How Police Respond to Increased Immigration Enforcement

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Abstract

A growing literature demonstrates that when deportations surge, serious harm to both noncitizens and citizens follows, from decreased birth weight to increased unemployment. Scholars have hypothesized that these effects of immigration enforcement occur both directly, through increased deportations, and indirectly, through increased policing of immigrant communities (which might in turn lead to more deportations as well). This research note tests this indirect mechanism—increased policing—in a difference-in-differences framework, combining data on traffic stops with data on the rollout of the Secure Communities program (which increased local deportations) and the implementation of sanctuary policies (which decreased such deportations). We find no evidence that increased immigration enforcement led to more traffic stops of Latino drivers (either proportionally or in absolute terms) or that decreased immigration enforcement led to fewer arrests of noncitizens. Our findings therefore clarify the relationship between immigration enforcement and local policing: immigration enforcement *depends on* local arrests, but—absent a collaborative agreement between police and immigration officials—generally does not drive police behavior.

1 Introduction

Most deportations within the United States occur after a criminal arrest by local police, and a new field of legal scholarship, known as "crimmigration," is devoted to the overlap between criminal and immigration law [Chacón, 2012, Stumpf, 2006]. Yet the relationship between traffic stops, arrests, and immigration enforcement has nonetheless received relatively little empirical study. Existing studies focus on the relatively few jurisdictions with explicit co-operation agreements ("287(g)" agreements) with federal immigration authorities [Armenta, 2017, Donato and Rodriguez, 2014, Coon, 2017, Pham and Van, 2022]. Counties and states with those agreements chose to have their police forces enforce immigration laws, and that choice often led to abusive police practices in immigrant communities [Armenta, 2017, Pham and Van, 2022]. We ask what happens when immigration enforcement intensifies without such an explicit agreement.

Outside the limited context of explicit cooperation between federal and local officials, immigration enforcement still depends on arrests by local police to identify noncitizens, but it is less clear whether variation in immigration enforcement *shapes* police behavior in making either traffic stops or arrests. Scholars have noted—but not tested[1]—the possibility that increased immigration enforcement creates incentives for police to stop and arrest people who they suspect are nonctizens (Eagly, 2010, 1348; Kubrin, 2014, 331-32; Kohli et al., 2011). The hypothesis is that, when the probability of transfer from local criminal custody to federal immigration custody rises, police officers may attempt to place more noncitizens in local criminal custody.

We test that hypothesis in this research note. We find no evidence that, when the number of local deportations rises (or falls), police are more (or less) likely to stop Latino motorists or to arrest noncitizens. This conclusion—that variation in the intensity of immigration enforcement has little effect on police behavior—follows from three findings. First, we combine data on the staggered rollout of the federal Secure Communities program with traffic stop data from the Stanford Open Policing Project to evaluate whether the Secure Communities program increased traffic stops of Latino drivers. Second, we use the same traffic stop data to evaluate the effect of sanctuary policies, which constrain transfers from local to federal custody and thereby reduce deportations [Hausman, 2020] and are intended to build trust between police and immigrant communities [Lasch et al., 2018]. Third, we use administrative data from Immigration and Customs Enforcement (graciously shared with us by Alberto

[1] Treyger et al. [2014, 307-08] investigate whether Secure Communities activation changes the ratio of Black to White arrests and, consistent with our results, find no effect, but they lack a direct measure of Latino arrests. And our results are consistent with those of Willoughby [2015], an unpublished undergraduate thesis that examines the effects of Secure Communities on measures of racial profiling in North Carolina alone.

Ciancio and Camilo Garcia Jimeno—see Ciancio and García-Jimeno [2022]) to evaluate the effect of sanctuary policies on the number of local arrests that triggered a match with ICE's database (suggesting that the arrestee was a noncitizen).

We find no evidence that the Secure Communities program, or the sanctuary policies that counteracted it, affected police behavior in making traffic stops of Latino drivers. Nor do we find evidence that sanctuary policies affected police decisions to arrest noncitizens. Finally, we confirm that these null results did not depend on the local political environment: the results are similar in counties that favored Trump and those that favored Clinton in 2016. These null effects may reflect the descriptive fact that a small percentage of local arrests lead to deportations even though most deportations begin with a local arrest.

These results have limitations. First, we do not suggest that direct agreements with ICE (so-called 287(g) agreements) fail to change police behavior. Indeed, there is convincing evidence of racial profiling following such agreements (Santos, 2012; Armenta, 2017, 85-87), but we are unable to add to that evidence because the initiation dates of those agreements do not sufficiently overlap with our dataset. And our results also have no implications concerning bias or abuse in police traffic stop and arrest behavior; we find only that Secure Communities and sanctuary policies did not systematically *change* traffic stop or arrest patterns.

Despite these limitations, our results shed light on the causes of variation in immigration enforcement and the harms that ensue from that enforcement. One might plausibly hypothesize that intensification of immigration enforcement leads to more policing of immigration communities, creating a kind of feedback loop in which more deportations lead to more local arrests and vice versa. We find no evidence of such a dynamic. Secure Communities, which led to a dramatic spike in deportations nationwide, owes that effect to its increase in the chance of transfer from local custody to federal immigration custody, not to changes in policing. Conversely, sanctuary policies, which decreased deportations by around a third in the counties in which they took effect, worked by making transfers from local custody to federal custody less likely. In other words, while immigration enforcement relies on police, police respond little to variation in that enforcement.

2 Existing Literature

These findings build on the existing literature on the causes and effects of variation in immigration enforcement. We add to work on the drivers of immigration enforcement [Cox and Miles, 2013, Hausman, 2020] by clarifying that Secure Communities and sanctuary policies produced their effects on deportations directly, not by causing police to arrest more (or fewer) noncitizens. The lack of an effect on policing contrasts with the well-established finding that federal-local immigration enforcement ("287(g)) agreements do shape police behavior and lead to racial profiling [Armenta, 2017, Donato and Rodriguez, 2014, Coon, 2017, Pham and Van, 2022].

More broadly, these findings add to the large literature in political science and economics on the harms of deportations, suggesting that those harms are imposed directly, through threatened and actual expulsions, rather than indirectly, through changes in police behavior.

First, the political effects of increased immigration enforcement likely reflect increased deportations rather changes in policing. Political scientists have typically found that immigration enforcement, as well as immigrant-hostile laws and proximate experiences with the deportation system, have a mobilizing effect. For example, White [2016] finds that increased local deportations lead to higher Latino voter turnout; Pantoja and Segura [2003] and Pantoja et al. [2001] find that immigrant-hostile laws lead to higher levels of political information and more consciousness of racial issues among newly naturalized Latinos, and Bowler et al. [2006] find that such laws drove Latinos away from the Republican party; Walker et al. [2020] find that proximate experiences with the deportation system make people more likely to participate in protests.[2] Finally, extensive qualitative work suggests that immigration-related policing can play a key mobilizing role in protest in social movements: immigrant communities in Maricopa County, for example, organized to counter abusive police practices there that targeted Latino citizens and noncitizens [Abrams, 2022].

Scholars have also found that these political effects were accompanied by economic and health costs. Using the same research design as White [2016], who relies on the staggered rollout of the Secure Communities deportation program, these scholars have found that increasing local immigration enforcement causes a large variety of harms, including reduced employment [East et al., 2018], reduced student achievement [Bellows, 2019], reduced school enrollment [Dee and Murphy, 2020], reduced use of public benefits [Alsan and Yang, 2019, Watson, 2014], and reduced birth weight [Amuedo-Dorantes et al., 2020]. All of these findings depend on variation in the type of immigration enforcement at issue in this study: deportations that begin with an arrest by a local police officer, rather than a federal immigration officer.

We add to this literature in political science and economics by testing a mechanism through which immigration enforcement might produce these many effects. Because Secure Communities relies on arrests by local police, it could harm immigrant communities either through increased deportations or through increased police stops of Latinos (or both). Harm

[2] Altema McNeely et al. [2022] find, by contrast, that knowing a deportee or detainee increases political discussion but makes voting less likely.

through policing is plausible given that many studies of Secure Communities have found that the program harmed Latino *citizens* as well as noncitizens [East et al., 2018, Watson, 2014, Alsan and Yang, 2019, Dee and Murphy, 2020]. These harms to citizens could reflect changes in policing: some advocates and scholars have suggested that local police might use race as a proxy for immigration status and therefore stop Latino drivers more often when they know that an arrest could lead to deportation [Ridgley, 2008, Kohli et al., 2011, Armenta, 2017, Coleman and Kocher, 2019, Ramos, 2011]; indeed, some scholars describe the variation in Secure Communities enforcement as variation in "immigrant policing" (Cruz Nichols et al., 2018), and many scholars suggest that the political effects of immigration enforcement reflect the "racialized threat" of that enforcement (Nichols and Valdéz, 2020, 691). We test that hypothesis by examining the effects of variation in immigration enforcement on traffic stops of Latino drivers and arrests of noncitizens.

