# Supplemental Appendix

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

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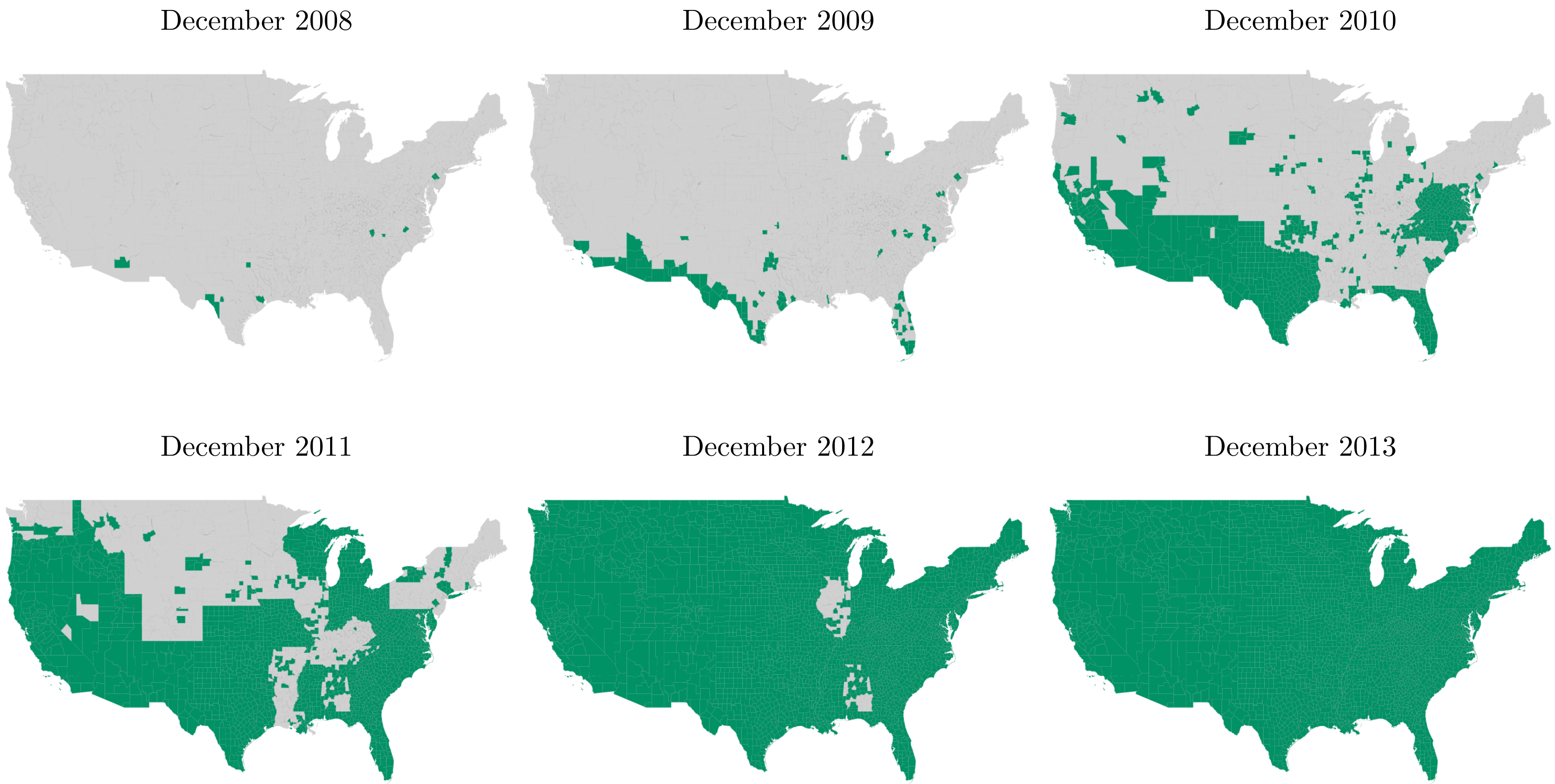
# A Appendix Figures and Tables

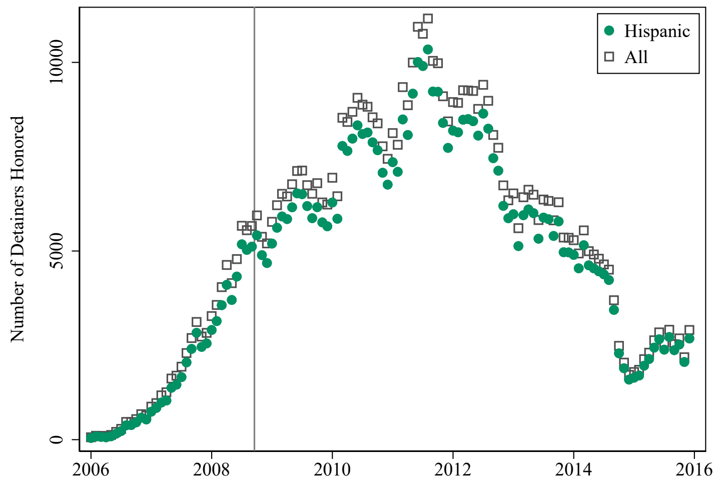
Figure A.1: Geography of Secure Communities (SC) Implementation



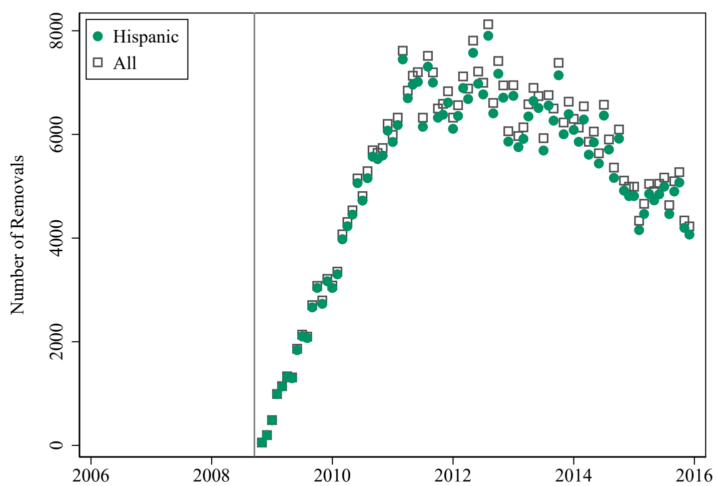

NOTE: This figure shows the county-level rollout of the Secure Communities Program over time, with counties that have implemented the program by each point in time highlighted in green.

Figure A.2: Number of ICE Honored Detainers and Removals Over Time

(a) Number of ICE Detainers Honored

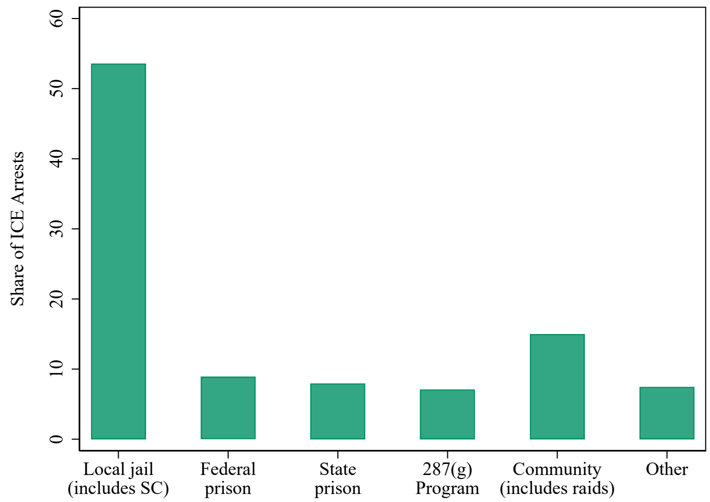



(b) Secure Communities (SC) Removals



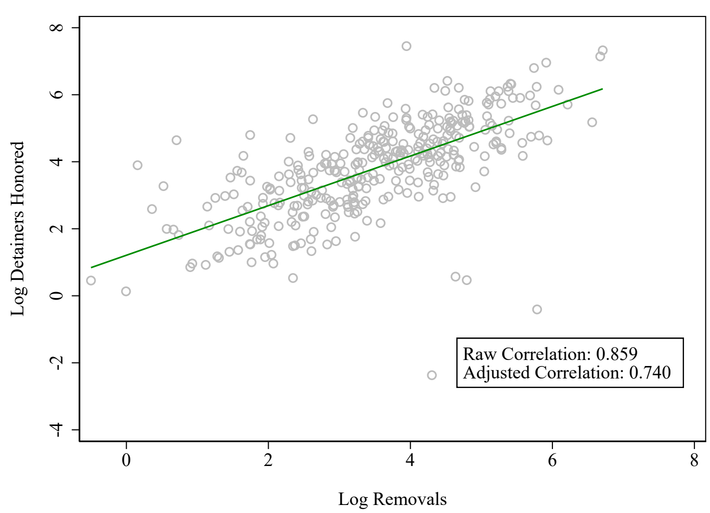

*Notes*: Panel (a) plots the number of monthly detainer requests honored by ICE using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. An honored detainer request refers to an ICE detainer request record that indicates that an individual was booked into detention. 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.3: Interior ICE Arrests by Source



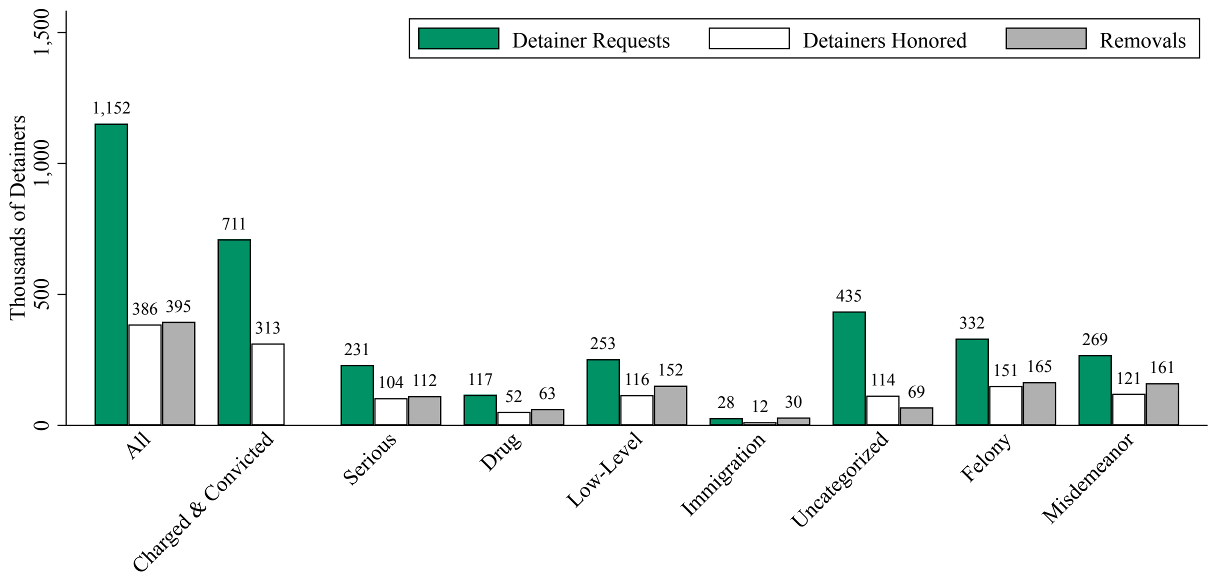

*Notes*: This figure plots the share of ICE interior arrests by source between 10/2008 and 8/2011 (Transactional Records Access Clearinghouse, 2018). "Local" refers to individuals arrested by local police or sheriff's offices. "State" and "federal" refers to individuals who were transferred to ICE after 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.4: 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).

