Immigration Enforcement, Policing, and Profiling

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Abstract

When does immigration enforcement cause police to target Latino drivers in traffic stops? We answer this question with two related studies. First, using a dataset covering dozens of large counties and evaluating the staggered onset of Secure Communities (increasing enforcement) and sanctuary policies (decreasing enforcement), we study the effect of immigration enforcement intensity—defined as the likelihood of deportation—on disparate traffic stops. We find no evidence that increased immigration enforcement intensity leads to more traffic stops of Latino drivers (either proportionally or in absolute terms) or that decreased immigration enforcement leads to fewer criminal arrests of noncitizens. But neither Secure Communities nor sanctuary policies directly engage local police. By contrast, in our second study, we evaluate the effect of a 2014 Texas initiative, Operation Strong Safety, in which police explicitly adopted immigration-related goals. We find strong evidence that Operation Strong Safety increased disparate stops of Latino drivers: its onset discontinuously increased traffic stop rates of Latino drivers and decreased citation and hit rates of those drivers. The contrast between these two results—a null effect of changing federal enforcement intensity and a large effect of a state program targeting immigrants—suggests that police respond to organizational incentives. Deportations are rare relative to traffic stops, and federal enforcement intensity is unlikely to affect local police behavior absent federal-local cooperation, but when police agencies directly adopt immigration goals, racial profiling can result.

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 have found that immigration enforcement does affect policing in the relatively few jurisdictions with explicit cooperation agreements ("287(g)" and/or intergovernmental service agreements) with federal immigration authorities [Armenta, 2017, Donato and Rodriguez, 2014, Coon, 2017, Pham and Van, 2022, Muchow, 2024]. Counties and states with those agreements choose to have their police forces enforce immigration laws, and scholars have found evidence that that choice leads to abusive police practices in immigrant communities [Armenta, 2017, Pham and Van, 2022] (but not always [van Tiem, 2023]).

We conduct two empirical studies to examine *under what conditions* immigration enforcement affects policing. Our results together suggest that we should expect immigration enforcement goals to affect police behavior where police departments themselves pursue immigration-related goals.

Our first study examines the effects of changes in federal enforcement intensity—the chance that a local arrest will lead to a deportation—on traffic stops and local arrests. Scholars have noted—but not tested[1]—the possibility that increased federal 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 our first study, and 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 federal 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

[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.

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 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.

In sum, the first study finds that a change in the already-small chance that a local arrest will lead to deportation does not affect police behavior. But the lack of an effect of this back-end change hardly suggests that immigration enforcement goals cannot lead to disparate policing. Our second study offers an example of immigration-enforcement-driven policing.

Our second study evaluates the effect of Texas's Operation Strong Safety program, adopted in 2014 in response to a spike in arrivals at the southern border. That program moved Texas Department of Public Safety resources to two heavily Latino counties (Hidalgo and Starr) along the southern border for the stated purposes of combating human and drug trafficking. We take advantage of the sudden implementation of this immigration-focused enforcement program (announced only two days ahead of time) to evaluate its effects. We observe a sudden, large jump in the number of stops of Latino drivers, with an accompanying sudden drop in the citation rate and the rate at which police discover contraband. Using a method proposed by Knox et al. [2020b] and Knox et al. [2020a], we argue that these findings imply an increase in racial profiling in traffic stops in Hidalgo and Starr counties.

Our two studies together shed light on when and how immigration enforcement may lead to racial profiling in traffic stops. Our first study finds that increases in the chance of deportation conditional on arrest do not, absent something more, change police behavior. But our second study shows that when police agencies dedicate themselves directly to immigration enforcement, racial profiling can result. Institutions matter: when the enforcement mandate comes from the police agency itself, police officers respond.

Existing Literature

These findings contribute to the literature on the causes of disparate policing as well as the literature on the effects of variation in immigration enforcement.

First, our results 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 traffic stops that might lead to deportations, particularly given that deportations are a rare consequence of arrests. By contrast, where a police agency adopts an immigration purpose explicitly—as during Operation Strong Safety in Hidalgo and Starr counties—officers do face an incentive to profile Latino drivers.