Our results are consistent with those of other studies finding little effect of immigration enforcement on administrative outcomes in the criminal justice system. Treyger et al. [2014] and Hines and Peri [2019], for example, find no effect of Secure Communities on criminal arrests or police efficiency. Our results are also consistent with the large body of evidence finding no relationship between immigration enforcement and crime (Hines and Peri [2019]; Miles and Cox [2014]; Treyger et al. [2014]; Masterson and Yasenov [2021]). And our results also match the growing body of evidence suggesting that police officers are sensitive to the incentives set by their supervisors [Mummolo, 2017, Ba and Rivera, 2019, Magaloni and Rodríguez, 2020]; in counties and states not working directly with ICE, officers have little incentive to pursue deportations, particularly given that deportations are a rare consequence of arrests.

Finally, our results also add to the small but growing literature on the partisan politics of local immigration enforcement. Our null finding is consistent across partisan environments: it persists in counties with both very high and very low shares of the population voting for Trump in 2016. This result is consistent with that of Thompson [2020], who shows that Democratic sheriffs (elected in close races) were no more or less likely than their Republican counterparts to enact local sanctuary policies.[3]

Together, our findings contribute to the scholarship on the ways in which immigration enforcement and local arrests are intertwined. Secure Communities deportations produce their political and economic effects directly, through deportation, rather than indirectly, through changes in police behavior.

[3]Like Thompson's, our results are from before the 2016 election, which may have increased the political salience of immigration enforcement in local politics [Zoorob, 2020].

3 Institutional Background

In order to find and deport noncitizens living within the United States—as opposed to noncitizens who have recently crossed the border—the federal government relies overwhelmingly on arrests by local police [Cantor et al., 2019]. That means that the large majority of Immigration and Customs Enforcement (ICE) arrests take place in jails and prisons, rather than at large. This reliance on criminal arrests for interior deportations means that immigration and criminal enforcement are necessarily linked. In order to study that link, we rely on the staggered rollout of two sets of countervailing interior deportation policies: the Secure Communities program, which increased deportations [Alsan and Yang, 2019], and local sanctuary policies, which decreased them [Hausman, 2020]. We use this variation over time and across counties to test whether increased or decreased deportations affected traffic stops of Latino motorists or arrests of noncitizens.

The Secure Communities program, which dates to 2008, linked U.S. Immigration and Customs Enforcement (ICE) and FBI databases. Since the (staggered) onset of that program, whenever a county jail books in a person arrested by local police, that person's fingerprints are automatically sent to the FBI, where they are matched against not only FBI databases but also the Department of Homeland Security's Automated Biographic Identification System (IDENT) (Council, 2011, 10). The IDENT database is drawn principally from Custom and Border Protection (CBP) records of noncitizens' entry into the United States, including apprehensions of people attempting to cross the border between ports of entry (of Homeland Security, 2012). IDENT also contains at least some U.S. citizens' fingerprints, such as those of noncitizens who have naturalized and of citizens who have opted into trusted traveler programs. The FBI nonetheless uses an IDENT match as enough of a proxy for noncitizenship to cause the transfer of an arrestee's records to ICE, which then makes a guess about whether an arrestee is deportable (Council, 2011, 10). This process produces the database matches that we treat as an imperfect proxy for the number of arrests of noncitizens in each county and month.

If ICE officers decide—after receiving a database match from the FBI—to attempt to deport the person, they typically issue a so-called detainer request (ACLU). Such a request asks the county jail continue to imprison the noncitizen for up to 48 hours beyond when he or she otherwise would have been released. Detainers are intended to make ICE arrests (i.e. transfers from local criminal custody to federal immigration custody) easier: when county jails comply with these requests, ICE has additional time to make the arrest, and need not be present exactly when the person is released.

The FBI-ICE database interoperability introduced by Secure Communities increased the rate of deportations [Alsan and Yang, 2019], and that interoperability was rolled out over time to different counties, creating an opportunity for causal inference. We exploit that opportunity, as many have done before us; by investigating the effect of Secure Communities on traffic stops, we test one of the possible mechanisms by which immigration enforcement imposes the harms that previous studies have demonstrated. Similarly, our sanctuary results take advantage of the fact that state and county sanctuary policies, which counteracted Secure Communities, were implemented at different times. These policies reduced deportations by about a third, on average [Hausman, 2020]. The details of sanctuary policies vary from jurisdiction to jurisdiction; following Hausman [2020], we code counties as sanctuary counties if their policies include refusals to comply with ICE detainer requests.

Finally, a key point is that we do not study 287(g) agreements: agreements between the federal government and local governments to cooperate on immigration enforcement. In states and localities that sign such agreements, state and local officers are actually deputized to act as federal officers: in so-called jail enforcement agreements, local officials question inmates about their immigration status and perform immigration arrests in the jail, and in so-called task force agreements (which were phased out in 2012), local officials can perform immigration arrests outside of jails as well [Pham and Van, 2022, 469-70].

4 Hypotheses

We test the hypothesis that, when local criminal arrests become more likely to result in transfers to federal immigration custody, police will become more likely to stop Latino motorists. We also test the converse of this hypothesis: when local criminal arrests become less likely to lead to transfers to federal immigration custody, police will become less likely to stop Latino motorists.

These hypotheses are plausible in the light of prominent examples of increased policing of immigrant communities when counties entered into cooperative agreements with federal immigration enforcement authorities. Perhaps the best known example involves Maricopa County.[4] There, soon after Sheriff Joe Arpaio entered a 287(g) agreement with ICE, sheriffs' deputies began to organize so-called saturation patrols, which resulted in disproportionate traffic stops and arrests of Latino residents.[5] Under Sheriff's Office's 287(g) agreement with the federal government, the office was authorized to engage in immigration enforcement and explicitly aimed to target noncitizens for stops.[6] The Sheriff's Office also explicitly (and

[4]Melendres v. Arpaio, 989 F.Supp.2d 822 (2013).
[5]*Id.* at 825-26.
[6]*Id.*

unlawfully) considered race as a factor in making such stops, targeting Latino motorists.[7] The Sheriff's Office continued these practices even after the federal government ended the cooperative agreement, doing all it could to cause more deportations.[8]

We test the possibility that intensifying immigration enforcement has similar effects even absent a cooperative agreement. The rollout of S-Comm did not give local authorities any similar mandate to engage in immigration enforcement themselves, but the increasing chance that an arrest would lead to deportation might nonetheless have influenced police behavior, causing more stops of Latino drivers and arrests of noncitizens. If police aimed to take actions resulting in deportations, S-Comm made arrests more likely to achieve that goal. Conversely, sanctuary policies lowered the chance that an arrest would lead to deportation and might have made police less likely to make such stops and arrests.

5 Data

5.1 Secure Communities Data

We merge data on the onset of Secure Communities (S-Comm) at the county level with traffic stop data from the Stanford Open Policing Project (SOPP) to evaluate whether S-Comm shifted police behavior. We use a set of criteria to generate a balanced panel of traffic stop data at the county/department/month level. First, the temporal domain must overlap with the time period in which S-Comm is an active Federal program (October 2008 - November 2014). Second, there must be at least 10 months of pre-treatment data, that is, ten months before the onset of S-Comm in the department/county at issue. Therefore, we include only counties/departments in which S-Comm activation occurred after July of 2009. Third, consistent with our sanctuary policy data detailed in Section 5.2, we use information from the largest 10% of counties by the proportion of the population that is Latino in 2010.

These criteria construct our main sample of interest. Because the traffic stop data from SOPP is relatively limited in time, we only have data on 10 states, 12 police departments (including 6 state highway patrols: Massachusetts, North Carolina, South Carolina, Tennessee, Texas, Virginia), and 485 counties. However, these data capture a significant proportion of the Latino population. Overall, these data cover 8.6 million Latinos based on 2010 ACS estimates, equivalent to roughly 17% of the Latino population.[9] Our data includes demographically relevant counties such as Los Angeles (CA), San Francisco (CA), Tarrant (TX),

[7] *Id.*
[8] *Id.*
[9] In 2010, there were 50.5 million Latinos nationally.

Cameron (TX), and Kern (CA). For each county/department/month, we count the number of stops for Latinos, non-Latinos, and whites.

5.2 Sanctuary Policy Data

We merge data on the onset of county sanctuary policies from Hausman [2020] with traffic stop data from the Stanford Open Policing Project (SOPP) to evaluate whether sanctuary policies change police behavior. The sanctuary policy data includes information from all but 12 of the 314 largest 10% of counties by Latino population between 2010-2015. After merging the sanctuary policy and SOPP data, we have a 72 month panel that includes 141 unique counties and 24 unique police departments. These counties cover 51% of the Latino population in the United States and include localities with demographically and politically significant Latino populations, such as Los Angeles, Houston, Dallas, San Antonio, and San Diego.[10] For each county/department/month in the data, we count the number of stops for Latinos, non-Latinos, and whites.

In addition, we merge the sanctuary policy data with data from the Department of Homeland Security's Automated Biographic Identification System (IDENT). The database includes information on the number of noncitizen arrestees whose information was submitted to ICE to verify immigration status in addition to the number of noncitizen arrestees whose information was matched to an ICE database after submission (that is, the arrestee was identified as a potential undocumented immigrant). The IDENT data is more complete than the SOPP data, covering 293 of the 314 largest 10% of counties by Latino population. Thus, the sanctuary policy data merged with the IDENT data captures 80% of the overall Latino population in 2010. We construct two outcomes from this data. The first is the logged number of ICE database matches (plus 1 to ensure identification). The second is the proportion of submissions to ICE that led to matches. To reiterate, more ICE database matches—either in absolute terms or as a proportion of submissions—might suggest the police are arresting more noncitizens.

