Supplemental Appendix:

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

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B Conceptual Framework

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

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

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

If they commit an offense and are caught (which is a function of victim behavior, discussed below), they face two costs. First, is the standard punishment $x > 0$. Second, there is a probability p_D they are referred to immigration enforcement 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 has distribution $F(h) \in [0, 1]$. Reporting also includes a potential risk of being referred to immigration enforcement officials, δp_D, in which case the victim faces a punishment D. Here, $\delta \in [0, 1]$ indicates that a victim's likelihood of being referred to immigration enforcement may differ from that of offenders. This risk of being referred to immigration enforcement may be real or perceived. The value of not reporting is normalized to 0.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Extensions

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

Framework with Citizen Victims and Offenders — Until now, we have assumed that all victims and offenders are unauthorized. Here, we allow for a share of victims α_c to be citizens and for a share of offenders γ_c to be citizens.[1] For simplicity, we assume that offenders cannot choose whom to target, so they face a uniform reporting probability, and all victims face the same offending rate.

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

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

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

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

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

Analogously, the number of offenses is:

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

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

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

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

offenders and how the probability of reporting has changed:

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

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

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

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

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

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

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

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

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

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

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

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

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

Distribution of Offender Costs — We also assumed that offenders' cost of offending c are unchanged by a change in p_D. The Secure Communities program intensified fears of participating in formal labor markets (East et al., 2023), so it may induce a leftward shift in the distribution of c, acting as another driver of more offenses O.

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

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

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

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

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

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

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

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

Incorporating the number of potential offenders adds an additional margin along which changes in p_D could impact overall offending, NO. This margin is summarized by $Orap_D$, which is the number of individuals who are deported in a given period. It is now no longer the case that the change in total offending always has the same direction of response as the change in the "per capita" offending rate.[2] In particular, even if the offending rate is unchanged ($\frac{\partial O}{\partial p_D} = 0$), overall offending could still decline in response to the policy if the mass of potential offenders shrinks ($\frac{\partial Orap_D}{\partial p_D} > 0$).

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

C NCVS Sample Design

C.1 Overview of Survey

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

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

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

C.2 Survey Non-Response

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

The final type of non-response is a household nonresponse, which occurs when an interviewer arrives at the selected address but is not able to obtain an interview. This could occur — similarly to the person-level non-response — because the household is not home or is unwilling or unable to participate in the survey. However, this type of non-response could also occur for other reasons (e.g., if the living quarters are vacant or the address is no longer used as a residence). Interviews that do not occur despite the persons in the household being eligible for the interview are referred to by the Census Bureau as "Type A" interviews.[3] Field representatives are instructed to keep Type A interviews to a minimum

[3] "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

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

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

After the data collection, the Census Bureau creates weights to adjust the sample counts and correct for differences between the sample and population totals.[4] 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.

been demolished).

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

D County-Specific Treatment Effects

We estimate county-specific treatment effects from the Secure Communities program. In particular, we use equation (1) to estimate an effect for each earlier-treated county, using respondents in later-treated counties as a comparison group. We generate an estimate $\hat{\beta}_c$ and standard error $\hat{\sigma}_c$ for SC's impact in each county c for each outcome and ethnicity group. The empirical distribution of estimated effects includes noise from estimation error and thus may overstate the degree of variation in county-level impacts. We address this issue with a deconvolution procedure (Goncalves and Mello, 2021; Kline et al., 2022).

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

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

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

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

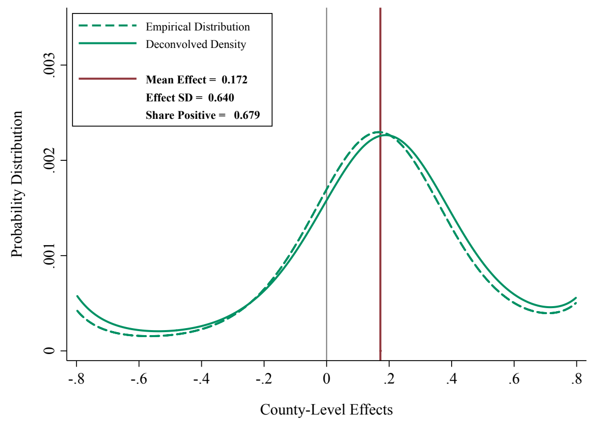
We estimate the distribution $f(\beta^k)$ using maximum likelihood, where we parametrize the effect distribution with a four-parameter exponential family distribution (Efron, 2016).[5] To produce the density of *estimated* effects, we conducted the same deconvolution procedure but divided all standard errors by 100. For Census disclosure purposes, we chose this procedure as an alternative to the standard kernel density approach, where each point in the distribution is estimated on a potentially small set of observations.