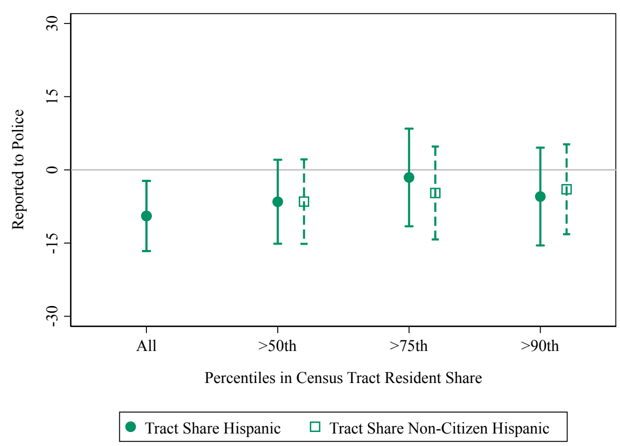
Figure A.5: ICE Detainer Requests, Detainers Honored, and Removals, by Offense Type



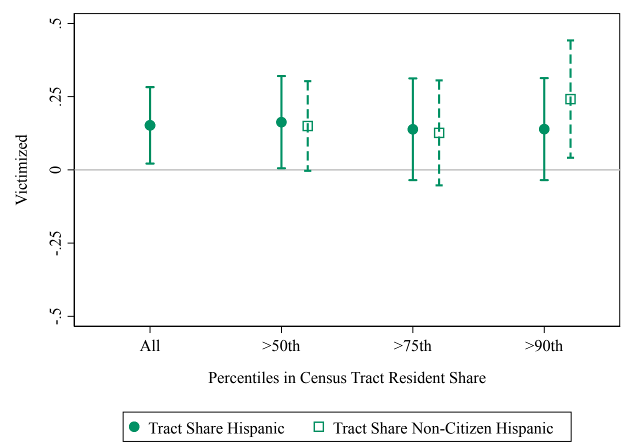

*Notes*: This figure shows the total number of ICE detainer actions by offense type using data from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University. The data covers all actions after Secure Communities (SC) was implemented in each county from 10/2008 through 12/2014. A detainer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

Figure A.6: Results by Neighborhood Resident Characteristics

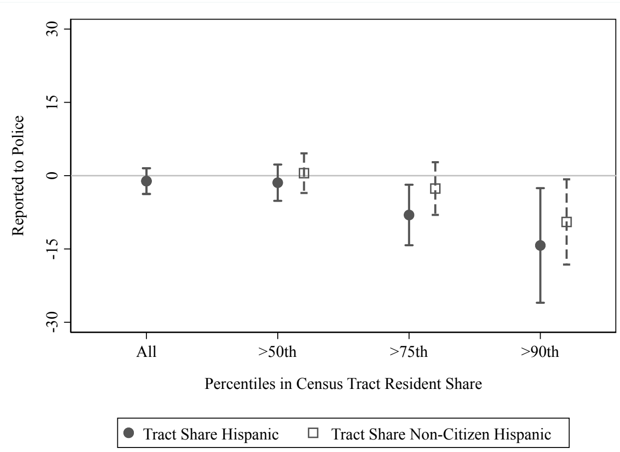
(a) Reported to Police, Hispanic



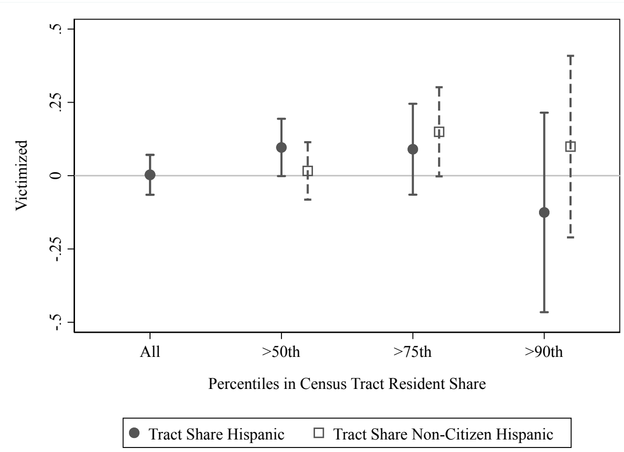

(b) Victimized, Hispanic




(c) Reported to Police, Non-Hispanic



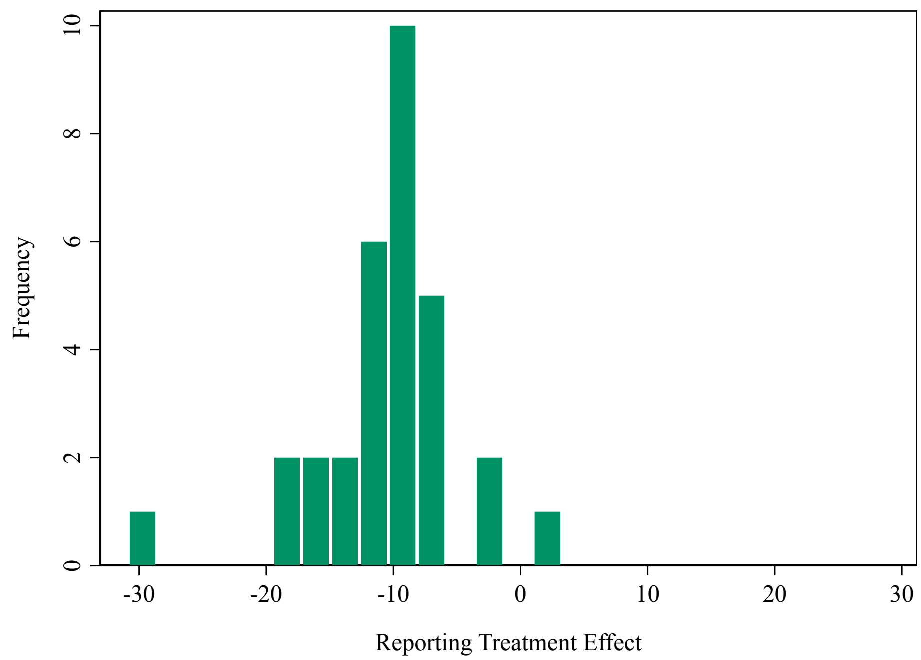

(d) Victimized, Non-Hispanic



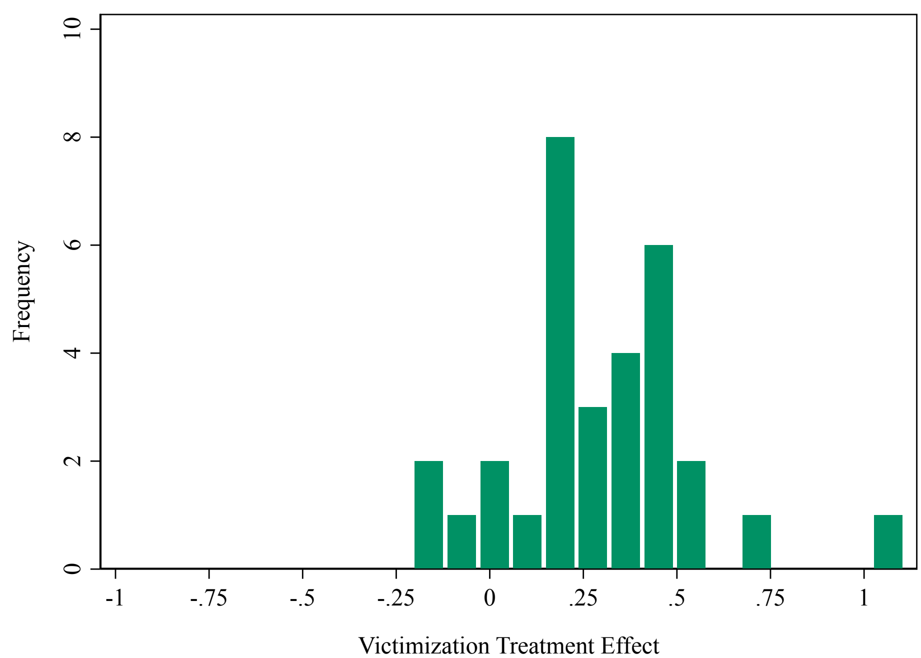

NOTE: This figure plots the estimates from equation (2) for subsamples of Hispanic and non-Hispanic survey respondents according to the share of their neighborhood that is Hispanic or non-citizen Hispanic. Data on neighborhood resident shares come from the 2000 Census and are linked to the NCVS based 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.7: 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.

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 (2021a) (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 (2023a,b). 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)

| | $\beta_{\text{Post}}$ | (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) on Reported Crime Rates using FBI Uniform Crime Reports

| | Index Crime | | Homicide | | Other Violent | | Property | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\beta_{\text{Post}}$ | -4.636 | -2.317 | -0.005 | 0.003 | -1.549** | -0.186 | -3.081 | -2.134 |
| | (4.451) | (4.061) | (0.023) | (0.021) | (0.696) | (0.456) | (4.140) | (3.806) |
| Percent Effect | -1.19% | -0.60% | -0.81% | 0.44% | -3.10% | -0.37% | -0.91% | -0.63% |
| Y Mean | 388.84 | 388.84 | 0.66 | 0.66 | 49.91 | 49.91 | 338.27 | 338.27 |
| Observations | 13,098 | 13,098 | 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 $\beta_{\text{Post}}$ 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, homicide, violent (excluding homicide), 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), (6), and (8) include agency-specific linear time trends. Estimates are weighted using the 2000 agency population. "Y Mean" refers to the average of the outcome variable in that specification. Standard errors are clustered at the county level.