6 Estimation Strategy

To evaluate the effect of sanctuary policies and S-Comm on police behavior, we use a difference-in-differences approach for the county/department/month dataset. We estimate:

$$Y_{cdm} = \tau \text{Policy}_{cdm} + \alpha_{cd} + \gamma_m + \delta_{sm} + \varepsilon_s$$

[10]Our sanctuary policy sample covers 26 million Latinos (2010 Census).

where Y_{cdm} is the number of logged Latino stops (+1 to facilitate identification), the proportion of stops that are Latino, the number of logged ICE database matches, or the proportion of ICE database submissions that led to matches in a given department (d) within a given county (c) on a given month (m). Policy_{cdm} is a binary indicator equal to 1 when a department operates in a county that has activated S-Comm in the S-Comm dataset or a sanctuary policy in the sanctuary policy dataset. τ is the coefficient of interest. If S-Comm motivates increases in policing against Latinos, τ should be positive. If sanctuary policies reduce levels of policing against Latinos, τ should be negative. α_{cd} are county/department fixed effects and γ_m are month fixed effects. In addition, consistent with prior research assessing the effects of immigration policy [Alsan and Yang, 2019], we account for time-varying common shocks within state by including state-by-month fixed effects δ_{sm}. ε_s are robust errors clustered by state since some sanctuary and S-Comm policies were either adopted directly by state governments or all counties within a state simultaneously [Hausman, 2020].

We also present event study estimates to test whether our comparison counties serve as valid counterfactuals and to test whether the effects are stable across months following the treatment. We estimate:

$$Y_{cdm} = \sum_{k \neq 0}^{k} \beta^k \text{P}^k_{cdm=k} + \alpha_{cd} + \gamma_m + \delta_{sm} + \varepsilon_s$$

where k is the time to treatment. P^k are a series of binary indicators measuring time to treatment for a specific county/department. The month in which the policy is implemented, $k = 0$, is the reference category. When $k = 10/k = -10$, all months on or after 10 months before/after the policy is implemented in a specific county/department are equal to one.

7 Results

7.1 Secure Communities and Traffic Stops

The Secure Communities (S-Comm) program made local arrests much more likely to lead to deportations. If local police are motivated to make traffic stops by the possibility of that such stops will lead to deportations, then S-Comm's increase in the chance of a transfer to ICE custody might lead police to make more traffic stops of Latino drivers. We find no evidence of such an effect.

First, we find an imprecise null effect of the Secure Communities rollout on the number of stops of Latino drivers. Our preferred difference-in-differences estimates (displayed in Table 1) suggest that Secure Communities *decreases* traffic stops of Latinos by 4%, a statistically insignificant effect ($p = 0.49$, see Table 1, Panel A, Model 3). That effect is equivalent to 12 fewer stops within a given county/department/month relative to a pre-treatment baseline of 383 traffic stops. But the confidence interval covers -0.10-0.02, 6 percent in each direction, leading us to place limited weight on this null result.

Table 1: Effect of Secure Communities on Stop Outcomes

Panel A: Log(Latino Stops + 1)	(1)	(2)	(3)	(4)
S-Comm	0.14^{**}	0.14	−0.04	−0.05
	(0.05)	(0.10)	(0.06)	(0.06)
R^2	0.87	0.87	0.90	0.92
Panel B: Pr(Latino)	(1)	(2)	(3)	(4)
S-Comm	0.00	0.00	−0.00	−0.01
	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.95	0.95	0.97	0.97
N	4453	4453	4453	4453
County/Departments	61	61	61	61
Months	73	73	73	73
County/Department FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
State x Month FE	N	N	Y	Y
County/Department Trend	N	N	N	Y
State CSE	N	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of Secure Communities under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered standard errors instead of county/department clustered standard errors (Model 1). Model 3 adjusts for state × month fixed effects. Model 4 adjusts for a county/department-specific trend. Panel A displays effect estimates of Secure Communities using logged Latino stops as the outcome, and Panel B displays effects estimates using the probability that a stop involves a Latino driver as the outcome. Effects displayed in Figure 1 below are from column 3.

Second, and more meaningfully, we find no evidence that S-Comm changes the chance that a traffic stop will involve a Latino driver. S-Comm decreases the proportion of stops that are Latino by 0.4 percentage points (pp., $p = 0.37$, see Table 1, Panel B, Model 3), a shift equivalent to 1.3% of the pre-treatment mean (22 pp.).[11] Moreover, these estimates are quite precise: a single percentage point increase in the proportion of stops involving Latinos is outside the 95% confidence interval (-0.010-0.002).

Event study estimates corroborate these findings (Figure 1). First, treated county/departments and untreated county/departments possess similar outcome trends prior to S-Comm for Latino stops (Panel A) and the proportion of traffic stops involving Latino drivers (Panel B), suggesting that untreated county/departments serve as a valid counterfactual. Second, consistent with the main findings, post-treatment coefficients are largely statistically null.

[11] These null effects are not sensitive to clustering by state SEs. The p-value for the effect of S-Comm on Latino stops (Panel A) and the proportion of stops that are Latino (Panel B) using Model 3 on Table 1 is $p < 0.31$ and $p < 0.2$ respectively using county/department clustered SEs.

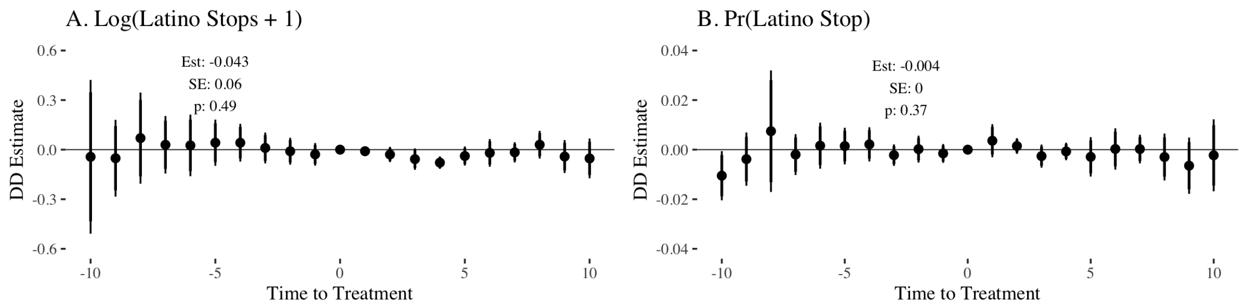

Figure 1: Event study estimates characterizing effect of Secure Communities (S-Comm). See Table 1 for corresponding difference-in-differences regression results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. All models include month, county-department, and state × month fixed effects. Each panel uses a different outcome and/or comparison group (specified by panel title). Annotations denote generalized (non-event study) difference-in-differences estimates, standard errors, and p-values. 95% CIs displayed derived from state-clustered SEs.

The Secure Communities program, by integrating FBI and ICE databases, increased the chance that a local arrest would lead to a transfer to federal immigration custody. These results suggest that that increasing chance of a transfer to ICE custody on the back end had little effect on police behavior. Our confidence in this result is increased by the fact that the onset of sanctuary policies—which disrupted the functioning of Secure Communities—also had no observable effect on traffic stops (see Appendix).

7.2 Sanctuary Policies and Arrests of Noncitizens

In order to obtain a more precise estimate of the effect of changing enforcement intensity, we assess whether sanctuary policies affected the number of police arrests of noncitizens. As a measure of these arrests, we use IDENT matches (see Data section above); because this data is created through the Secure Communities program and did not exist before its rollout, we only consider the effect of sanctuary policies. If sanctuary policies caused widespread changes in police officers' stop behavior, we would expect to see changes in the number of arrests of the noncitizens who would be the targets of such stops.

We find no evidence that sanctuary policies changed the number of arrests of noncitizens (i.e. IDENT matches) or the proportion of all arrests involving noncitizens (i.e. IDENT matches as a proportion of IDENT submissions). Our preferred estimate suggests that sanctuary policies do not change the logged number of ICE matches (Table 2, Panel A, Model 3). Additionally, sanctuary policies do not change the proportion of ICE matches

Table 2: Effect of Sanctuary Policies on Arrests Matched To ICE Databases: Limited Table

Panel A: Log(All Matches + 1)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.33***	0.33	0.00	0.03	−0.05
	(0.03)	(0.20)	(0.15)	(0.14)	(0.14)
N	26663	26663	26663	26663	26663
R^2	0.75	0.75	0.87	0.90	0.96
Panel B: Pr(Matches | Submissions)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.00	0.00	−0.00	0.00	
	(0.00)	(0.00)	(0.00)	(0.00)	
N	19932	19932	19932	19932	
R^2	0.68	0.68	0.72	0.76	
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	N	N	Y	Y	Y
County Trend	N	N	N	Y	Y
S-Comm Indicator	N	N	N	N	Y
State CSE	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of sanctuary policies under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-5 use state clustered standard errors instead of county clustered standard errors. Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county-specific trend. Model 5 adjusts for an additional Secure Communities indicator. Panels A and B display effect estimates of sanctuary policies using logged IDENT matches and the probability a submission is a match as the respective outcome. Model with S-Comm indicator not available for Panel B since they are not identified (the outcome depends on S-Comm activation).

among submissions of arrestee information to ICE (Table 2, Panel B, Model 3). This effect is particularly precise, with changes of more than one quarter percentage point falling outside the confidence interval (-0.002-0.002).