Second, 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 in our first study contrasts with the more established finding, which matches our second study, 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].

Our findings on the (non)effects of Secure Communities and sanctuary policies 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

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

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.

Our first study adds 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 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]).

Finally, our results also add to the small but growing literature on the partisan politics of local immigration enforcement. Our first study's 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 first study's 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] Our second study's result complicates this picture: Operation Strong Safety was highly politicized, with real effects.

Together, our findings contribute to the scholarship on the ways in which immigration enforcement and local policing are, and are not, intertwined. Secure Communities deportations produce their political and economic effects directly, through deportation, rather than indirectly, through changes in police behavior. Changes in police behavior, by contrast, arise when police departments directly pursue immigration aims.

1 Federal Enforcement and Local Policing

Our first study is of the effect of expanding and contracting federal immigration enforcement. We study two sets of policies that increased or decreased the chance of being deported, conditional on having been arrested by local police.

1.1 Context

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

[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].

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].

1.2 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 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.

1.3 Data

1.3.1 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

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

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

1.3.2 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

[9]In 2010, there were 50.5 million Latinos nationally.

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

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.

1.4 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:

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

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:

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

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.

1.5 Results

1.5.1 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 $\times$ 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.

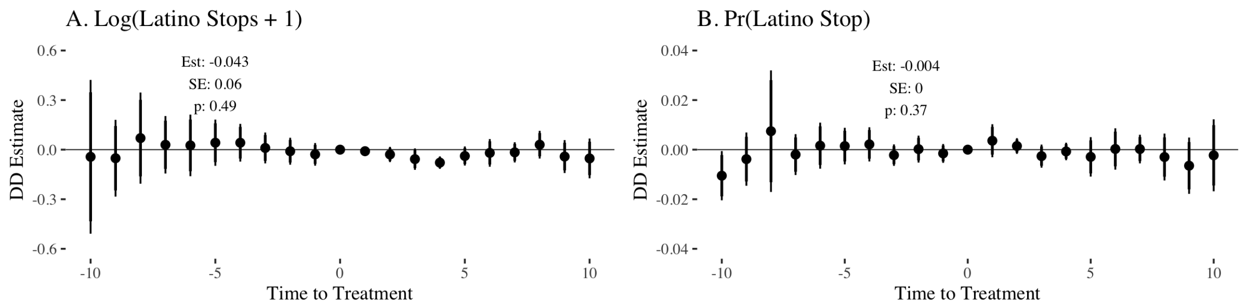

Figure 1: Event study estimates characterizing effect of Secure Communities (S-Comm). See Table 1 for corresponding difference-in-differences regression results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm. Binary 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.

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.

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).

[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.

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).

1.5.2 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 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).

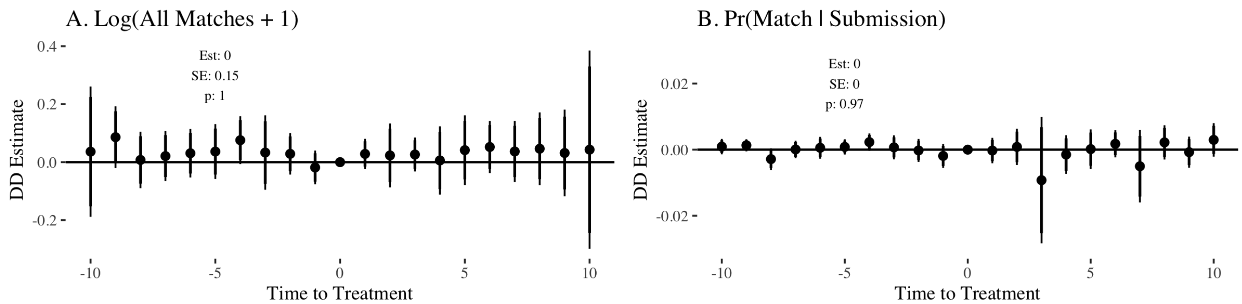

Figure 2: Event study estimates characterizing effect of sanctuary policy on IDENT outcomes. See Table 2 for corresponding difference-in-differences results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies. 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.

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.