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

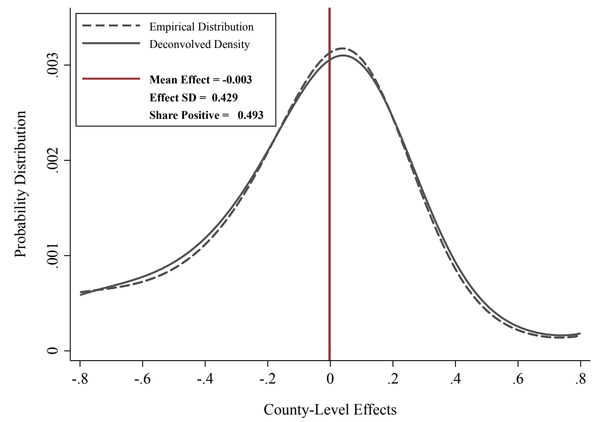
[5] We make the simplifying assumption here that the error terms ϵ_c are independent across counties. Although this assumption is likely to be violated (the same set of counties are used as controls for each treated county), we impose it for tractability, as it allows us to construct the log-likelihood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

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

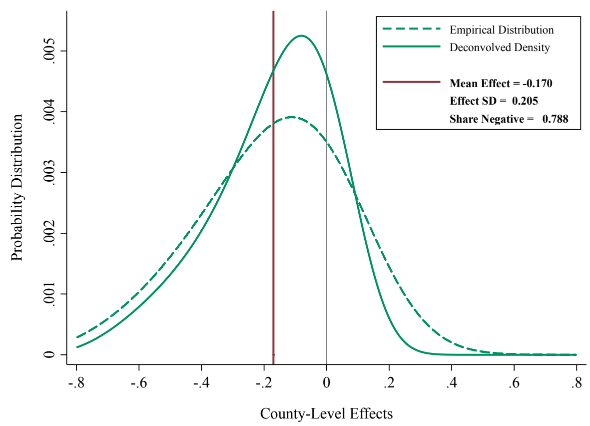
(a) Victimized, Hispanic



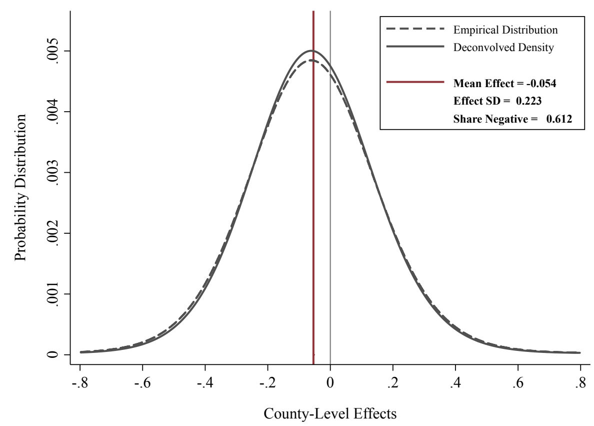
(b) Victimized, Non-Hispanic



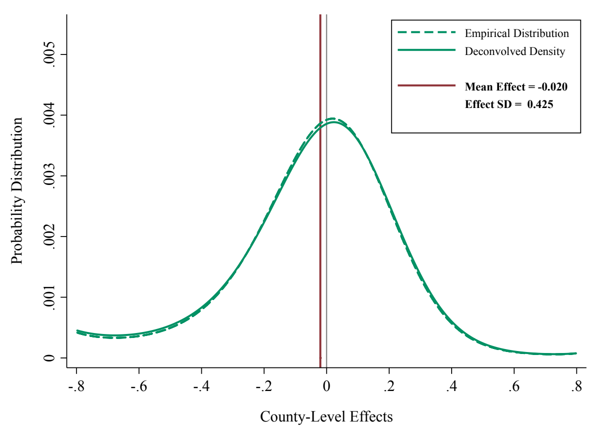
(c) Reported to Police, Hispanic



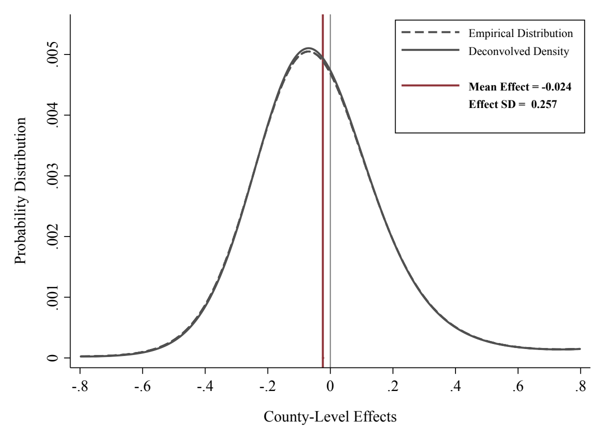
(d) Reported to Police, Non-Hispanic



(e) Victimized and Reported, Hispanic



(f) Victimized and Reported, Non-Hispanic



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

E Description of Police Administrative Data

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

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

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

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

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

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

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

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

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

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

Table E.1: Cities with Police Administrative Data

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

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

NOTE: This table lists cities with police administrative data, described in Section 8.2 and Appendix E.