Table A.4: Effect of Secure Communities (SC) on Reason for *Not Reporting* Crime for Hispanic Individuals

| | $\beta_{\text{Post}}$ | (S.E.) | Y Mean |
|---|---|---|---|
| **A. Reported to Police** | | | |
| Reported to Police | -9.446** | (3.664) | 30.98 |
| **B. *Did Not Report* and Gave Following Reasons** | | | |
| SC Policy Related Reason *(e.g., Fear, Mistrust, Offender Concern)* | 8.267*** | (3.044) | 25.30 |
| Other Reason | 1.178 | (3.086) | 43.10 |

Note: *p<0.1, **p<0.05, ***p<0.01. This table reports difference-in-differences estimates using equation (2) among crime incidents committed against Hispanic victims, examining different reasons for not reporting a crime. The estimate $\beta_{\text{Post}}$ and standard error correspond to an indicator variable equal to one in the eight quarters following the implementation of the SC program. Panel (a) reproduces the main result from Table 2 on the likelihood of reporting a crime to police. Panel (b) studies the reasons for not reporting a crime to the police provided by survey respondents. "SC Policy Related Reason" includes the following nine reasons: (1) police wouldn't think it was important enough, wouldn't want to be bothered or get involved; (2) police would be inefficient, ineffective; (3) police would be biased, would harass/insult respondent, cause respondent trouble; (4) offender was police officer; (5) respondent did not want to get offender in trouble with the law; (6) respondent was advised not to report to police; (7) respondent was afraid of reprisal by offender or others; (8) respondent did not want to or could not take time — too inconvenient; or (9) other reason. "Other Reason" refers to all other provided reasons, which fall within the broader categories of "dealt with another way", "not important enough to respondent", "insurance wouldn't cover", "police couldn't do anything", or unknown reason/missing respondent. This table considers the baseline sample of survey respondents and uses individuals in later-treated counties as the control group for estimating the treatment effects of SC on the outcomes of individuals in earlier-treated counties (Sun and Abraham, 2021). Estimates are weighted using NCVS person weights to maintain sample representativeness. "Y Mean" refers to the share of crimes reported in panel (a). In panel (b), "Y Mean" is the pre-SC program share of crimes not reported for a given reason group. All outcomes are multiplied by 100 for ease of exposition (scale 0 to 100). Standard errors are clustered at the county level.

Table A.5: Effect of Secure Communities (SC)
for Hispanic Individuals, by Crime Type

| | $\beta_{\text{Post}}$ | (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 $\beta_{\text{Post}}$ 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.6: Relationship between County-Level Characteristics and Immigration Enforcement, Victimization, & Reporting

| | ICE Removals | | Victimized | | | Reported to Police | | |
|---|---|---|---|---|---|---|---|---|
| | Per Capita | Felony Share | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\beta_{\text{Post}}$ | | | 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): $\beta_{\text{Post}}$ 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 ($\beta_{\text{Post}}$) 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.

# B Robustness Tests

In this appendix, we present a series of exercises probing the robustness of the main results: the decline in reporting and the increase in victimization among Hispanic individuals.

## B.1 Robustness of Sample and Specification

We first test the sensitivity of the results to sample construction choices, and report the findings for Hispanic individuals in Table B.1 and Figure B.1. In our baseline sample, we follow Alsan and Yang (2024) and exclude Illinois, Massachusetts, and New York given that these states actively resisted implementation of SC. In row (2), we include these three states and shift the county population threshold from 100,000 to 75,000.[1] The results are comparable, although smaller in magnitude than those from our baseline sample, consistent with the fact that these additional states resisted SC, and thus their Hispanic residents were likely less responsive to the policy's implementation. In row (3), we keep the original set of states but shift the population threshold to 50,000 and likewise find similar results.

The baseline empirical approach follows Sun and Abraham (2021) and uses the last 25% of counties that activated SC as the comparison group for earlier-treated counties. In practice, this empirical strategy restricts the sample frame, only considering time periods before September 2011 (when the comparison group begins to be treated). We test the sensitivity of the main results to this restricted time frame by using the final 10% of counties that activated the program as a control group, thus extending the sample window to March 2012 (row (4)). We also estimate a standard 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 B.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 a control variable (row (8)).[2] These results are likewise highly similar to the baseline findings.

As an additional test, we adjust crime outcomes for location-specific time trends, a procedure that is common in the economics of crime literature.[3] During the time period

[1] 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.

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

[3] These controls are likewise employed in the literature studying the impact of Secure Communities on reported crime. See, e.g., Miles and Cox (2014) or Treyger et al. (2014).

of our study, there were strong secular decreases in crime, and these declines may have been more pronounced in populous places, potentially because of technological advances in policing in better resourced departments (Garicano and Heaton, 2010; Ponce de Leon and Rizzotti, 2024). More populous counties were also more likely to be treated earlier (Table A.1), making them more likely to be a part of our treated group. In row (9), we add county-specific linear time trends to our baseline specification to address this concern — that secular trends may differ in ways that could be coincidentally correlated with the timing of program implementation — and find results that are nearly identical to our baseline findings.

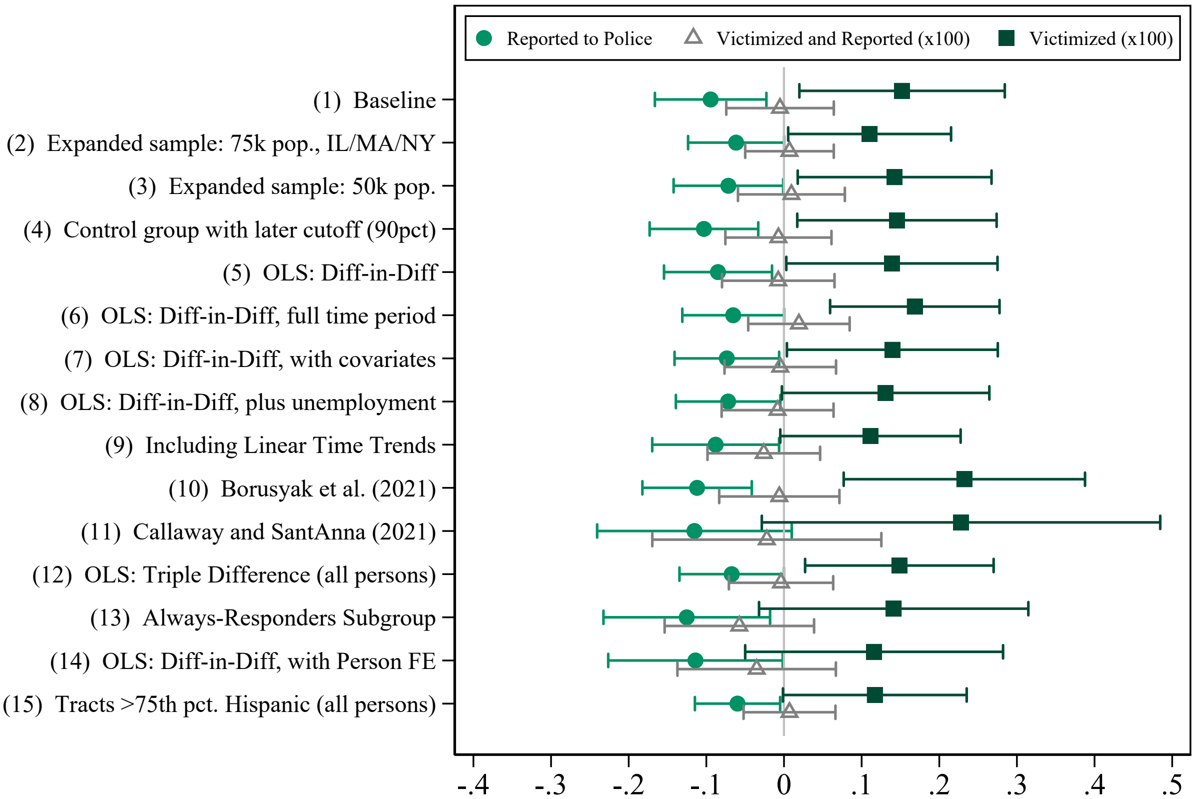
Further, we test the sensitivity of the results to alternative difference-in-differences models following Borusyak et al. (2024) and Callaway and Sant'Anna (2021) (rows (10) and (11)). We also estimate an OLS triple-difference specification in which we additionally leverage differences between Hispanic and non-Hispanic respondents, considering the latter as a control group (row (12)).[4] Across these strategies, we continue to find a significant decline in reporting and increase in victimization among Hispanic respondents. Figure B.3 displays the event-study coefficients from these alternative strategies, showing that the dynamic evolution of outcomes is comparable across these specifications.

Table B.2 and Figure B.2 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 B.4 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.

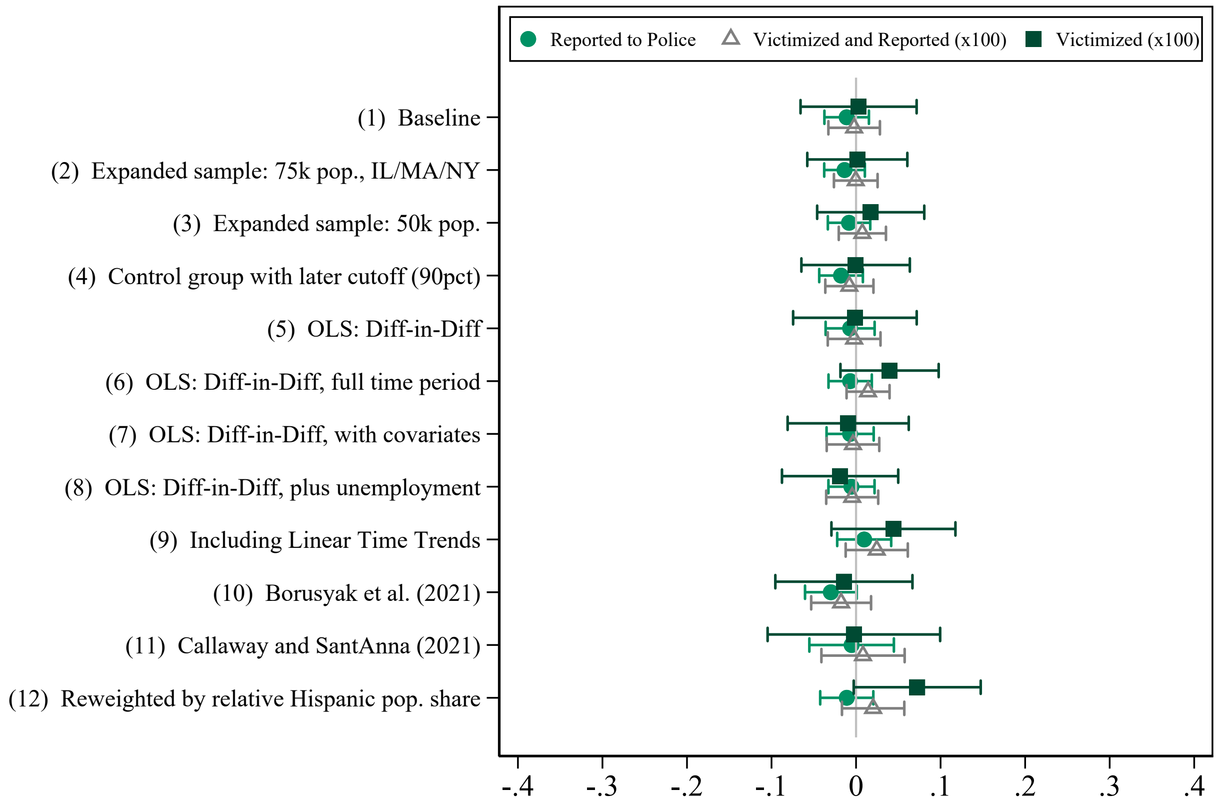
[4] To preserve statistical power, this specification controls for "cohort" groups of counties based on the year-month of SC activation instead of individual counties (i.e., using Hispanic×time, cohort×time, and cohort×Hispanic fixed effects).

