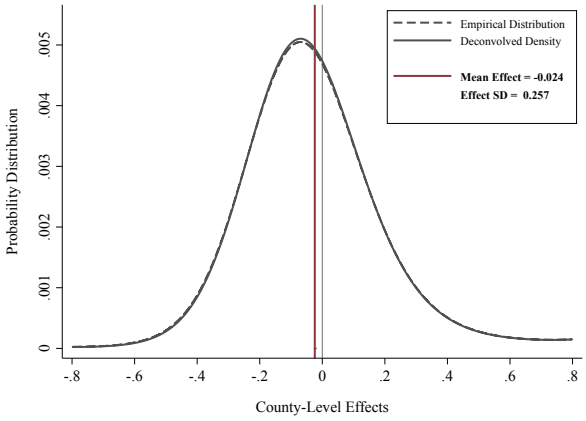
(d) Reported to Police, Non-Hispanic



(e) Victimized and Reported, Hispanic



(f) Victimized and Reported, Non-Hispanic



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

F Description of Police Administrative Data

We acquired micro-data on 911 calls and arrests from police departments across the country for the years 2006 to 2011. Every 911 observation records the date, time, and address of the incident, as well as a basic description of the call type. Each arrest observation also records the date, time, and address where the arrest occurred, as well as basic demographic information on the arrestee including their age, gender, and race/ethnicity.

Coverage of Outcomes — The data were obtained through public records requests to medium and large cities in the U.S. We only include cities in our sample that provided data for all months in the period between 2006 and 2011. In addition, we only include cities that satisfy our baseline sample restriction of non-border counties, counties with $> 100,000$ residents in 2000, and outside of Massachusetts, Illinois, and New York.

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

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

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

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

We identified officer-initiated incidents by hand-coding the categories of call types. We assigned two research assistants to code each city's 911 call categories. In cases where they disagreed on whether a category should be designated as officer initiated, a third research assistant made a final decision. We do not include cities in our sample where this process for identifying officer-initiated calls performs poorly.

Sample Selection — Our initial set of cities come from public records requests with complete coverage of the sample window and where the data quality of the obtained records met a minimum threshold. For both calls and arrests, we vetted data quality by plotting the

time series of outcomes to identify odd breaks, trends, or levels that likely stem from data quality issues. We also confirmed the quality of geocoding in each city, and exclude cities where a high share of calls cannot be geocoded or where a large share of months have a high share of calls that cannot be geocoded.

We collapse the data to be at the level of tracts-by-months. Because the data comprise every call taken and arrest made by a department, some tracts only appear rarely. Many of these cases occur because of enforcement actions taken outside of a department's typical jurisdiction. To restrict attention to tracts that we are confident are consistently covered by the department, we take several sample selection steps: (1) we drop tracts in counties where the county contains less than five percent of all records from an agency; (2) we drop tracts that are observed in more than one agency in our data; (3) in our calls data, we drop tracts that appear in fewer than 25% of months; in our arrest data, we drop tracts that appear in fewer than 5% of months.

Final sample — Certain outcomes can only be measured using a subset of the cities. When considering police arrival time, we restrict the analysis to the 16 cities with available information on seconds to arrival in the 911 calls data. When considering the number of Hispanic and non-Hispanic arrestees, we restrict the analysis to the 37 cities with consistently available information on arrestee ethnicity. The list of cities in our data is reported in Table F.2. Across all specifications, we define a tract as a "Hispanic tract" if the share of the population that is Hispanic is above the 90th percentile of the tract-level Hispanic share distribution among tracts that meet the sampling criteria described in Section 3.

Table F.2: Cities with Police Administrative Data

	(1) Response time	(2) Arrests
Antioch, CA	✓	✓
Beaverton, OR		✓
Bellingham, WA		✓
Billings, MT		✓
Cedar Rapids, IA		✓
Charlotte, NC		✓
Chico, CA		✓
Dayton, OH		✓
Durham, NC		✓
Fort Worth, TX		✓
Fresno, CA	✓	
Gilbert, AZ		✓
Glendale, AZ		✓
High Point, NC		✓
Kalamazoo, MI	✓	
Lees Summit, MO	✓	
Lewisville, TX	✓	✓
Long Beach, CA		✓
Longview, TX	✓	✓
Los Angeles, CA		✓
Mansfield, TX	✓	✓
Maple Grove, MN	✓	✓
Marysville, WA		✓
Menlo Park, CA		✓
Milwaukee, WI		✓
Mission Viejo, CA	✓	
Olympia, WA	✓	
Pasadena, TX		✓
Pomona, CA		✓
Providence, RI		✓
Reno, NV		✓
Richardson, TX	✓	
Rochester, MN	✓	
Roseville, CA		✓
Round Rock, TX		✓
San Clemente, CA	✓	
San Jose, CA		✓
Santa Clara, CA	✓	✓
Stockton, CA		✓
Surprise, AZ		✓
Temecula, CA	✓	
Temple, TX		✓
Tustin, CA		✓
Ventura, CA		✓
Virginia Beach, VA	✓	✓
Walnut Creek, CA		✓

NOTE: This table lists cities with police administrative data, described in Section 8.3 and Appendix F.