F Mediation Analysis of Victimization Increase

In this appendix, we provide further detail on the decomposition exercise discussed in Section 8.3. 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.

F.1 Framework

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

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

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

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

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

$$\mathrm{E}[V_1 - V_0] = \underbrace{\sum_{j \in \mathcal{J}_p} \alpha^j \mathrm{E}[\theta_1^j - \theta_0^j]}_{\substack{\text{Treatment effect due} \\ \text{to observed mediators}}} + \underbrace{\mathrm{E}[\tau_1 - \tau_0]}_{\substack{\text{Treatment effect due} \\ \text{to unobserved mediators}}} \tag{1}$$

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

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

F.2 Effect of Secure Communities on Mediators

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 (1), as follows:

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

where Y_{ct} is an outcome variable for county c at year t. SC_c is an indicator variable equal to one if county c had implemented the Secure Communities program for at least half of year t. μ_c and δ_t correspond to county and year fixed effects, respectively. Standard errors are clustered at the county level and the model is weighted by the county's 2000 population. The coefficient of interest is β_{Post}, estimating the average difference in outcome Y in the two years after the implementation of the Secure Communities program relative to the difference in the outcome prior to the program's launch.[6]

In this exercise, we consider four outcomes that Secure Communities may have affected beyond Hispanic individuals' reporting behavior. Specifically, we estimate the effect of SC on the employment-to-population ratio and logged hourly wages of Hispanic low-educated foreign-born individuals; the share of Hispanic household heads that are female; and the population share of Hispanic low-educated foreign-born men, which is a proxy for the unauthorized immigrant share of the population.[7] Like East et al. (2023), we define this final outcome as the number of individuals in this group divided by the working-age population in that county in 2005. Given time trends, we use a de-trended version of this outcome: specifically, we implement a two-step method in which we first estimate a linear trend for each county using pre-period data only, and then we subtract the fitted trend from all of the county's data points (Goodman-Bacon, 2021).

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

[7] 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. For the immigrant population share, we additionally restrict our attention to males given the more marked decline in their population share.

F.3 Effect of Mediators on Crime

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

For victim reporting, we rely on Golestani (2021), which studies nuisance property ordinances (NPOs) that increase the cost of contacting 911 for residents. This paper finds that NPOs lead to a -0.075 p.p. decline in victim reporting off a mean rate of 0.585 (Table 3) and a 0.21 p.p. increase in the likelihood of assault victimization off a mean of 1.5 (Table 6). These estimates imply an elasticity of assault victimization on reporting of -1.09.

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

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

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

F.4 Implied Effect of Mediators on Victimization

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

F.5 Robustness to Alternative Choices

A central conclusion from our decomposition is that the victim reporting decline is the primary driver of increased victimization. This conclusion, of course, relies on the validity of using Golestani (2021)'s estimate of how offending responds to changes in victim reporting.

In Appendix Figure A.15, we show how the decomposition calculation changes with alternative choices for the effect of victim reporting on offending. We keep the same estimates for the non-reporting mediators, so their predicted impacts on victimization are unchanged.

The leftmost bar for the reporting mediator is our primary estimate based on Golestani (2021). Next, we use our own estimate from the cohort-level analysis in Section 8.1: we calculate the implied elasticity of victimization to reporting using variation across cohorts in the magnitude of effect sizes. We estimate that a 1 p.p. decline in victim reporting is associated with a 0.017 p.p. higher cohort-level victimization rate, off a mean victim reporting decline of 9 p.p. and mean victimization increase of 0.15 p.p. Our implied elasticity of victimization to reporting is thus -1.02. Interpreting our cohort-level analysis as a causal elasticity of victimization to reporting requires the strong assumption that differences in reporting effects across cohorts are exogenous. However, it aligns closely with the approach 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.

Next, we borrow an estimate from Miller and Segal (2019), which also studies victim reporting behavior and crime. 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.

Finally, we replace victim reporting as a mediator with the arrest rate. As described in Section 6.4 and Table A.9, we estimate a Secure Communities effect of -1.6 p.p. (SE= 1.202) on the likelihood of an arrest being made in cases with a Hispanic victim, 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 arrest rate, 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 arrest rate 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.

Across all approaches, we find broadly similar conclusions. In all cases, the contribution of reduced victim reporting (or arrest probability) on victimization is *greater* than our estimated victimization increase. Combined with the relatively small contributions from all non-reporting mediators, all cases result in a sizable negative residual.

G Identifying Race of Marginal Offenders

In this appendix, we describe the calculations related to the ethnic composition of marginal offenders (i.e., those who offend against Hispanics because of SC but would not offend otherwise), as discussed in Section 8.2. We make several assumptions to identify the marginal offender Hispanic share, and we show that our estimate would be larger under violations of two key assumptions. We therefore treat our main estimate as a lower bound on the true marginal offender Hispanic share.

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

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

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

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

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

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

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

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

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

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

This translates to $\pi_m = 0.43$.

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

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

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

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

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

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

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

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

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

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

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

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

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