2 Operation Strong Safety

Study 1 finds that immigration enforcement policies that increase the chance of deportation after arrest, but do not explicitly mandate shifts in street-level police behavior, do not lead to increased disparate policing of Latino communities. However, Study 1 has little to say about policies that *do* explicitly mandate and encourage shifts in street-level bureaucratic behavior. Do state or local policies with explicit immigration goals in facilitate disparate policing of immigrant ethnic groups? Studies of federal-local enforcement agreements suggest that the answer is yes; we find evidence consistent with these studies as well. We test the effect of "Operation Strong Safety (OSS)," a Texas state policy jointly implemented by the Texas Governor and Chief of the Texas Department of Public Safety (DPS) to increase traffic enforcement at the border for the purpose of fighting human smuggling, undocumented immigration, and drug trafficking. In previous work, one of us examined this program as part of a study of the effect of consent searches (without considering race) in Hidalgo and Starr counties [Dias et al., 2024]; we build on that work to study the statewide effect of that program, allowing us to draw inferences about disparate policing of Latino drivers.

2.1 Context

Operation Strong Safety (OSS) began on June 23, 2014, when the Texas DPS moved highway patrol officers from other counties into Hildalgo and Starr counties, along the Texas-Mexico border. The policy only became public two days before it took effect, and news coverage was limited; we therefore do not believe that there were any significant opportunities for drivers to anticipate the new policy. For a more detailed discussion of the context and the lack of media and Google search activity ahead of the policy onset, see Dias et al. [2024, 50-51, 89-90] and Figures G9 and G10 in the Appendix.

Although OSS did not formally target undocumented immigration itself, journalists and other observers reported that officers began to focus on unauthorized immigration—and stops of Latino drivers [del Bosque, 2018, Schladen, 2015, 2016, Aguilar, 2014]. In Hidalgo and Starr counties, which are overwhelmingly Latino, the stop rate more than doubled overnight (Figure G11). Moreover, Hidalgo and Starr counties saw a jump in the use of consent (as opposed to probable cause) searches and an accompanying decline in the rate at which those searches yielded contraband (the hit rate). Dias et al. [2024] demonstrate these patterns in the search and hit rate in Hidalgo and Starr counties; we go further by estimating the statewide effect of the policy, which also allows us to estimate the effect of the policy on stop, search, and hit rates by race.

In sum, OSS gave DPS traffic patrol officers an explicit mandate to redirect resources toward two predominantly Mexican-American Texas border counties and to engage in activities associated with federal border enforcement, such as the interdiction of drug trafficking, human smuggling, and unauthorized immigration. Consistent with journalistic accounts and prior empirical evidence, we expect OSS to increase disparate policing of Latinos within the Texas DPS highway patrol's jurisdiction.

2.2 Data and Design

We use Texas DPS highway patrol data from SOPP to evaluate whether OSS increased disparate policing of Latinos statewide. We use data on all DPS traffic stops between January

1, 2009-December 31, 2016 ($N = 15,753,883$). Importantly, SOPP has re-coded the Latino race variable so that stops that are reported by the DPS as non-Latino are re-classified as stops of Latinos if the subject stopped has a more than 75% chance of being Latino based on the joint probability of being Latino conditional on their surname and location-of-stop (county) [Imai and Khanna, 2016].[12] This adjustment is appropriate given that prior research has found that the Texas DPS often incorrectly classifies Latinos as "white" to manipulate traffic stop statistics by race/ethnicity [Luh, 2022].

We measure several outcomes from the DPS traffic stop data at the daily level. To measure whether OSS disparately increased policing of Latinos, we measure the proportion of traffic stops where the subject is Latino (*Pr(Latino)*). To assess whether the potential increase in *Pr(Latino)* is driven by an increase in policing in Hidalgo and Starr, we also measure the daily proportion of traffic stops that occurred in Hidalgo and Starr counties (*Pr(HS)*).

We use several measures to assess whether OSS increased unwarranted policing against Latinos within the Texas DPS. We measure two citation rates at the daily-level: 1) the proportion of Latino traffic stops that led to a citation as opposed to a warning (*Latino citation rate*) and 2) the proportion of white traffic stops that led to a citation as opposed to a warning (*white citation rate*). Citation rates offer an additional measure of disparate policing: a decrease in the *Latino citation rate* after OSS without a commensurate shift in *white citation rates* may suggest the DPS increasingly stopped Latinos for reasons unrelated to traffic violations.