Figure B.1: Robustness of Main Results, Hispanic Respondents




Note: This figure reports variants of $\beta_{\text{Post}}$ 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 (2023a,b). (9) includes county-specific linear time trends within the baseline model. (10) and (11) replicate the analysis using Borusyak et al. (2024) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (12) 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). (13) restricts attention to households that responded to the NCVS survey in each wave ("always-responders"). (14) re-estimates (5), including person fixed effects. (15) 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 B.1.

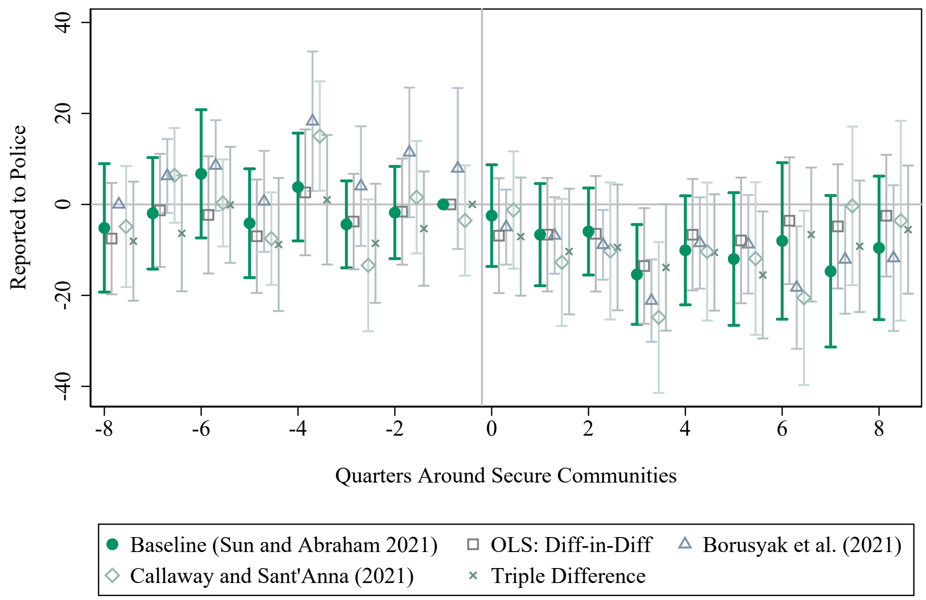
Figure B.2: Robustness of Main Results, Non-Hispanic Respondents



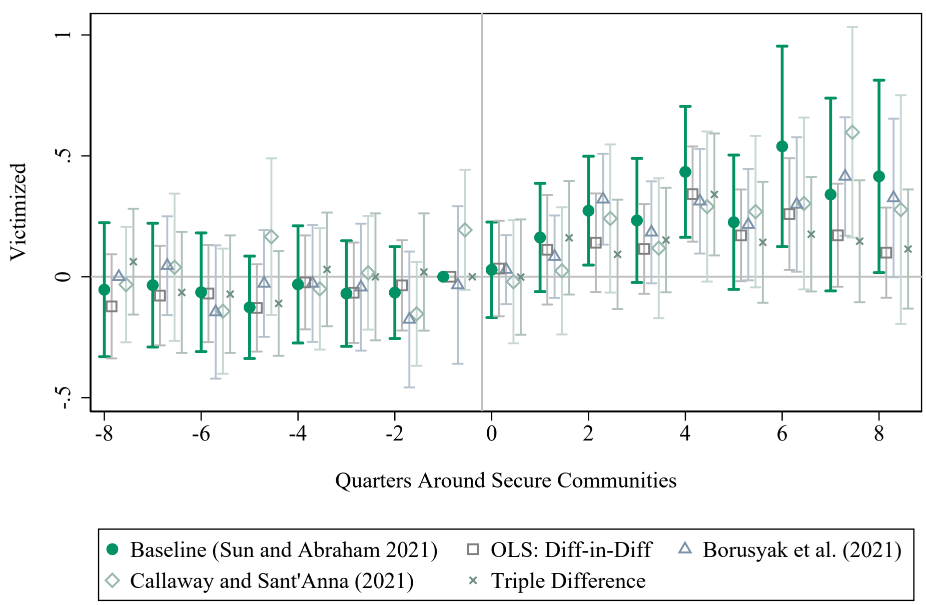

Note: This figure reports variants of $\beta_{\text{Post}}$ from equation (2) for the non-Hispanic sample. The bars refer to the 95% confidence interval for the two-year post-period estimate of the Secure Communities (SC) program. (1) reproduces the baseline model using Sun and Abraham (2021). (2) and (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 (2023a,b). (9) includes county-specific linear time trends within the baseline model. (10) and (11) replicate the analysis using Borusyak et al. (2024) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (12) 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 B.2.

Figure B.3: Robustness of Event-Study Results, Hispanic Respondents

(a) Reported to Police



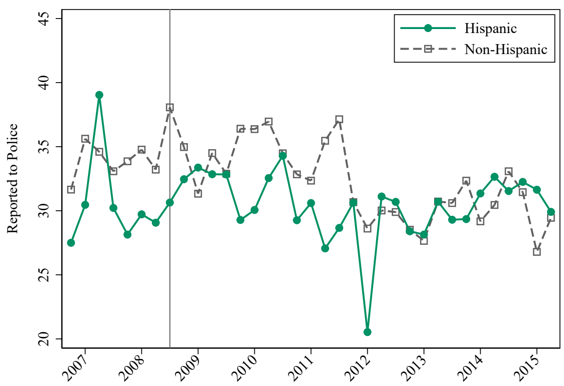

(b) Victimized



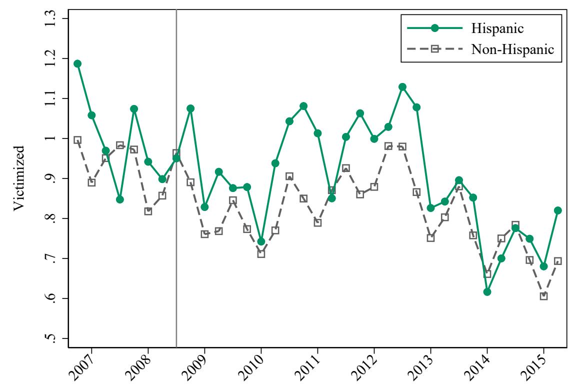

NOTE: This figure reports variants of $\beta_{\text{Post}}$ from equation (3) for the Hispanic sample, with bars indicating 95% confidence intervals from standard errors clustered at the county level. The figures plot the baseline model estimates alongside the standard OLS difference-in-difference, a 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 B.4: Share of Crimes Reported to Police, Victimization Rates, and Reported Crime Rate In Calendar Time

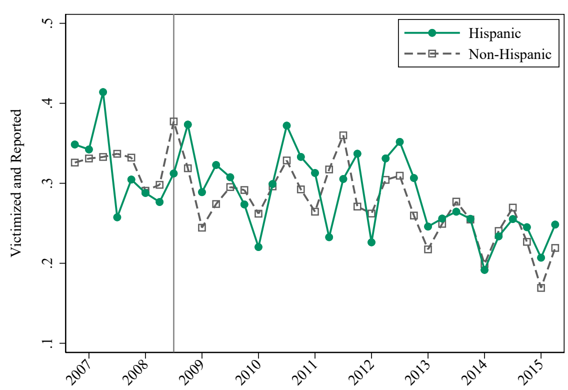
(a) Reported to Police




(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.

Table B.1: Robustness of Main Results, Hispanic Respondents

| | Victimized | | | Reported to Police | | | Victimized & Reported | | |
|---|---|---|---|---|---|---|---|---|---|
| | $\beta_{\text{Post}}$ | (S.E.) | Y Mean | $\beta_{\text{Post}}$ | (S.E.) | Y Mean | $\beta_{\text{Post}}$ | (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) Including Linear Time Trends | 0.111* | (0.059) | 0.96 | -8.799** | (4.167) | 31.00 | -0.026 | (0.037) | 0.31 |
| (10) Borusyak et al. (2021) | 0.232*** | (0.079) | 0.97 | -11.190*** | (3.593) | 30.29 | -0.006 | (0.040) | 0.31 |
| (11) Callaway and SantAnna (2021) | 0.228* | (0.131) | 0.97 | -11.530* | (6.393) | 30.29 | -0.022 | (0.075) | 0.31 |
| (12) OLS: Triple Difference (all persons) | 0.149** | (0.062) | 0.85 | -6.733* | (3.434) | 32.21 | -0.004 | (0.034) | 0.28 |
| (13) Always-Responders Subgroup | 0.141 | (0.088) | 0.81 | -12.520** | (5.475) | 31.45 | -0.057 | (0.049) | 0.26 |
| (14) 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 |
| (15) 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 (2023a,b). (9) includes county-specific linear time trends within the baseline model. (10) and (11) replicate the analysis using Borusyak et al. (2024) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (12) 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). (13) restricts attention to households that responded to the NCVS survey in each wave ("always-responders"). (14) re-estimates (5), including person fixed effects. (15) 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 B.2: Robustness of Main Results, Non-Hispanic Respondents

| | Victimized | | | Reported to Police | | | Victimized & Reported | | |
|---|---|---|---|---|---|---|---|---|---|
| | $\beta_{\text{Post}}$ | (S.E.) | Y Mean | $\beta_{\text{Post}}$ | (S.E.) | Y Mean | $\beta_{\text{Post}}$ | (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) Including Linear Time Trends | 0.044 | (0.037) | 0.87 | 0.962 | (1.629) | 34.50 | 0.024 | (0.019) | 0.31 |
| (10) Borusyak et al. (2021) | -0.014 | (0.041) | 0.86 | -2.978* | (1.561) | 34.07 | -0.018 | (0.018) | 0.30 |
| (11) Callaway and SantAnna (2021) | -0.003 | (0.052) | 0.86 | -0.524 | (2.550) | 34.07 | 0.008 | (0.025) | 0.30 |
| (12) 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 (2023a,b). (9) includes county-specific linear time trends within the baseline model. (10) and (11) replicate the analysis using Borusyak et al. (2024) and Callaway and Sant'Anna (2021), respectively, and the full sample period. (12) 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.

## B.2 Sample Attrition

One concern with using survey data is that response rates could be 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 (a "chilling effect" in survey response), then the observed increase in victimization could reflect a compositional change among respondents rather than a true increase in victimization.

The NCVS's sample design allows us to directly assess this possibility. As described in Appendix D, the NCVS contacts a fixed set of addresses in each 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. Because we do not know a household's Hispanic composition if they do not respond, we use the household address to perform versions of this analysis in Census tracts with increasingly larger Hispanic population shares (all households and then restricting to tracts above the 50th, 75th, and 90th percentile of the tract-level Hispanic share distribution in the NCVS data). In all samples, we find small and statistically insignificant coefficients for SC's impact on response rates, indicating no change in household response rates after the program's rollout, even in areas with a large Hispanic presence (Table B.3).[5]

Despite the reassuring evidence of minimal attrition, we use the point estimates from these regressions to conduct a back-of-the-envelope calculation of the potential bias from response rate changes, 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 corresponding 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 response rate bias cannot explain the observed victimization increase. We note that this exercise is quite conservative: it assumes that all non-respondents are Hispanic *and* not victimized, and that SC materially affects response rates, despite all estimates being statistically insignificant.