Event study estimates are consistent with these results. Prior to the onset of sanctuary policies, there are not differential trends in counties that are about to adopt sanctuary policies and those that are not (Figure 2, Panels A, B respectively). Nor is there any evidence of an effect in the post-treatment period. There is no evidence that sanctuary policies caused police to reduce the number of noncitizens they brought into county jails.

Taking these three sets of results together, we find no evidence of any systematic effect of enforcement intensity on police stop or arrest behavior. But it remains possible that this lack of an effect masks countervailing effects in different counties. In the Appendix, we test whether there are countervailing effects in conservative and liberal counties, and we find no evidence of such heterogeneity.

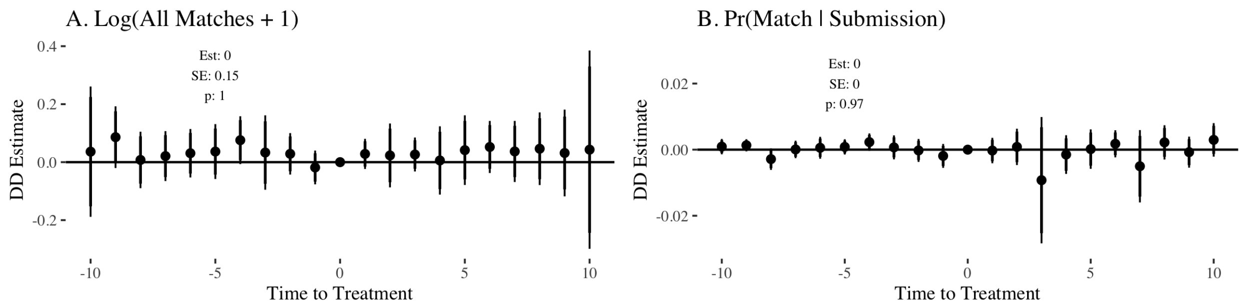

Figure 2: Event study estimates characterizing effect of sanctuary policy on IDENT outcomes. See Table 2 for corresponding difference-in-differences results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Each panel uses a different outcome (specified by panel title). Annotations denote generalized (non-event study) difference-in-differences estimates, standard errors, and p-values. 95% CIs displayed derived from state-clustered robust SEs.

8 Discussion

We evaluate whether heightened immigration enforcement—a higher probability of deportation for noncitizens who have been arrested by local police—shifts police stop and arrest behavior. We find no evidence of such a shift. This null result is consistent across three empirical tests: of the effect of Secure Communities on traffic stops, the effect of sanctuary policies on traffic stops, and the effect of sanctuary policies on arrests of noncitizens. And the same null result holds across a wide variety of difference-in-differences and event study specifications, in addition to different political environments.

We are particularly convinced by the fairly precise null finding that sanctuary policies, which reduce deportations by a third [Hausman, 2020], do not make arrests of noncitizens more or less frequent. We find that null effect convincing not only because it is precisely estimated but because noncitizen arrests and local deportations are (unsurprisingly) very highly correlated. Figure 3, Panel B, shows that correlation.

We do *not* interpret our findings to suggest that police arrests and deportations are unrelated—Figure 3, Panel B, shows how far that is from the case—but instead to suggest that deportations depend on arrests rather than shaping patterns of arrest behavior.

A key limitation of our study is that our null results for traffic stop outcomes are not precisely estimated, unlike those for noncitizen arrests. We are skeptical, however, that immigration enforcement affects Latino traffic stops without affecting noncitizen arrests. A traffic stop can only lead to deportation through an arrest, which triggers a notification to ICE. It would therefore be surprising to find evidence of profiling in traffic stops but not in ar-

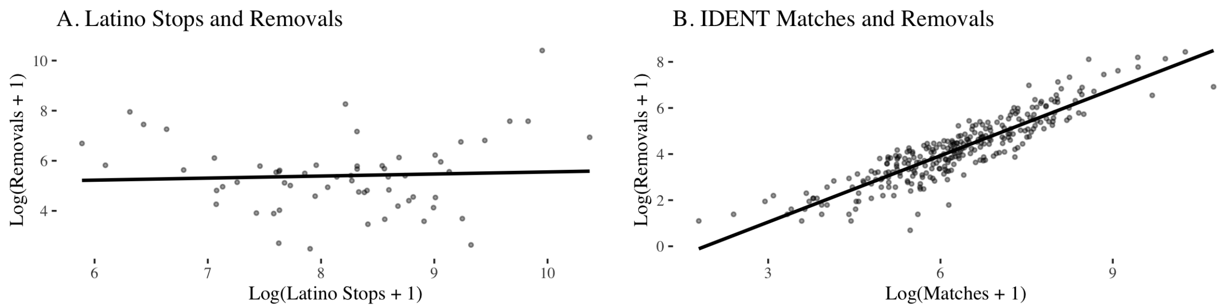

Figure 3: Association between Latino stops, IDENT matches, and removals.

rests. More broadly, whereas there is an extremely close cross-sectional relationship between noncitizen arrests and deportations, there is no such relationship for Latino traffic stops and deportations (see Figure 3, Panel A). That descriptive fact should not be surprising—even though many deportations begin with convictions for traffic offenses—simply because deportations are so rare relative to traffic stops and to arrests. In 2014 and 2015, across our sample of the largest ten percent of counties by Latino population, about six percent of arrests triggered a match in ICE's database and 11 percent of those matches resulted in deportations, meaning that under one percent of arrests resulted in deportations. Because our dataset does not connect traffic stops with arrests (and many arrests occur without a traffic stop), we lack a similar measure of the proportion of traffic stops leading to arrests and deportations, but there is every reason to guess that traffic stops result even more rarely in deportations.

In sum, our results suggest that police behavior drives immigration enforcement but is not driven by it. *This does not mean disparate policing is a non-factor immigration enforcement.* Racial disparities in traffic stops exist at baseline regardless of interior immigration enforcement policies [Pierson et al., 2020], and they may exacerbate the effects of those policies. It remains possible that some individual officers stop more Latinos for the purposes of ensnaring them in deportation proceedings [Sontag, 2018]. And we know that cooperative agreements between police and immigration agencies may lead to such racial profiling. We do not study that cooperative context, however, and we find no evidence that *changes* in the level of immigration enforcement lead to *changes* in police behavior when it comes to disparately stopping Latinos.

This conclusion matters not only directly, for our understanding of the relationship between policing and immigration enforcement, but also more broadly.

First, our findings shed light on the mechanism driving the many political, economic and human effects of increased immigration enforcement. Immigration enforcement likely imposes these effects directly, through detention and deportation of noncitizens, rather than indirectly, through increased police profiling in stops or arrests.

Second, our findings build on scholarship on the importance of police officers' organizational incentives. Secure Communities was a database integration program that allowed ICE to identify noncitizens more quickly; it did little to alter the day-to-day tasks and incentives of police officers. And even sanctuary policies, which were typically implemented by county sheriffs, targeted behavior at county jails (refusals to hold noncitizens for ICE) rather than behavior in making arrests and stops. Our findings are therefore consistent with those of researchers showing that unequivocal departmental policies can radically reshape the behavior of police bureaucrats [Mummolo, 2017, Ba and Rivera, 2019, Magaloni and Rodríguez, 2020]. Indeed, that scholarship might help explain why cooperative agreements between localities and ICE had important effects on policing, whereas S-Comm and sanctuary policies did not: cooperative agreements sought to shift police stop and arrest behavior, whereas S-Comm and sanctuary policies did not.[12].

Our analysis has some shortcomings. Given limitations in the accessibility of traffic stop data, our results do not generalize to the entire United States, nor do they capture police operations covering 100% of the Latino population. But our analyses do include contexts with a large Latino population (e.g. Los Angeles county). In addition, it is unclear why out-of-sample geographic contexts or police departments would be motivated differently in response to the policies we evaluate than the contexts/departments in our sample. To this end, we conduct intra-state replications of our results covering the California and North Carolina highway patrols. These departments have jurisdiction over the first and twelvth largest Latino populations by state. Consistent with our broader, yet limited, analysis, we do not find that Secure Communities increased disparate policing against Latinos in either state (Section C.2, Figure C5 and Section C.3, Figure C6).

In summary, we find that changes in the level of immigration enforcement do not change police behavior. These findings suggest that police behavior shapes immigration enforcement, rather than the other way around. And these findings suggest that immigration enforcement produces a variety of harms directly (through deportation) instead of indirectly (through policing).