G Mediation Analysis of Victimization Increase

In this appendix, we provide further detail on the decomposition exercise discussed in Section 8.4. Specifically, we follow the mediation analysis framework and notation of Heckman et al. (2013) and Fagereng et al. (2021) to model how the increase in victimization that we find is related to a set of "intermediate" outcomes (i.e., mediators) that were also impacted by the Secure Communities program. To conduct this exercise, we first quantify the effect of Secure Communities on various mediator variables and then we utilize estimates from the economics literature to assess the plausible effect of these mediators on victimization.

G.1 Framework

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

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

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

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

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

$$\mathrm{E}[V_1 - V_0] = \underbrace{\sum_{j \in \mathcal{J}_p} \alpha^j \mathrm{E}[\theta_1^j - \theta_0^j]}_{\substack{\text{Treatment effect due} \\ \text{to observed mediators}}} + \underbrace{\mathrm{E}[\tau_1 - \tau_0]}_{\substack{\text{Treatment effect due} \\ \text{to unobserved mediators}}} \tag{5}$$

The left-hand side of this equation is the overall victimization effect of Secure Communities,

reported in Table 2.

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

G.2 Effect of SC on Mediators

Deportations. To quantify the effect of Secure Communities on deportations, we return to our analysis of the program's impact on enforcement, shown in Appendix Table A.2. Honored detainers are our proxy of removals, and we use honored detainers per 100k capita. We estimate that Secure Communities increased county-level deportations by 62%.[45]

Labor Market and Demographic Outcomes. We follow the approach of East et al. (2023) and use the 2005–2014 American Community Surveys to quantify the effect of the Secure Communities program on labor market and demographic outcomes. To quantify the two-year effect of the program, we estimate a specification analogous to equation (2), as follows:

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

where Y_{ct} is an outcome variable for county c at year t. SC_c is an indicator variable equal to one if county c had implemented the Secure Communities program for at least half of year t. μ_c and δ_t correspond to county and year fixed effects, respectively. Standard errors are clustered at the county level and the model is weighted by the county's 2000 population. The coefficient of interest is β_{Post}, estimating the average difference in outcome Y in the two years after the implementation of the Secure Communities program relative to the difference in the outcome prior to the program's launch.[46]

In this exercise, we consider three outcomes that Secure Communities may have affected beyond Hispanic individuals' reporting behavior. Specifically, we estimate the effect of SC on the employment-to-population ratio and logged hourly wages of Hispanic low-educated foreign-born individuals. East et al. (2023) shows large impacts on the employment and wages of low-educated foreign-born workers, so we similarly focus on this subgroup of His-

[45] Note that the estimated 62% impact on honored detainers is slightly different from our first stage estimate of 54% in Figure A.7, where we use the logarithm of honored detainers. We use a per-capita outcome here in order to have an interpretable mean value for the outcome and to maintain a consistent specification across mediators. Using the alternative 54% impact on log honored detainers leads to identical findings from the mediation analysis.

[46] There are a few notable differences between this specification and that in East et al. (2023). First, to remain consistent with NCVS data, we use counties — rather than commuting zones — as the measure of geography. Second, we quantify the two-year effect of the program, rather than focusing on all time periods after the program's implementation. And third, we omit county-specific linear time trends from the specification.

panics. Finally, we consider the effect of SC on the share of Hispanic household heads that are female.

G.3 Effect of Mediators on Crime

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

For deportation, we rely on Johnson and Raphael (2012), which studies the impact of changes in incarceration rates on crime rates. This paper reports elasticities of property crime with respect to incarceration of -0.213 and of violent crime on incarceration of -0.113 (Table 4). Since 83% of victimizations are for property offenses (see Table 1), we weight the elasticities accordingly to get an overall elasticity of -0.196. Note that the estimand of interest in this paper is the causal effect on crime of the stock of incarcerated individuals. Therefore, their treatment effect captures both incapacitation and deterrence effects, which are both relevant in our context.

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

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

G.4 Implied Effect of Mediators on Victimization

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

After accounting for the observed mediators, we calculate a large and positive "residual" effect. This is the change in victimization that is not explained by the observed mediators. As we discuss below, the residual effect is very similar in magnitude to the victim reporting effect on victimization implied by our cohort-level regression.

G.5 Robustness to Alternative Choices

Deportations. An alternative approach for estimating the impact of deportations on crime would be to use TRAC data on SC removals to calculate the average number of individuals deported in our baseline counties during our sample period. When we use this number in conjunction with semi-elasticites from Johnson and Raphael (2012) (Table 4), we find a nearly identical implied effect of SC deportations on victimization.