Next, we leverage the panel nature of the NCVS to focus on subsets of survey respondents. Specifically, we restrict the sample to Hispanic respondents present at each of their interviews and thus do not leave the survey (row (13) of Table B.1 and Figure B.1).[6]

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

[6] The NCVS samples addresses for 3.5 years, so that the same person responds to the survey

We also estimate an OLS model with person 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) in row (14). Both checks yield point estimates similar to our baseline, though the victimization effects lose significance due to 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.

A related concern could be that Hispanic individuals may be less likely to self-identify as Hispanic in the survey after the program's implementation, an effect that has been found in other contexts (Duncan and Trejo, 2011). We conduct two exercises to consider ethnic re-classification.

First, we estimate the victimization and reporting effects on *all* 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 (15) of Table B.1 and Figure B.1). We continue to find a 0.12 p.p. increase in victimization (p-value= 0.05) and 6 p.p. decline in reporting (p-value= 0.03) among these respondents, indicating that changes in reported ethnicity are not driving the results.

Second, we estimate a regression similar to equation (2) to quantify changes in the outcome of whether a respondent self-identifies as Hispanic. We find a 3% decline in the likelihood that respondents identify as Hispanic (Panel B of Table B.3). 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.[7]

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.

[7] These findings in some ways contrast with prior work documenting the "chilling" effects of immigration enforcement on public program participation (Alsan and Yang, 2024; Santillano et al., 2020; Watson, 2014). We note a few features of the NCVS that may reconcile its high response rates and the minimal survey attrition we observe: interviews may be conducted in Spanish and field representatives stress the confidentiality of the survey, which likely reduces the perceived risk of responding to this instrument. https://bjs.ojp.gov/media/video/68736.

Table B.3: Effect of Secure Communities (SC) on Survey Response Rates and Self-Reported Ethnicity

| | $\beta_{\text{Post}}$ | (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.

### B.3 Changes in Sample Composition

Next, we address a concern closely related to sample attrition, or changes in the composition of the sample. 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 B.1, we re-estimate the main specification including respondent characteristics, such as age, gender, and educational attainment, and the estimates are nearly identical to the baseline effects. The robustness of the results to the inclusion of these characteristics suggests that respondent and victim composition are uncorrelated with treatment.

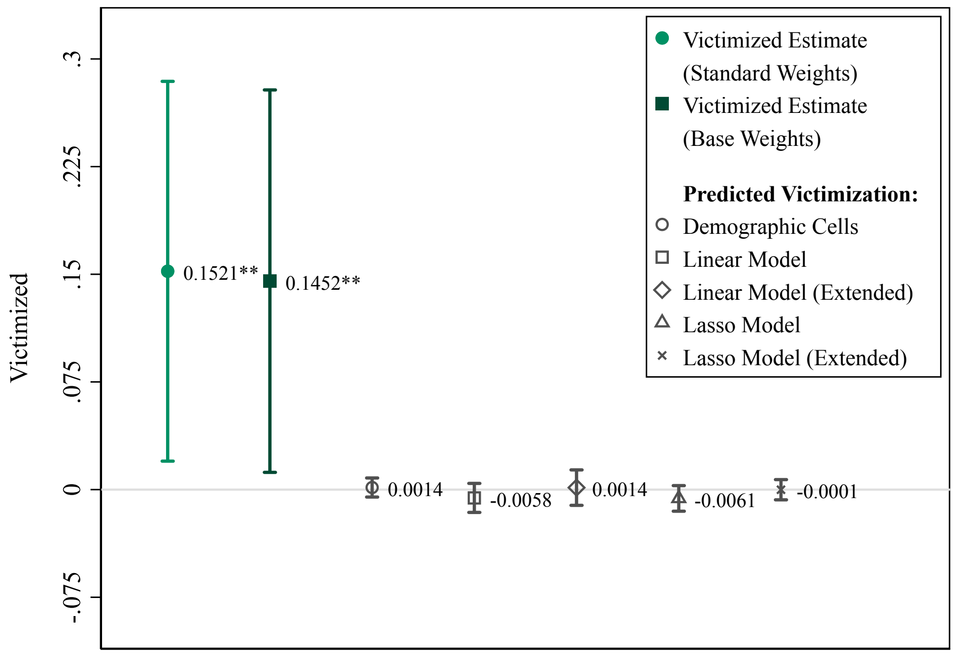
Next, we conduct two parallel exercises to probe this issue further. First, we construct measures of *predicted victimization* for each survey respondent based on their demographic characteristics and the victimization patterns prior to SC, during the period of 2005–2007. We then re-estimate equation (2) using predicted victimization as the dependent variable. Figure B.5 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.[8]

Likewise, we consider whether the policy may have impacted the composition of victims or crime incidents. In particular, if the composition of crimes changes to include crimes with lower reporting rates, the decline in reporting could be due to compositional changes in crime incidents, rather than behavioral responses in willingness to report crime.[9] 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. Figure B.6 shows 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.

[8] Standard NCVS weights include a survey attrition adjustment; Figure B.5 also displays an estimate that uses alternative weights that do not adjust for attrition (results are nearly identical). Further, the baseline results for Hispanic and non-Hispanic samples are robust to using these alternative weights or no survey weights, instead of the standard NCVS person weights.

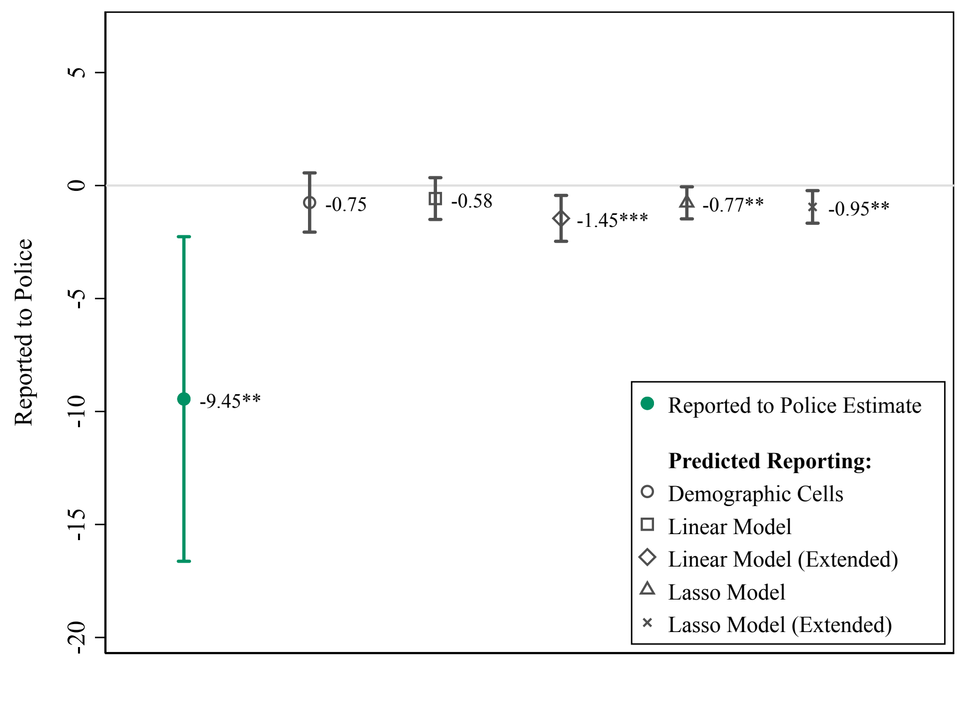
[9] Note that we find large, significant reporting declines when focusing on "always-responders" and when using individual-fixed effects (Table B.1), ruling out that the effect is driven by respondents with lower reporting rates entering the survey after SC.

Figure B.5: 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 B.6: 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.

# C Conceptual Framework

We present here our baseline conceptual framework with two groups: potential offenders and potential victims. For simplicity, we begin by assuming that all individuals are Hispanic non-citizens 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, $\delta p_D$, 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:

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

We model the Secure Communities program as 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:

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

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, $C = rO$, 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:

$$\frac{dO/O}{dp_D} \Bigg/ \frac{dr/r}{dp_D} = \epsilon_{O,r} + \frac{\epsilon_{O,p_D}}{\epsilon_{r,p_D}}, \tag{C.1}$$

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, 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 non-Hispanics 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 (C.1) (or equivalently, equation (1) in the main text), that the ratio of program impacts on offending and reporting provides a biased-upwards estimate of the impact of reporting on offending.

### C.1 Changes in Police Effectiveness

In the baseline framework, we have assumed that the probability of arrest $a$ among reported crimes (or the clearance rate) 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. Additionally, if victims are relatively less likely to report non-serious crimes following SC, the composition of *reported crimes* could become relatively more severe; because law enforcement tends to devote more resources to more severe crimes, $a$ could increase due to a change in the composition of reported crimes.

Allowing $a$ to change in response to $p_D$, we see:

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

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:

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

Allowing for impacts on police effectiveness changes equation (C.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 7, multiple pieces of evidence point against police effectiveness changing in response to Secure Communities.

### C.2 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 $\mu_c$, 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,

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

Written in elasticity form, we have:

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

Note that, with the possibility that deportation risk changes the mean cost of offending, the ratio of program impacts on offending and reporting is no longer necessarily a biased-upwards estimate of the impact of reporting on offending, $\epsilon_{O,r}$. However, as we show in Section 7, 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.

### C.3 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 incident, 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 $\lambda$ of new potential offenders who enter the economy. In addition, a share of non-deported individuals, $\theta$, exit the economy. If there are $N_t$ potential offenders in period $t$, the following period's number can be represented as

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

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.[10] 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 ($\frac{dOrap_D}{dp_D} > 0$).

Allowing for offender incapacitation does not change our result that the ratio of program impacts provides an upper bound on the offending-to-reporting effect. To see this fact, consider how $NO$ responds to a change in reporting:

$$\frac{\partial NO}{\partial r} = \frac{\partial N}{\partial r}O + \frac{\partial O}{\partial r}N$$

Next, consider how $NO$ responds to the policy change, which contains the reporting impact and a direct deterrence effect:

$$\begin{aligned}\frac{dNO}{dp_D} &= \frac{dN}{dp_D}O + \frac{dO}{dp_D}N \\ &= \Big[\frac{\partial N}{\partial p_D} + \frac{\partial N}{\partial r}\frac{\partial r}{\partial p_D}\Big]O + \Big[\frac{\partial O}{\partial p_D} + \frac{\partial O}{\partial r}\frac{\partial r}{\partial p_D}\Big]N \\ &= \frac{\partial NO}{\partial r}\frac{\partial r}{\partial p_D} + \Big[\underbrace{\frac{\partial N}{\partial p_D}O}_{\leq 0} + \underbrace{\frac{\partial O}{\partial p_D}N}_{\leq 0}\Big]\end{aligned}$$

Thus, the ratio of program impacts provides an upward biased estimate of the offending-to-reporting effect:

$$\frac{dNO}{dp_D}\Big/\frac{\partial r}{\partial p_D} = \frac{\partial NO}{\partial r} + \frac{\frac{\partial N}{\partial p_D}O + \frac{\partial O}{\partial p_D}N}{\partial r/\partial p_D} \geq \frac{\partial NO}{\partial r}$$

### C.4 Changes in Benefits to Reporting

Returning to the baseline framework, we have modeled the benefits of reporting $b$ for Hispanic non-citizens 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

[10] 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.

high shares of immigrants — so that $\frac{\partial b}{\partial p_d} < 0$. Or, consider a scenario in which victims factor the well-being of offenders into their reporting decision, and are less likely to report an incident given the new, more severe punishment (deportation). In such cases, 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 $\delta D$ and $\frac{\partial b}{\partial p_d}$, so we expect the change in reporting to be ambiguously signed.