[12]Examples include the Maricopa County's Sheriffs Office (discussed above) and Operation Strong Safety, a policy that mandated Texas Highway Patrol engage in immigration enforcement at the South Texas border [Benen, 2014]

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Appendices

A County Distribution of State Patrol Officers

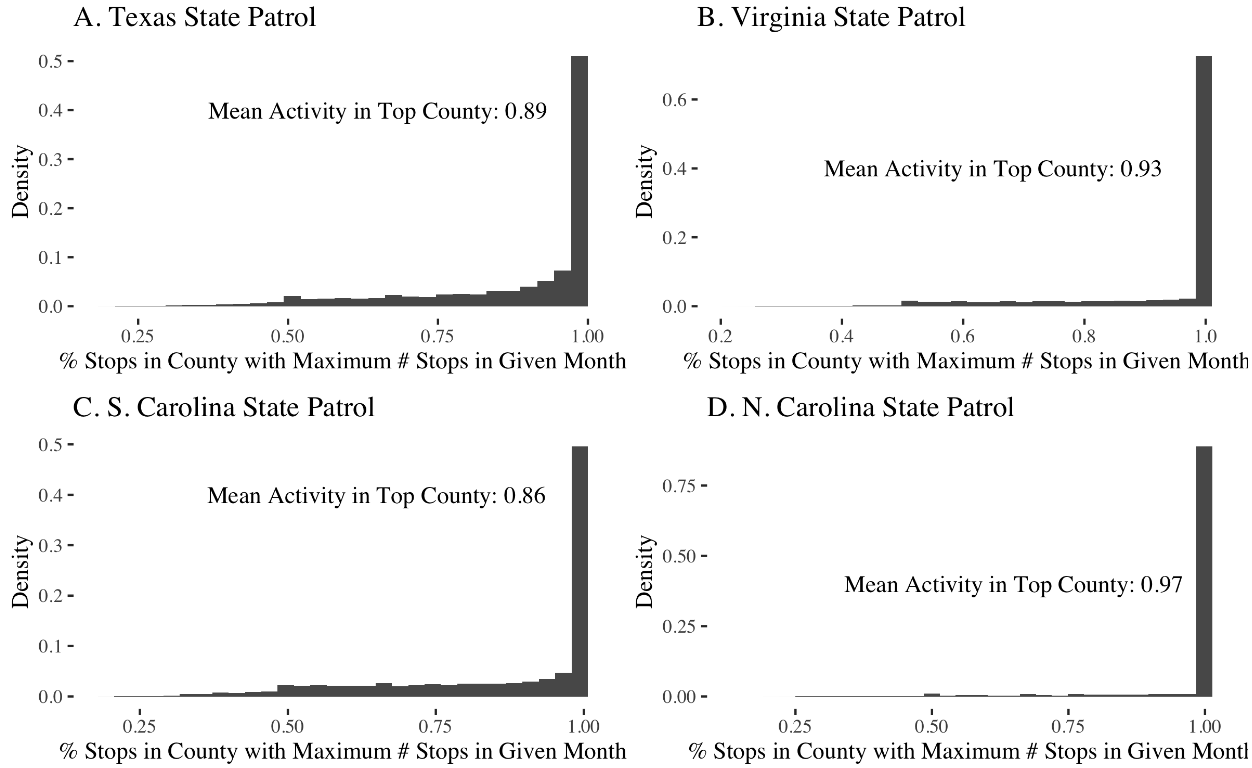

Figure A1: Histogram characterizing proportion of time spent in county with maximum level of stop activity (x-axis) at the officer/month level for State Patrol departments (2008-2015).

Given that the majority of our data are from various State Patrols, one concern may be that State Patrol officers will not have strong relationships with county jails such that they are aware of the jail's immigration enforcement priorities. We contend that this is unlikely. State Patrol officers are typically assigned to work in one county when they conduct their operations. Figure A1 displays the distribution at the officer/month level of the proportion of State Patrol stops in the county with the maximum number of stops in a given month across Texas (Panel A), Virginia (Panel B), South Carolina (Panel C) and North Carolina (Panel D). Across the board, individual State Patrol officers within a given month typically operate within a single county. The percentage of stops in a single county is 89%, 93%, 86%, and 97% respectively. State Patrol officers likely have strong relationships particular county jails, allowing them to perceive changes in jail enforcement priorities.