Share of Male Immigrants. Instead of only considering changes in population stemming from deportations, we can also consider the overall change in the population share of Hispanic low-educated foreign-born men using the ACS. This estimate combines deportation and migration responses for the likely unauthorized immigrant share of the population. Like East et al. (2023), we define this outcome as the number of male likely unauthorized immigrants divided by the working-age population in that county in 2005.[47] We find a 3.34% decline in the male immigrant share following SC.

We then rely on Chalfin and Deza (2020), which studies the impact of labor market immigration enforcement on crime rates. This paper finds that the Legal Arizona Workers Act (LAWA) reduced Arizona's foreign-born Mexican (non-citizen) population share by 17%. After LAWA's passage, violent and property crime fell by 10.7% and 19.7%, respectively. These estimates imply an elasticity of violent crime on immigrant share of 0.63, and an elasticity of property crime on immigrant share of 1.16. Similarly to above, we weight the elasticities to get an overall elasticity of 1.07.

Putting together the estimate with the corresponding elasticity, we conclude that the predicted effect on victimization from this change in the population is -0.032. Had we incorporated this population share as a an additional observed mediator, we would have found a slightly more positive "residual" effect (0.236 versus 0.204). However, given the overlap between these measures, our baseline analysis does only considers direct changes in deportations.

G.6 Discussion: Role of Reporting

We argue that the decline in victim reporting is the key driver of the increase in victimization. Accordingly, the mediation analysis shows that the SC-induced change in victimization that is unexplained by our observed mediators (0.203) is quite similar to the baseline impact on victimization (0.152). Here, we further show that, if we use victim reporting as an observed mediator in our framework, we would have reached a similar conclusion.

Because of the lack of a general elasticity of offending-to-reporting from the literature,

[47] Given time trends, we use a de-trended version of this outcome: specifically, we implement a two-step method in which we first estimate a linear trend for each county using pre-period data only, and then we subtract the fitted trend from all of the county's data points (Goodman-Bacon, 2021).

we measure this object directly from our data. To do so, we use the estimates from the cohort-level analysis in Section 8.1: we calculate the implied elasticity of offending to reporting using variation across cohorts in the magnitude of effect sizes. We estimate that a 1 p.p. decline in victim reporting is associated with a 0.017 p.p. higher cohort-level victimization rate, leading to an implied elasticity of offending to reporting of -1.02. Interpreting our cohort-level analysis as a causal elasticity of offending to reporting requires the assumption that differences in reporting effects across cohorts are independent of changes in other mediators of the victimization effect. This approach aligns closely with the approaches taken in mediation analyses (e.g., Heckman et al., 2013; Fagereng et al., 2021) that estimate the effect of mediators on the outcome directly using own data. In sum, our estimates from the NCVS yield a predicted effect on victimization that is close in magnitude, although slightly larger, than the residual effect from the decomposition.

In Appendix Figure A.16, we then compare our predicted effect from the NCVS against what we would have predicted using other estimates from the literature. Note that this comparison is imperfect, since these existing estimates pertain to specific offender populations.

We begin by considering estimates from Golestani (2021), which studies nuisance property ordinances (NPOs) that increase the cost of contacting 911 for residents experiencing intimate partner violence. This paper finds that NPOs lead to a -0.075 p.p. decline in victim reporting off a mean rate of 0.585 (Table 3) and a 0.21 p.p. increase in the likelihood of assault victimization off a mean of 1.5 (Table 6). These estimates imply an elasticity of assault victimization on reporting of -1.09. The predicted effect of reporting on victimization using this study is quite close to our estimate.

Next, we borrow an estimate from Miller and Segal (2019), which also studies victim reporting behavior and domestic violence. This paper finds that a 10% increase in the female officer share leads to a 0.39 p.p. increase in victim reporting (Table 7, column 5) off a mean reporting rate of 0.55 (Table 1, panel A). A 10% increase in the female officer share leads to a 0.007 decline in the rate of domestic violence victimization (Table 6, column 2) off a mean victimization rate of 0.002 (calculation using paper's replication files). These estimates imply an elasticity of domestic violence victimization to reporting of -5.2. Using this elasticity, we would have predicted a significantly larger effect on victimization given the decline in reporting that we observe.

Finally, instead of considering victim reporting, we consider apprehension risk as the mediator. As described in Appendix D, we estimate a Secure Communities effect of -1.6 p.p. (SE= 1.202) on the likelihood of an arrest being made for a victimization of a Hispanic individual, relative to a mean arrest rate of 4.36%. While our point estimate is statistically insignificant, it is quantitatively large — corresponding to a 37% decline in the apprehension risk — and it is consistent with the fact that victims are less likely to report to the police, which is almost always necessary for an arrest. To measure how a change in the apprehension risk affects crime, we borrow estimates from Anker et al. (2021), which estimates the effect of adding offenders to a DNA database in Denmark on apprehension probability and deterrence. They find that being added to the database increases the likelihood that an offender is detected for committing a future crime and reduces the likelihood of a future offense. Their estimates imply an elasticity of offending with respect to apprehension probability of -2.7.

Again, we would have predicted an even larger effect on victimization if we had relied on estimates from this study.