### C.5 Framework with Citizen and Non-Citizen Individuals

Until now, we have assumed that all victims and offenders are non-citizens. Here, we allow for a share of victims ($\alpha_c$) and offenders ($\gamma_c$) to be citizens. 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.

Here, we consider the population as split by non-citizens and citizens for ease of exposition. In practice, the risk of deportation is low for non-Hispanic individuals, and thus we could alternatively consider a grouping of Hispanic non-citizens versus all other individuals (Hispanic citizens plus non-Hispanics). Further, given the potential spillover costs of reporting for Hispanic victims who are citizens (discussed in the next extension), one could consider a more nuanced distinction, with two groups of offenders (non-citizen Hispanics and others) and two groups of victims (Hispanics and non-Hispanics). Throughout the paper, we focus on the difference between victimization and reporting outcomes across Hispanic victim ethnicity. The logic of this extension continues to apply given these potential alternative groupings.

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 potential victims who are citizens. Notice here that the reporting decisions for this group 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:

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

Just like before, the two expressions inside the brackets for non-citizen offenders have opposite sign, so the impact of this change 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.

Although offenders cannot target victims based on their ethnicity in this extension, this framework can generate ethnicity-specific victimization impacts if the two populations reside in different geographic areas.

Incorporating the presence of offenders who are citizens 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 immigrant 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.

### C.6 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-citizen responses).[11] 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, 2024). Such concerns for family members or neighbors could therefore also enter as an additional cost into individual reporting decisions.

Notationally, we denote the extra cost of reporting by $\eta p_D$, 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 $h < b - \eta p_D D$.

[11] In practice, the deportation risk to legal or authorized immigrant victims may also be close to zero, but the perception of this individual risk may be non-zero.

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 those who are Hispanic, 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 wrongfully ensnared in the immigration system. Here, we would also expect a larger decline in the aggregate reporting rate relative to the baseline framework.

### C.7 Offender Choice of Victim Ethnicity

So far, we have assumed that offenders have no ability to target victims based on their demographic characteristics.

In this extension, we allow offenders to target victims based on their ethnicity. For simplicity, we assume that all offenders are Hispanic non-citizens, so their utility from offending is directly impacted by a change in $p_D$. As noted in the paper, in practice, nearly all immigration enforcement is against Hispanic non-citizens, and thus we restrict attention to Hispanic non-citizen offenders who face the risk of deportation, while non-Hispanic offenders face minimal risk of deportation.

There are two groups of possible victims, who are denoted by their ethnic group, $R \in \{H, N\}$ for Hispanic and non-Hispanic. As before, we assume that Hispanic victims respond to Secure Communities by reporting less ($\frac{\partial r_H}{\partial p_D} < 0$), but non-Hispanic victims do not change their reporting behavior ($\frac{\partial r_N}{\partial p_D} = 0$). In addition, we continue to assume that police effectiveness does not change ($\frac{\partial a}{\partial p_D} = 0$).

Offenders do not directly observe the ethnicity of potential victims. At the point of choosing whether to offend, they face two potential victims, one Hispanic and one non-Hispanic. They have a "signal" of the ethnicities of each victim, which takes the form of "person A is Hispanic, person B is non-Hispanic" or vice versa. This signal is correct with probability $p \in [1/2, 1]$.

Offenders have three options to choose from: 1) not offend, 2) offend against the likely Hispanic victim, and 3) offend against the likely non-Hispanic victim.

The value of offending against group $R$ is $M_R - r_R ax - r_R a p_D D - c_R$, where $M_R$ and $r_R$ continue to be constants but vary by ethnic group, and $c_R$ is an offender-specific cost of offending against group $R$. Each potential offender draws cost $c_R$ from CDF $G_R(\cdot)$, and we assume these costs are independent across ethnic groups and offenders. For simplicity, we add the assumption that $G_R(\cdot)$ are symmetric distributions centered around zero, so $1 - G_R(v) = G_R(-v)$ for all $v \in \mathbb{R}$. We also assume that the cost accompanies the believed ethnic group rather than true ethnic group. In other words, if the offender targets the likely Hispanic, they pay $c_H$ cost even if the victim is actually non-Hispanic.

When an offender observes the *likely* Hispanic victim, the perceived benefit of offending against them is $pM_H + (1-p)M_N - (pr_H + (1-p)r_N)a(x + p_D D) - c_H$, which we shorten to $\tilde{V}_H - c_H$. Conversely, the perceived benefit of offending against the *likely* non-Hispanic is $pM_N + (1-p)M_H - (pr_N + (1-p)r_H)a(x + p_D D) - c_N$, which we shorten to $\tilde{V}_N - c_N$. The offender chooses to offend against likely Hispanic if

$$\tilde{V}_H - c_H \geq \tilde{V}_N - c_N \;\;\&\;\; \tilde{V}_H - c_H \geq 0$$

They instead choose to offend against likely non-Hispanics if

$$\tilde{V}_N - c_N \;>\; \tilde{V}_H - c_H \;\;\&\;\; \tilde{V}_N - c_N \;\geq\; 0$$

We will denote the probability/volume of offending against likely Hispanics by $\tilde{O}_H$ and likely non-Hispanics by $\tilde{O}_N$.

The probability of offending against likely Hispanics, $\tilde{O}_h$, can be calculated as follows:

$$\begin{aligned}
\tilde{O}_h &= Pr(\tilde{V}_H - c_H \geq \tilde{V}_N - c_N \;\;\&\;\; \tilde{V}_H - c_H \geq 0) \\
&= E_{c_H}\big[Pr(\tilde{V}_H - c_H \geq \tilde{V}_N - c_N \;\;\&\;\; \tilde{V}_H - c_H \geq 0 \mid c_H)\big] \\
&= E_{c_H}\big[Pr(-c_N \leq \tilde{V}_H - \tilde{V}_N - c_H \;\;\&\;\; c_H \leq \tilde{V}_H \mid c_H)\big] \\
&= E_{c_H}\big[G_N(\tilde{V}_H - \tilde{V}_N - c_H)\mathbb{I}[c_H \leq \tilde{V}_H]\big] \\
&= \int_{c_H \leq \tilde{V}_H} G_N(\tilde{V}_H - \tilde{V}_N - c_H) dG_H(c_H)
\end{aligned}$$

We can thus express the probabilities of offending against each likely ethnic group as:

$$\tilde{O}_H \;=\; \int_{c_H \leq \tilde{V}_H} G_N\big(\tilde{V}_H - \tilde{V}_N - c_H\big) dG_H(c_H), \qquad \tilde{O}_N \;=\; \int_{c_N \leq \tilde{V}_N} G_H\big(\tilde{V}_N - \tilde{V}_H - c_N\big) dG_N(c_N)$$

And volumes of offending against actual Hispanics, $O_H$, and actual non-Hispanics, $O_N$, are mixtures of these volumes:

$$O_H \;= p\tilde{O}_H + (1-p)\tilde{O}_N, \qquad O_N \;=\; p\tilde{O}_N + (1-p)\tilde{O}_H$$

How does offending against Hispanics change in response to Secure Communities, i.e., an increase in $p_D$?

The two relevant objects are the value of offending against likely Hispanics (relative to not offending), $\tilde{V}_H$, and the value of offending against likely Hispanics relative to offending against likely *non-Hispanics*, $\tilde{V}_H - \tilde{V}_N$. The change in $\tilde{V}_H$ captures the competing effects of lower reporting and greater deterrence:

$$\frac{d\tilde{V}_H}{dp_d} = \underbrace{-pr_H a(x+p_D D)\frac{\partial r_H}{\partial p_D}}_{\text{Lower Reporting} \uparrow \text{Crime}} \quad \underbrace{-(pr_H+(1-p)r_N)aD}_{\text{Deterrence} \downarrow \text{Crime}}$$

The change in $\tilde{V}_H - \tilde{V}_N$ also has ambiguous sign:

$$\begin{aligned}
\tilde{V}_H - \tilde{V}_N &= (2p-1)[(M_H - M_N) - (r_H - r_N)a(x+p_D D)] \\
\frac{d(\tilde{V}_H - \tilde{V}_N)}{dp_D} &= \underbrace{-(2p-1)a(x+p_D D)\frac{\partial r_H}{\partial p_D}}_{\geq 0\text{, from Lower Reporting}} \quad \underbrace{-(r_H - r_N)(2p-1)aD}_{\lesseqgtr 0\text{, Depending on } r_H - r_N}
\end{aligned}$$

The first expression above is positive and captures that a reduction in Hispanic reporting induces a relative increase in the value of targeting that group. The second term captures the direct effect of an increase in $p_D$ and is ambiguously signed. If $r_H > r_N$, i.e., Hispanic victims have a higher reporting rate, then an increase in $p_D$ induces a reduction in $\tilde{V}_H - \tilde{V}_N$. However, if $r_H < r_N$, the increase in $p_D$ induces an *increase* in $\tilde{V}_H - \tilde{V}_N$. Notice that the reporting effects for both $\tilde{V}_H$ and $\tilde{V}_H - \tilde{V}_N$ increase with $p$, the ease of targeting Hispanics.

With this extension, the impact of SC on Hispanic victimization thus depends on: 1) the relative magnitude of the direct deterrence effect of $p_D$ and the reporting decline $\partial r_H/\partial p_D$ effect (which determines the sign of $\frac{d\tilde{V}_H}{dp_D}$), 2) the relative magnitudes of $r_H$ and $r_N$, and 3) the ease of targeting Hispanics, captured by $p$.

How offending against non-Hispanics responds to SC depends on the same factors. Notice that, when $p$ is sufficiently high and $r_N \geq r_H$, offending by non-citizen offenders against non-Hispanics should unambiguously *decrease*, since offenders should switch to targeting Hispanics or simply reduce their offending.

Note again that our analysis here focuses on non-citizen offenders. For citizen offenders, there is no direct impact of a change in $p_D$, so all changes are due to $\partial r_H/\partial p_D$, how Hispanic victims change their reporting. Thus, offending against likely Hispanics (non-Hispanics) unambiguously increases (decreases).

**Bounds on Offending-to-Reporting Effect** — We want to now show that, under this extended model, our ratio of program impacts continues to be an upper bound for the effect of Hispanic victim reporting on Hispanic victimization. This proposition will require an additional assumption that, at the point from which the policy is occurring, reporting rates are equal across victim groups, $r_H = r_N$. While this assumption imposes a specific set of parameter values, our summary statistics in Table 1 indicate approximately similar average reporting rates between Hispanics and non-Hispanics and are thus consistent with these parameter values. We discuss below where this assumption is used for our result.