B Map Characterizing Sample and Treatment

B.1 S-Comm Analysis

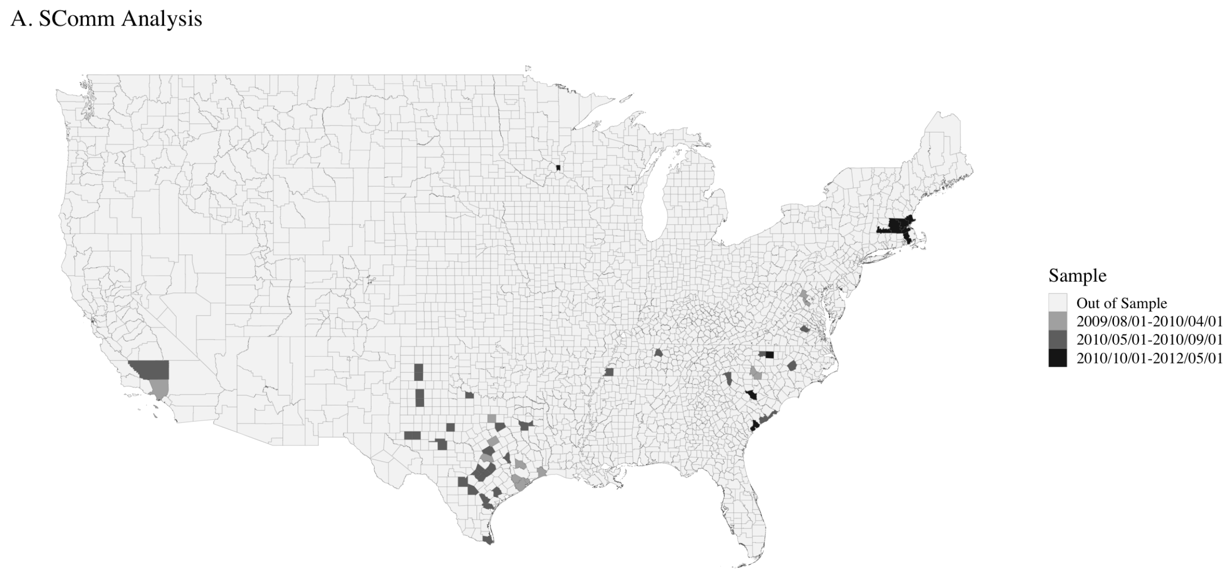

Figure B2: Map Characterizing Sample and Treatment Status Across Counties

B.2 Sanctuary Analysis

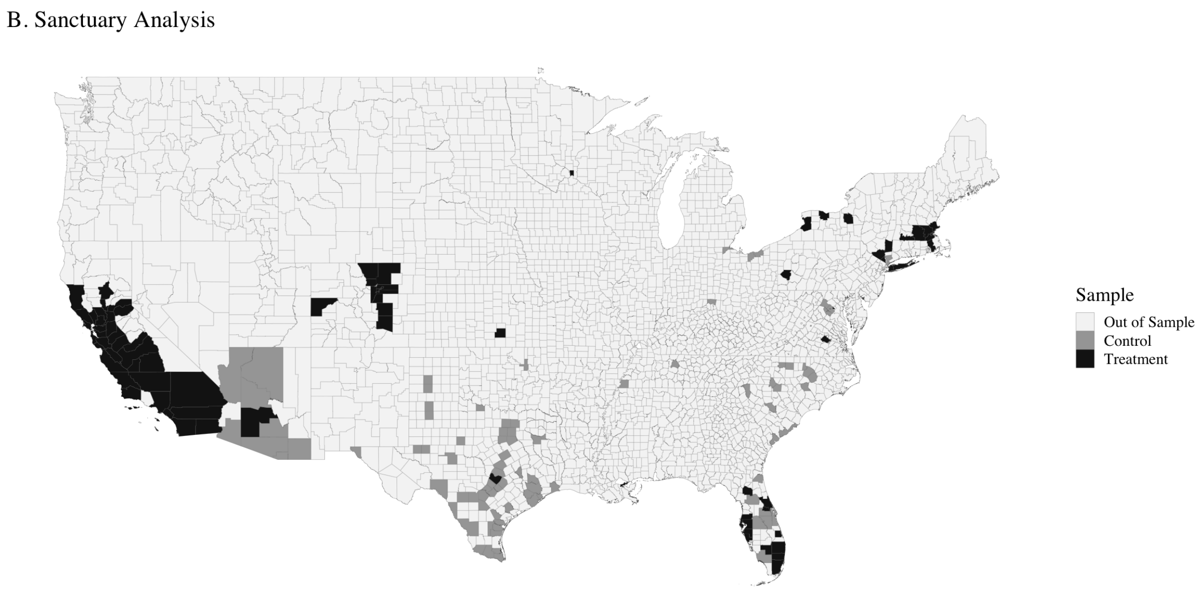

Figure B3: Map Characterizing Sample and Treatment Status Across Counties

B.3 IDENT Analysis

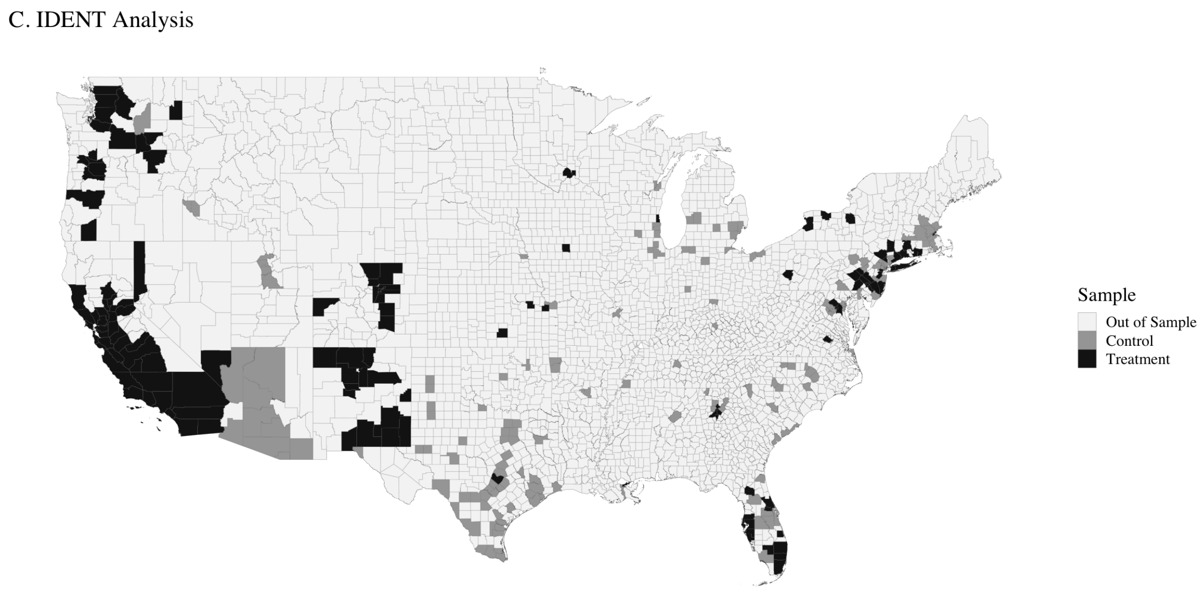

Figure B4: Map Characterizing Sample and Treatment Status Across Counties

C Secure Communities Results

C.1 Full Regression Table

Table C1: Effect of Secure Communities on Relevant Stop Outcomes (county/department/month data)

Panel A: Log(Latino Stops + 1)	(1)	(2)	(3)	(4)
S-Comm	0.15**	0.15	−0.03	−0.03
	(0.05)	(0.10)	(0.06)	(0.06)
R^2	0.87	0.87	0.90	0.92
Panel B: Log(non-Latino Stops + 1)	(1)	(2)	(3)	(4)
S-Comm	0.11**	0.11	−0.04	0.01
	(0.04)	(0.08)	(0.04)	(0.05)
R^2	0.89	0.89	0.91	0.93
Panel C: Log(white Stops + 1)	(1)	(2)	(3)	(4)
S-Comm	0.11**	0.11	−0.03	0.02
	(0.04)	(0.08)	(0.03)	(0.05)
R^2	0.86	0.86	0.89	0.92
Panel D: Pr(Latino, non-Latino ref)	(1)	(2)	(3)	(4)
S-Comm	0.00	0.00	−0.00	−0.01
	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.95	0.95	0.97	0.97
Panel E: Pr(Latino, white ref)	(1)	(2)	(3)	(4)
S-Comm	0.00	0.00	−0.00	−0.01
	(0.00)	(0.00)	(0.01)	(0.01)
R^2	0.93	0.93	0.96	0.97
N	4453	4453	4453	4453
County/Departments	61	61	61	61
Months	73	73	73	73
County/Department FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
State x Month FE	N	N	Y	Y
County/Department Trend	N	N	N	Y
State CSE	N	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of Secure Communities under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered standard errors instead of county/department clustered standard errors (Model 1). Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county/department-specific trend. Panels A, B, C, D and E display effect estimates of Secure Communities using logged Latino stops, logged non-Latino stops, logged white stops, the probability a stop is Latino with non-Latino reference category, and the probability a stop is Latino with a white reference category as the respective outcome. Effects displayed in main text on Figure D7 are from column 3.

Table C2: Effect of Secure Communities on Latino stops (race/county/department/month data)

	Log(Stops + 1)					
	(1)	(2)	(3)	(4)	(5)	(6)
S-Comm x Latino	0.32**	0.00	0.00	0.35**	0.05	0.05
	(0.05)	(0.07)	(0.07)	(0.08)	(0.12)	(0.12)
Outcome SD	1.24	1.24	1.24	1.11	1.11	1.11
Comparison	Non-Lat.	Non-Lat.	Non-Lat.	White	White	White
Months	73	73	73	73	73	73
County/Departments	61	61	61	61	61	61
N	8906	8906	8906	8906	8906	8906
R^2	0.78	0.87	0.88	0.76	0.85	0.86
County/Department FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Race x Month FE	N	Y	Y	N	Y	Y
Race x State FE	N	Y	Y	N	Y	Y
State x Month FE	N	Y	Y	N	Y	Y
County/Dept. Trend	N	N	Y	N	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. County cluster robust standard errors in parentheses.

C.2 California Replication

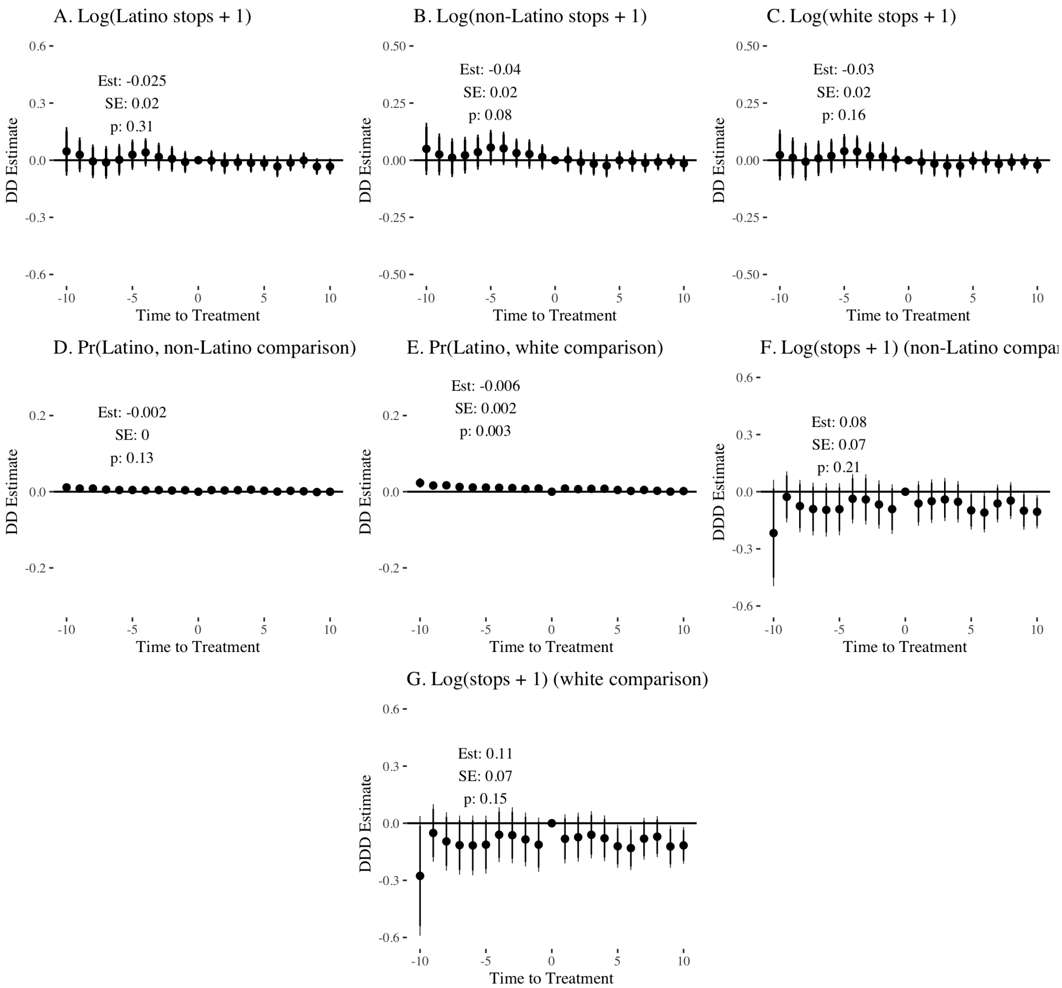

Figure C5: Event study estimates characterizing effect of Secure Communities (S-Comm) on California Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies for Panels A-E. For Panels F-G, it is the triple differences estimate for the effect of S-Comm on Latino stops. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Each panel uses a different outcome and/or comparison group (specified by panel title). Annotations denote generalized (non-event study) difference-in-differences estimates, standard errors, and p-values. 95% confidence intervals displayed derived from standard errors clustered at the state level.

C.3 North Carolina Replication

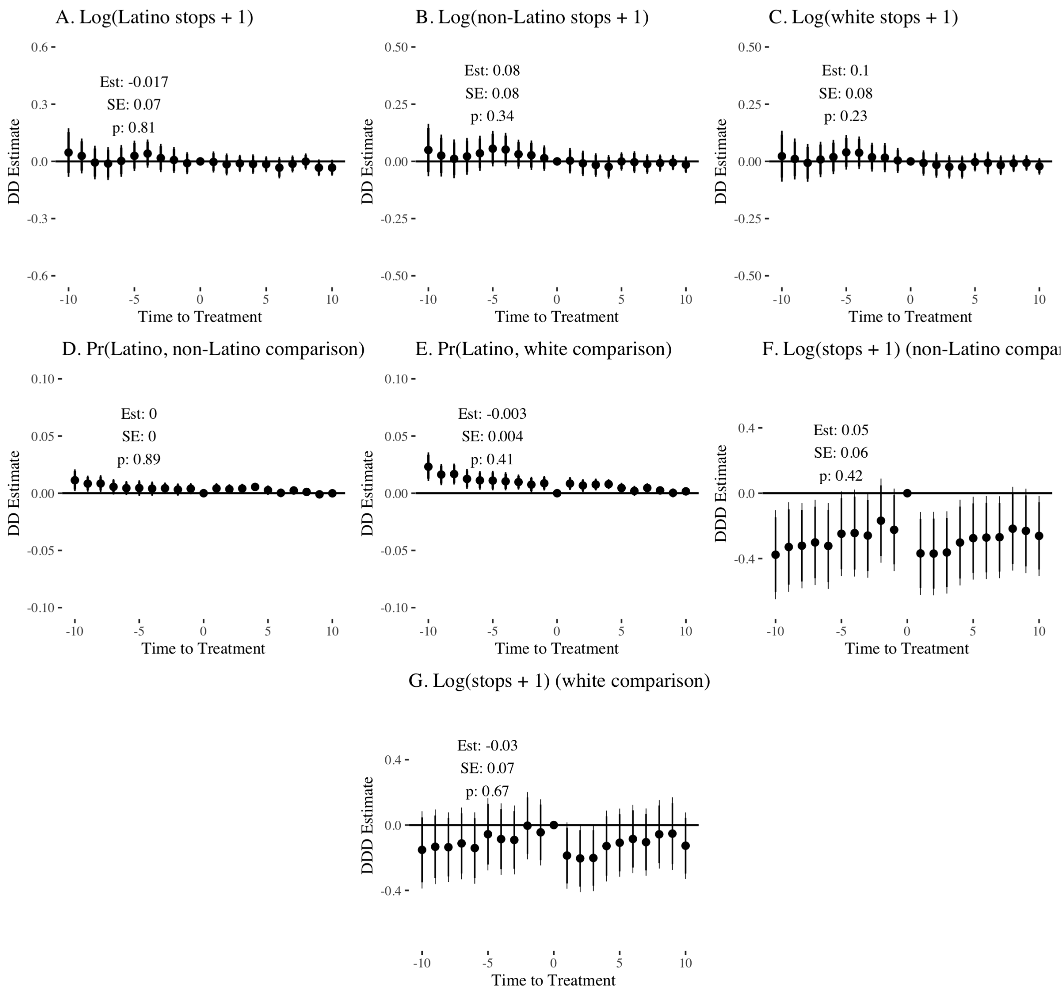

Figure C6: Event study estimates characterizing effect of Secure Communities (S-Comm) on North Carolina Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies for Panels A-E. For Panels F-G, it is the triple differences estimate for the effect of S-Comm on Latino stops. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Each panel uses a different outcome and/or comparison group (specified by panel title). Annotations denote generalized (non-event study) difference-in-differences estimates, standard errors, and p-values. 95% confidence intervals displayed derived from standard errors clustered at the state level.

Table D3: Effect of Sanctuary Policies on Stop Outcomes

Panel A: Log(Latino Stops + 1)	(1)	(2)	(3)	(4)
Sanctuary	0.05	0.05	0.08	−0.14
	(0.04)	(0.06)	(0.10)	(0.13)
R^2	0.94	0.94	0.95	0.97
Panel B: Pr(Latino)	(1)	(2)	(3)	(4)
Sanctuary	−0.12*	−0.12	−0.07	0.00
	(0.05)	(0.08)	(0.06)	(0.01)
R^2	0.95	0.95	0.96	0.97
N	11304	11304	11304	11304
County/Departments	157	157	157	157
Months	72	72	72	72
County/Department FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
State x Month FE	N	N	Y	Y
County/Department Trend	N	N	N	Y
State CSE	N	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of sanctuary policies under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered standard errors instead of county/department clustered standard errors (Model 1). Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county/department-specific trend. Panel A displays effect estimates of sanctuary policies using logged Latino stops as the outcome, and Panel B displays effects estimates using the probability that a stop involves a Latino driver as the outcome. Effects displayed on Figure D7 are from column 3.

D Sanctuary Policy Results

D.1 Sanctuary Policies and Traffic Stops

Sanctuary policies have no measurable effect on traffic stops of Latino drivers, although again, our estimates are imprecise. One might expect sanctuary policies to reduce traffic stops of Latino drivers for the same reason that one might expect Secure Communities to increase those stops: both policies changed the chance that a local arrest would lead to a transfer to federal custody for deportation.

Our preferred estimate suggests that sanctuary policies increase Latino stops by a statistically insignificant 8%, or 90 stops relative to a pre-treatment mean of 1112 stops per county/department/month ($p = 0.45$, see Table D3, Panel A, Model 3). Likewise, sanctuary policies reduce the proportion of traffic stops that are Latino (relative to non-Latino) by 7 percentage points, a statistically insignificant effect equivalent to 9% of the pre-sanctuary average ($p = 0.25$, Table D3, Panel B, Model 3).

Event study estimates also reveal no evidence of either an effect or of pre-treatment trends that might undermine the estimation strategy (Figure D7). In the pre-sanctuary period,

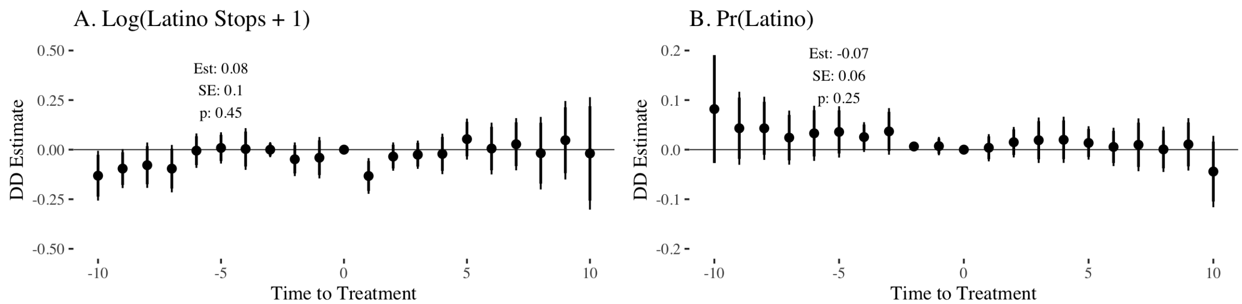

Figure D7: Event study estimates characterizing effect of sanctuary policy. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Annotations denote generalized (non-event study) difference-in-differences estimates, standard errors, and p-values. 95% CIs displayed derived from state-clustered SEs.

there are not consistent statistically significant differences between treated and untreated county/departments relative to the moment of sanctuary activation. Likewise, relative to the moment of sanctuary activation, the post-sanctuary coefficients are not statistically different for both the level of Latino stops (Panel A) and the proportion of stops that are Latino (Panel B).

If sanctuary policies systematically affect police traffic stop behavior, we do not detect that effect. But we acknowledge that our results are relatively imprecise. For example, a ten percentage point reduction in the proportion of traffic stops involving Latino motorists would be consistent with these results. Because of this imprecision, we turn next to an outcome for which we have more data: arrests of noncitizens.

D.2 Full Regression Tables

Table D4: Effect of Sanctuary Policies on Relevant Stop Outcomes (county/department/month data)

Panel A: Log(Latino Stops + 1)	(1)	(2)	(3)	(4)
Sanctuary	0.05	0.05	0.08	−0.14
	(0.04)	(0.06)	(0.10)	(0.13)
R^2	0.94	0.94	0.95	0.97
Panel B: Log(non-Latino Stops + 1)	(1)	(2)	(3)	(4)
Sanctuary	0.04	0.04	−0.01	−0.02
	(0.04)	(0.07)	(0.05)	(0.04)
R^2	0.94	0.94	0.96	0.97
Panel C: Log(white Stops + 1)	(1)	(2)	(3)	(4)
Sanctuary	0.06	0.06	−0.01	−0.03
	(0.04)	(0.07)	(0.05)	(0.05)
R^2	0.93	0.93	0.95	0.96
Panel D: Pr(Latino, non-Latino ref)	(1)	(2)	(3)	(4)
Sanctuary	−0.12*	−0.12	−0.07	0.00
	(0.05)	(0.08)	(0.06)	(0.01)
R^2	0.95	0.95	0.96	0.97
Panel E: Pr(Latino, white ref)	(1)	(2)	(3)	(4)
Sanctuary	−0.23*	−0.23	−0.15	−0.02
	(0.09)	(0.16)	(0.12)	(0.01)
R^2	0.94	0.94	0.94	0.97
N	11304	11304	11304	11304
County/Departments	157	157	157	157
Months	72	72	72	72
County/Department FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
State x Month FE	N	N	Y	Y
County/Department Trend	N	N	N	Y
State CSE	N	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of sanctuary policies under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered standard errors instead of county/department clustered standard errors (Model 1). Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county/department-specific trend. Panels A, B, C, D and E display effect estimates of sanctuary policies using logged Latino stops, logged non-Latino stops, logged white stops, the probability a stop is Latino with non-Latino reference category, and the probability a stop is Latino with a white reference category as the respective outcome. Effects displayed in main text on Figure D7 are from column 3.