The reporting effect on victimization satisfies the following inequality:

$$\frac{dO_H/dp_D}{\partial r_H/\partial p_D} \geq \partial O_H/\partial r_H \quad \text{if} \;\; r_H = r_N$$

We will prove this inequality in several steps. First, we will show that $\frac{d\tilde{V}_H/dp_D}{\partial r_H/\partial p_D} \geq \partial \tilde{V}_H/\partial r_H$, $\frac{d\tilde{V}_N/dp_D}{\partial r_H/\partial p_D} \geq \partial \tilde{V}_N/\partial r_H$, and $\frac{d(\tilde{V}_H-\tilde{V}_N)/dp_D}{\partial r_H/\partial p_D} = \partial(\tilde{V}_H - \tilde{V}_N)/\partial r_H$. Then we will show $\frac{d\tilde{O}_H/dp_D}{\partial r_H/\partial p_D} \geq \partial \tilde{O}_H/\partial r_H$ and $\frac{d\tilde{O}_N/dp_D}{\partial r_H/\partial p_D} \geq \partial \tilde{O}_N/\partial r_H$, and then $\frac{dO_H/dp_D}{\partial r_H/\partial p_D} \geq \partial O_H/\partial r_H$.

First, $\tilde{V}_H$:

$$\begin{aligned}
\frac{d\tilde{V}_H}{dp_D} &= -\tilde{r}_H aD - pa(x + p_D D)\frac{\partial r_H}{\partial p_D} \\
\frac{\partial \tilde{V}_H}{\partial r_H} &= -pa(x + p_D D) \leq 0 \\
\Rightarrow \; \frac{d\tilde{V}_H/dp_D}{\partial r_H/\partial p_D} &= \frac{\partial \tilde{V}_H}{\partial r_H} - \frac{\tilde{r}_H aD}{\partial r_H/\partial p_D} \geq \frac{\partial \tilde{V}_H}{\partial r_H}
\end{aligned}$$

By the exact same derivation, we have $\frac{d\tilde{V}_N/dp_D}{\partial r_H/\partial p_D} \geq \partial \tilde{V}_N/\partial r_H$. Next, $\tilde{V}_H - \tilde{V}_N$:

$$\begin{aligned}
\frac{d(\tilde{V}_H - \tilde{V}_N)}{dp_D} &= -(2p-1)(r_H - r_N)aD - (2p-1)a(x + p_D D)\frac{\partial r_H}{\partial p_D} \\
\frac{\partial(\tilde{V}_H - \tilde{V}_N)}{\partial r_H} &= -(2p-1)a(x + p_D D) \\
\frac{d(\tilde{V}_H - \tilde{V}_N)/dp_D}{\partial r_H/\partial p_D} &= \frac{\partial(\tilde{V}_H - \tilde{V}_N)}{\partial r_H} - \frac{(2p-1)(r_H - r_N)aD}{\partial r_H/\partial p_D} = \frac{\partial(\tilde{V}_H - \tilde{V}_N)}{\partial r_H} \quad \text{if} \;\; r_H = r_N
\end{aligned}$$

What is the intuition for why we need the assumption of $r_H = r_N$ for the SC program impacts to give an upper bound on the offending-on-reporting effect on $\tilde{V}_H - \tilde{V}_N$? The reason is because, as the probability of deportation $p_D$ increases, non-citizen offenders are induced into shifting from the victim group with a higher reporting rate to the group with the lower reporting rate, which is captured by how $\tilde{V}_H - \tilde{V}_N$ changes. If Hispanics are the lower reporting rate group, the direct deterrence effect may actually *reduce* victimization by non-citizen offenders. This kind of shift is ruled out under the assumption that $r_H \geq r_N$. As we show below, this weak inequality must actually be an equality to rule out shifts in the opposite direction.

Next, we examine how $\tilde{O}_H$ changes:

$$\begin{aligned}
\frac{d\tilde{O}_H}{dr_H} &= G_N\big(-\tilde{V}_N\big)G'_H\big(\tilde{V}_H\big)\frac{\partial \tilde{V}_H}{\partial r_H} + \int_{c_H \le \tilde{V}_H} G'_N\big(\tilde{V}_H - \tilde{V}_N - c_H\big)\frac{\partial(\tilde{V}_H - \tilde{V}_N)}{\partial r_H} dG_H(c_H) \\
\frac{d\tilde{O}_H}{dp_D} &= G_N\big(-\tilde{V}_N\big)G'_H\big(\tilde{V}_H\big)\frac{d\tilde{V}_H}{dp_D} + \int_{c_H \le \tilde{V}_H} G'_N\big(\tilde{V}_H - \tilde{V}_N - c_H\big)\frac{d(\tilde{V}_H - \tilde{V}_N)}{dp_D} dG_H(c_H) \\
\frac{d\tilde{O}_H/dp_D}{\partial r_H/\partial p_D} &= G_N\big(-\tilde{V}_N\big)G'_H\big(\tilde{V}_H\big)\frac{d\tilde{V}_H/dp_D}{\partial r_H/\partial p_D} + \int_{c_H \le \tilde{V}_H} G'_N\big(\tilde{V}_H - \tilde{V}_N - c_H\big)\frac{d(\tilde{V}_H - \tilde{V}_N)/dp_D}{\partial r_H/\partial p_D} dG_H(c_H) \\
&\ge G_N\big(-\tilde{V}_N\big)G'_H\big(\tilde{V}_H\big)\frac{\partial \tilde{V}_H}{\partial r_H} + \int_{c_H \le \tilde{V}_H} G'_N\big(\tilde{V}_H - \tilde{V}_N - c_H\big)\frac{\partial(\tilde{V}_H - \tilde{V}_N)}{\partial r_H} dG_H(c_H) \quad \text{if} \quad r_H = r_N \\
&= \frac{d\tilde{O}_H}{dr_H}
\end{aligned}$$

Next, we prove the same inequality for $\tilde{O}_N$:

$$\begin{aligned}
\frac{d\tilde{O}_N}{dr_H} &= G_H\big(-\tilde{V}_H\big)G'_N\big(\tilde{V}_N\big)\frac{\partial V_N}{\partial r_H} + \int_{c_N \le \tilde{V}_N} G'_H\big(\tilde{V}_N - \tilde{V}_H - c_N\big)\frac{\partial(\tilde{V}_N - \tilde{V}_H)}{\partial r_H} dG_N(c_N) \\
\frac{d\tilde{O}_N}{dp_D} &= G_H\big(-\tilde{V}_H\big)G'_N\big(\tilde{V}_N\big)\frac{d\tilde{V}_N}{dp_D} + \int_{c_N \le \tilde{V}_N} G'_H\big(\tilde{V}_N - \tilde{V}_H - c_N\big)\frac{d(\tilde{V}_N - \tilde{V}_H)}{dp_D} dG_N(c_N) \\
\frac{d\tilde{O}_N/dp_D}{\partial r_H/\partial p_D} &= G_H\big(-\tilde{V}_H\big)G'_N\big(\tilde{V}_N\big)\frac{d\tilde{V}_N/dp_D}{\partial r_H/\partial p_D} + \int_{c_N \le \tilde{V}_N} G'_H\big(\tilde{V}_N - \tilde{V}_H - c_N\big)\frac{d(\tilde{V}_N - \tilde{V}_H)/dp_D}{\partial r_H/\partial p_D} dG_N(c_N) \\
&\ge G_H\big(-\tilde{V}_H\big)G'_N\big(\tilde{V}_N\big)\frac{\partial \tilde{V}_N}{\partial r_H} + \int_{c_N \le \tilde{V}_N} G'_H\big(\tilde{V}_N - \tilde{V}_H - c_N\big)\frac{\partial(\tilde{V}_N - \tilde{V}_H)}{\partial r_H} dG_N(c_N) \quad \text{if} \quad r_H = r_N \\
&= \frac{d\tilde{O}_N}{dr_H}
\end{aligned}$$

We now have bounds on how the program impacts on the likely Hispanic and likely non-Hispanic victims relate to the reporting effects. Combining them gives us the desired bound:

$$\begin{aligned}
\frac{dO_H/dp_D}{dr_H/dp_D} &= p\frac{d\tilde{O}_H/dp_D}{dr_H/dp_D} + (1-p)\frac{d\tilde{O}_N/dp_D}{dr_H/dp_D} \\
&\ge p\frac{\partial \tilde{O}_H}{\partial r_H} + (1-p)\frac{\partial \tilde{O}_N}{\partial r_H} \\
&= \frac{\partial O_H}{\partial r_H}
\end{aligned}$$

Thus, we show that a change in the probability of deportation induced by Secure Communities provides a conservative estimate of the effect of reporting on offending, where offenders are non-citizens and victims are Hispanic.

The analysis above pertains to how non-citizen offenders respond to SC and how their ratio of program impacts is an upper bound on their offending-to-reporting effect. In the case of citizen offenders, they face $p_D = 0$, and the only impact of the policy for them is a change in $r_H$. Thus, the ratio of program impacts for citizen offenders is *equal* to their offending-to-reporting effect.

# D NCVS Sample Design

## D.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 nation'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).

## D.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.[12]

[12] "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 B.3 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.[13] 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.

[13] 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.

# E 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, which we denote as $ra$ in our Conceptual Framework, 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 3 shows that within the NCVS, the average likelihood of arrest among reported offenses, $a$, is 9%. If we combine this rate with the estimates in Table 2 on the change in victim reporting $r$, we conclude that the total apprehension risk, $ra$, 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 to measure changes in total apprehension risk, $ra$.[14] The first row of Table E.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 30% decline in reporting, providing evidence that the decline in reporting was the primary driver of the decrease in the overall apprehension rate.

Table E.1: Apprehension Risk Among All Victimizations

| | $\beta_{\text{Post}}$ | (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 $\beta_{\text{Post}}$ 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.

[14] This exercise differs from that in Table 3, which only considers *reported* victimizations and is thus quantifying changes in $a$.

## 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 being in non-border counties, in counties with $> 100,000$ residents in 2000, and outside of Massachusetts, Illinois, and New York.[15]

**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.

**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.

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

[15] Some cities span multiple counties. In these cases, we only keep tracts within counties that satisfy these sample restrictions.

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 so each observation is a Census tract in a given month. 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.1. 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.1: 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 Sections 7.2 and 7.3 as well as Appendix F.

## G Mediation Analysis of Victimization Increase

In this appendix, we provide further detail on the mediation analysis discussed in Section 7.4. 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. We then: confirm the robustness of our findings to alternative choices; compare the offending-to-reporting elasticity to similar estimates in the literature; compare the elasticity to an alternative approach that isolates the offending response of non-Hispanic offenders; and conduct a simple normative analysis comparing the cost-effectiveness of crime reduction via increased victim reporting relative to increased police force size.

### 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 $\kappa_d$ is an intercept term, and $\alpha_d$ 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 $\alpha^j$, measuring the effect of each mediator on victimization.

### G.2 Effect of Secure Communities 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 100,000 residents. We estimate that Secure Communities increased county-level deportations by 62%.[16]

**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:

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

where $Y_{ct}$ is an outcome variable for county $c$ at year $t$. $\mathrm{SC}_c$ is an indicator variable equal to one if county $c$ had implemented the Secure Communities program for at least half of year $t$. $\mu_c$ and $\delta_t$ 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 $\beta_{\text{Post}}$, 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.[17]

[16] Note that the estimated 62% impact on honored detainers is slightly different from our first stage estimate of 54% in Figure 1, 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.