Table D5: Effect of Sanctuary Policies on Latino stops (race/county/department/month data)

	Log(Stops + 1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Sanctuary x Latino	0.16	−0.06	−0.06	0.19	−0.05	−0.05
	(0.47)	(0.05)	(0.05)	(0.43)	(0.06)	(0.06)
Comparison	Non-Lat.	Non-Lat.	Non-Lat.	White	White	White
Months	73	73	73	73	73	73
County/Departments	61	61	61	61	61	61
N	22608	22608	22608	22608	22608	22608
R^2	0.75	0.87	0.88	0.73	0.85	0.86
County/Department FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Race x Month FE	N	Y	Y	N	Y	Y
Race x State FE	N	Y	Y	N	Y	Y
State x Month FE	N	Y	Y	N	Y	Y
County/Dept. Trend	N	N	Y	N	N	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. State cluster robust standard errors in parentheses.

E IDENT Results

Table E6: Effect of Sanctuary Policies on Arrests Matched To ICE Databases: Full Table

Panel A: Log(All Matches + 1)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.33***	0.33	0.00	0.03	−0.05
	(0.03)	(0.20)	(0.15)	(0.14)	(0.14)
N	26663	26663	26663	26663	26663
R^2	0.75	0.75	0.87	0.90	0.96
Panel B: Log(L1 Matches + 1)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.34***	0.34*	0.11	−0.01	−0.06
	(0.02)	(0.14)	(0.12)	(0.12)	(0.12)
N	26663	26663	26663	26663	26663
R^2	0.79	0.79	0.87	0.90	0.93
Panel C: Log(L2/L3 Matches + 1)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.29***	0.29	0.01	0.07	−0.01
	(0.03)	(0.20)	(0.14)	(0.13)	(0.12)
N	26663	26663	26663	26663	26663
R^2	0.73	0.73	0.86	0.89	0.95
Panel D: Log(Submissions + 1)	(1)	(2)	(3)	(4)	(5)
	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.44***	0.44	−0.18	0.02	−0.14
	(0.04)	(0.38)	(0.25)	(0.14)	(0.13)
N	26663	26663	26663	26663	26663
R^2	0.73	0.73	0.87	0.90	0.99
Panel E: Pr(L1 Matches | Matches)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.02***	0.02	−0.01	−0.01	
	(0.00)	(0.02)	(0.01)	(0.01)	
N	19638	19638	19638	19638	
R^2	0.42	0.42	0.51	0.53	
Panel F: Pr(Matches | Submissions)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.00	0.00	−0.00	0.00	
	(0.00)	(0.00)	(0.00)	(0.00)	
N	19932	19932	19932	19932	
R^2	0.68	0.68	0.72	0.76	
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	N	N	Y	Y	Y
County Trend	N	N	N	Y	Y
S-Comm Indicator	N	N	N	N	Y
State CSE	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of sanctuary policies under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-5 use state clustered standard errors instead of county clustered standard errors. Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county-specific trend. Model 5 adjusts for an additional Secure Communities indicator. Panels A-F display effect estimates of sanctuary policies using logged IDENT matches, logged L1 IDENT matches, logged L2/L3 IDENT matches, logged submissions, the probability a match is an L1 match, and the probability a submission is a match as the respective outcome. Models with S-Comm indicators not available for Panels E and F since they are not identified (the respective outcomes depend on S-Comm activation).

F Political Heterogeneity

Even a precisely estimated null effect might mask countervailing effects in different partisan contexts. Police in Republican-leaning counties might be more inclined to stop Latinos when immigration enforcement intensifies, while police in Democratic-leaning counties might be inclined to do the opposite. Conversely, we might expect police in Republican-leaning counties to resist sanctuary policies by making more stops and arrests of Latinos, while we might expect police in Democratic-leaning counties to work to implement sanctuary policies partly by reducing policing of Latino communities.

To test these possibilities, we evaluate whether the effects of S-Comm and sanctuary policies vary with McCain's vote share (at the county level) in the 2008 presidential election. To avoid post-treatment bias, we use McCain vote share; the 2008 presidential election occurred *prior* to the implementation of nearly all of the policies in our panel.

Generally, we find little evidence that the null effects of S-Comm and sanctuary policies vary with the political environment. We include full results below, in Tables 1, F8, and F9. We find no evidence that the effect of S-Comm on traffic stops of Latino drivers varies with the percentage of the county population that voted for McCain in 2008. Similarly, we find no evidence that sanctuary policies produced different effects in liberal vs. conservative counties, either for traffic stops of Latino drivers or arrests of noncitizens.

In sum, we find no evidence that our null results are driven by diverging patterns in different political environments.

F.1 Political Heterogeneity: Secure Communities Results

Table F7: Effect of Secure Communities on Stop Outcomes

Panel A: Log(Latino Stops + 1)	(1)	(2)	(3)	(4)
S-Comm	0.03	0.03	0.11	0.02
	(0.11)	(0.13)	(0.14)	(0.14)
S-Comm x % McCain	0.22	0.22	−0.27	−0.13
	(0.20)	(0.21)	(0.28)	(0.23)
R^2	0.87	0.87	0.90	0.92
Panel B: Pr(Latino)	(1)	(2)	(3)	(4)
S-Comm	-0.03^{*}	-0.03^{*}	0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.02)
S-Comm x % McCain	0.08^{**}	0.08^{***}	−0.03	−0.04
	(0.02)	(0.01)	(0.03)	(0.03)
R^2	0.95	0.95	0.97	0.97
N	4453	4453	4453	4453
County/Departments	61	61	61	61
Months	73	73	73	73
County/Department FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
State x Month FE	N	N	Y	Y
County/Department Trend	N	N	N	Y
State CSE	N	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of Secure Communities conditional on support for John McCain in the 2008 presidential election under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered standard errors instead of county/department clustered standard errors (Model 1). Model 3 adjusts for state × month fixed effects. Model 4 adjusts for a county/department-specific trend. Panels A and B effect estimates of Secure Communities using logged Latino stops and the probability a stop is Latino with non-Latino reference category as the respective outcome. Effects displayed in main text on Figure D7 are from column 3.

F.2 Political Heterogeneity: Sanctuary Policy Results

Table F8: Effect of Sanctuary Policies on Stop Outcomes Conditional On County-Level Republican Support

Panel A: Log(Latino Stops + 1)	(1)	(2)	(3)	(4)
Sanctuary	0.04	0.04	0.08	−0.31
	(0.07)	(0.06)	(0.08)	(0.19)
Sanctuary x % McCain	0.01	0.01	−0.02	0.45
	(0.16)	(0.11)	(0.10)	(0.43)
R^2	0.94	0.94	0.95	0.97
Panel B: Pr(Latino)	(1)	(2)	(3)	(4)
Sanctuary	-0.14^{**}	−0.14	−0.10	−0.02
	(0.05)	(0.08)	(0.06)	(0.04)
Sanctuary x % McCain	0.04	0.04^{**}	0.08^{*}	0.07
	(0.05)	(0.01)	(0.03)	(0.09)
R^2	0.95	0.95	0.96	0.97
N	11304	11304	11304	11304
County/Departments	157	157	157	157
Months	72	72	72	72
County/Department FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
State x Month FE	N	N	Y	Y
County/Department Trend	N	N	N	Y
State CSE	N	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of sanctuary policies conditional on support for John McCain in the 2008 presidential election under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered standard errors instead of county/department clustered standard errors (Model 1). Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county/department-specific trend. Panels A and B display effect estimates of sanctuary policies using logged Latino stops and the probability a stop is Latino with non-Latino reference category as the respective outcome. Effects displayed in main text on Figure D7 are from column 3.

F.3 Political Heterogeneity: IDENT Results

Table F9: Effect of Sanctuary Policies on Arrests Matched To ICE Databases

Panel A: Log(All Matches + 1)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.66^{***}	0.66	0.19	-0.21	-0.57
	(0.07)	(0.51)	(0.48)	(0.35)	(0.29)
Sanctuary * % McCain	-0.80^{***}	-0.80	-0.50	0.63	1.35^{**}
	(0.16)	(0.96)	(0.93)	(0.58)	(0.40)
N	26663	26663	26663	26663	26663
R^2	0.75	0.75	0.87	0.90	0.96
Panel B: Pr(Matches | Submissions)	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.00	0.00	0.00	0.00	
	(0.00)	(0.00)	(0.00)	(0.00)	
Sanctuary * % McCain	-0.00	-0.00	-0.01	-0.00	
	(0.01)	(0.01)	(0.01)	(0.01)	
N	19932	19932	19932	19932	
R^2	0.68	0.68	0.72	0.76	
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	N	N	Y	Y	Y
County Trend	N	N	N	Y	Y
S-Comm Indicator	N	N	N	N	Y
State CSE	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Model 1 evaluates the effect of sanctuary policies under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-5 use state clustered standard errors instead of county clustered standard errors. Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county-specific trend. Model 5 adjusts for an additional Secure Communities indicator. Panels A and B display effect estimates of sanctuary policies using logged IDENT matches and the probability a submission is a match as the respective outcome. Model with S-Comm indicator not available for Panel B since they are not identified (the outcome depends on S-Comm activation).