[17] 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

We consider three outcomes that Secure Communities may have affected. Specifically, we estimate the effect of the program 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 Hispanics. We also consider the effect of Secure Communities on the share of Hispanic household heads that are female.

### G.3 Effect of Mediators on Crime

We borrow estimates from the literature on the impact of each mediator on crime and convert each estimate 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 to 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, the 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. The estimates imply an elasticity of crime to unemployment of -1.66 and an elasticity of crime to 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 to 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 G.2. The elasticities and corresponding sources are displayed in column 1. To calculate the implied levels 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

county-specific linear time trends from the specification.

SC's effect on the mediator (column 3) and the implied effect of the mediator on victimization (column 2). The estimates from column 4 are the ones depicted in Figure 4.

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 effect of victim reporting on victimization implied by our cohort-level regression.

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Table G.2: Decomposing Victimization Increase into Various Components

| *Mediator Variable* | Elasticity & Source | | Implied Effect on Victimization | Effect of SC (% Effect) | Predicted Effect on Victimization |
|---|---|---|---|---|---|
| | | | | Total Victimization Effect | 0.152 |
| | (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 mediation analysis outlined in Section 7.4 (also depicted in Figure 4). 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 to get the implied levels effect of the mediator on victimization. "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 3). For more details on these calculations, see Appendix G.

### 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 Secure Communities 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 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 male immigrant share of the population, who were the primary targets of the SC policy. 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.[18] We find a 3.34% decline in the male immigrant share.

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 the effect of this measure and that of deportations, our baseline analysis only considers direct changes in deportations.

### G.6 Alternative Approaches to Estimating the 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 (0.204) is quite similar to the baseline impact on victimization (0.152). Here, we show that, if we had used 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, we measure this object directly from our data. To do so, we use the estimates from the cohort-level analysis in Section 7.1: we calculate the implied elasticity of offending to report-

[18] Given time trends, we use a de-trended version of this outcome: 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).

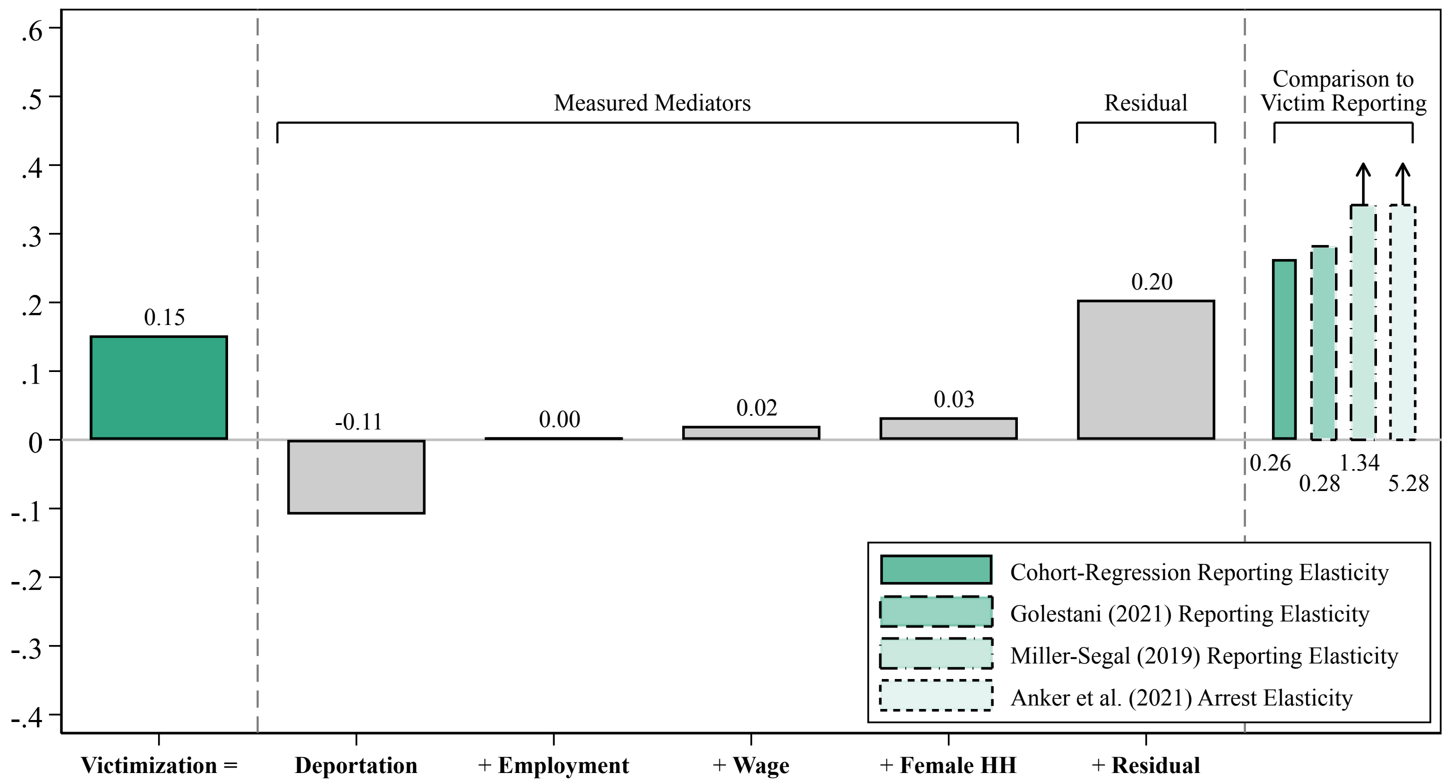
ing using variation across cohorts in the magnitude of effect sizes. We estimate that a 1 p.p. decline in victim reporting is associated with a 0.017 p.p. higher cohort-level victimization rate, leading to an implied elasticity of offending to reporting of -1.02. Interpreting our cohort-level analysis 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 G.1, 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 E, 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.

Figure G.1: Victimization Decomposition, Robustness to Alternative Elasticity Choices




*Notes*: This figure presents estimates from the mediation analysis outlined in Section 7.4. 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.

### G.7 Impacts of Secure Communities on Offending, by Offender Ethnicity

This section estimates offending impacts separately for Hispanic and non-Hispanic offenders. We will do so by combining our victimization estimates from the NCVS with our estimates of the Hispanic arrestee share from our multi-city micro data (described in Appendix F). This analysis allows us to calculate the elasticity of offending-to-reporting for non-Hispanic offenders, who face no deportation risk and thus respond solely to the victim reporting decline.[19] In other words, this exercise offers a complementary strategy to our mediation analysis for estimating the offending-to-reporting elasticity.

Let $Y_H$ ($Y_N$) be the pre-program monthly probability that a Hispanic civilian is victimized by a Hispanic (non-Hispanic) offender. From Figure 2, we know that $Y_H + Y_N = 0.0090$, the total rate of victimization among Hispanic civilians. We denote by $\Delta_H$ and $\Delta_N$ the SC impacts on the monthly probability that a Hispanic civilian is victimized, distinguishing again by offender ethnicity. Table 2 tells us that $\Delta_H + \Delta_N = 0.00152$.

We next use information from Table 4 on arrestee composition and how it changes with SC. To do so, we have to assume that the victim reporting rate $r$ and the police arrest rate $a$ do not depend on offender ethnicity. We also assume that the arrestee composition from tracts with a high Hispanic share (middle panel of Table 4) reflect the arrestee composition of offenders against Hispanic civilians. We have that $\frac{raY_H}{ra(Y_H+Y_N)} = \frac{Y_H}{Y_H+Y_N} = 0.473$, or the share of arrestees that are Hispanic. In addition, the decline in the Hispanic arrest share can be represented as $\frac{Y_H+\Delta_H}{Y_H+\Delta_H+Y_N+\Delta_N} - \frac{Y_H}{Y_H+Y_N} = -0.023$.

Therefore, our estimates have to satisfy a system of four equations in four unknowns:

$$\begin{aligned}
Y_H + Y_N &= 0.0090 \\
\Delta_H + \Delta_N &= 0.00152 \\
Y_H &= 0.473(Y_H + Y_N) \\
Y_H + \Delta_H &= (0.473 - 0.023)(Y_H + \Delta_H + Y_N + \Delta_N) = 0.45 * 0.01052 = 0.0047
\end{aligned}$$

These equations are solved by $Y_H = 0.0043$, $Y_N = 0.0047$, $\Delta_H = 0.0005$, and $\Delta_N = 0.0010$. These estimates imply that offending by Hispanic offenders against Hispanic civilians increased by 11.2%, and offending by non-Hispanic offenders against Hispanic civilians increased by 22.0%.

These results are consistent with the idea that the program's increase in deportation risk led to a smaller increase in Hispanic offending than non-Hispanic offending. In addition, dividing the 22.0% increase in non-Hispanic offending by the 30.5% reporting decline gives an elasticity of non-Hispanic offending with respect to reporting of $-0.72$. This number is remarkably close to our primary elasticity estimate of $-0.79$ using our mediation analysis.

[19] This exercise assumes that non-Hispanics experience no change in economic and social outcomes from Secure Communities.

### G.8 Comparison of Offending-to-Reporting Elasticity to Offending-to-Police Force Size Elasticity

In Section 7.5, we calculate an elasticity of offending to victim reporting across a range of assumptions. In our preferred specification, we estimate an elasticity of -0.79, meaning that a 10% (3.4 p.p. in our data) increase in victim reporting causes a 7.9% decline in offending.

In this section, we benchmark this elasticity to the well-studied elasticity of offending to police force size. Several studies have documented that an increase in police force size leads to a decline in crime (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019). Mello (2019) calculates an implied elasticity of -0.8 for property crimes and -1.3 for violent crimes. Taking the crime composition from that study's data (85% property crimes), these effects translate to an overall elasticity of -0.875. While these estimates are derived from data on *reported* offenses, they also apply to true offending under the assumption that victim reporting did not change in response to changes in police force size.

While this elasticity is similar in magnitude to ours, it is not clear that they are directly comparable. Unlike police employment, victim reporting is not a direct policy variable. We therefore aim to make these two elasticities more comparable by considering a hypothetical policy that raises victim reporting and asking: at what fiscal cost is this policy as cost-effective in lowering crime as spending on police employment?

To calculate the fiscal cost of lowering crime through police, we borrow estimates from Mello (2019). This study estimates that each additional police officer has a fiscal cost of $185,000. To reduce crime by 10% requires increasing police employment by 11.43% (10/0.875). Police employment in that study's sample is 22.85 officers per 10k capita, so this increase translates to 2.61 additional officers (per 10k capita). This increase in hiring amounts to $48.3 per capita.

To reduce crime by 10% through victim reporting requires a policy that raises reporting rates by 12.66%. Based on the reporting rate of 34.41% (Table 1), this translates to raising the reporting rate by 4.36 p.p. For this policy to be as cost-effective as increasing police employment, it thus must cost $48.3 per capita, or $11.09 per capita for each percentage point increase in the reporting rate.

We do not take a stance on whether the $11.09 per capita cost implies that interventions to improve victim reporting are cost effective, but this figure provides a useful benchmark for assessing future policy experiments with this aim.