We also measure two consent stop-and-search rates at the daily-level: 1) the proportion of searches of Latinos that were conducted on the basis of driver consent as opposed to probable cause (*Latino consent rate*) and 2) the same consent search proportion for white drivers (*white consent rate*).[13] Unlike probable cause searches, where officers must have reasonable suspicion in order to initiate a search, a consent search requires no justification as long as the officer asks for consent to search. Unsurprisingly, consent searches are less likely than probable cause searches to lead to identification of criminal activity [Dias et al., 2024]. Drivers rarely say no to the police even if the officer has limited cause to conduct a search [Dias et al., 2024, Sommers and Bohns, 2024].

In addition to consent stop-and-search rates, we measure daily contraband recovery stop-and-search rates for Latino and white stop-and-searches (*hit rate*). Given that OSS was

[12] SOPP uses Census data to estimate the probability that an individual is Latino based on surname and county of stop.

[13] These consent search measures (as well as the hit rate measure below) are similar to the measures in Dias et al. [2024], except that we calculate them statewide and are therefore able to calculate rates separately by driver race.

meant to identify drug trafficking, human smuggling, and unauthorized immigration, we define contraband as weapons, drugs, and/or (illicit) money in addition to the identification of human smuggling. Lower hit rates after OSS may suggest that searches became increasingly superfluous and unwarranted after OSS.

Finally, we measure the proportion of Latino stop-and-searches that are the result of racially disparate policing. To do this, we follow Knox et al. [2020b] and Knox et al. [2020a] and rely on three relatively reasonable assumptions: 1) **mandatory reporting** (police report stop-and-searches when they happen): 2) **mediator monotonicity** (there are no circumstances in which a white person would be stopped-and-searched but an identically situated Latino would be allowed to not be searched conditional on a stop); and 3) **treatment ignorability** (no factors jointly affected the onset of Operation Strong Safety and the *Latino/white hit rate* (more on this later). The quantity of interest (*anti-Latino bias*) is the difference in the Latino and white hit rate normalized by the white hit rate:

$$\frac{\mathbb{E}[Y_i|D_i = 0, M_i = 1] - \mathbb{E}[Y_i|D_i = 0, M_i = 1]}{\mathbb{E}[Y_i|D_i = 0, M_i = 1]}$$

Where Y_i is the hit rate, D_i is an indicator for race/ethnicity where 1 = Latino, 0 = white, and M_i is an indicator for being stopped-and-searched. Under the assumptions outlined above, the quantity of interest is smaller than or at least equal to $\mathbb{E}[M_i(1) - M_i(0)|D_i = 1, M_i = 1]$, the probability that a stop-and-search would have not occurred if a white subject experienced the same circumstances as a Latino subject.

Given we expect OSS to increase disparate policing of Latinos, we expect OSS to: increase *Pr(Latino)*; increase *Pr(HS)*; reduce the *Latino citation rate* while having no commensurate effect on the *white citation rate*; increase the *Latino consent rate* while having no commensurate effect on the *white consent rate*; decrease the *Latino hit rate* while having no commensurate effect on the *white hit rate*; and increase *anti-Latino bias*.

Since the outcomes are measured at the day level and the unit of analysis is the date ($N = 2557$ days between January 1, 2009-December 31, 2015), our independent variable of interest is a binary indicator equal to 1, 0 otherwise, if the date is after June 23, 2014, the day Operation Strong Safety was implemented (*OSS*).

We use a regression discontinuity-in-time (RDiT) design to assess the discontinuous, immediate effect of *OSS* on our outcomes. The RDiT is an advantageous design because our coefficient estimates are less likely to be affected by secular differential time trends independent of *OSS*, and the unanticipated nature of *OSS* makes it reasonable to assume that driver characteristics or other factors associated with our outcomes of interest did not change immediately before and after *OSS* (e.g. the propensity to engage in criminal

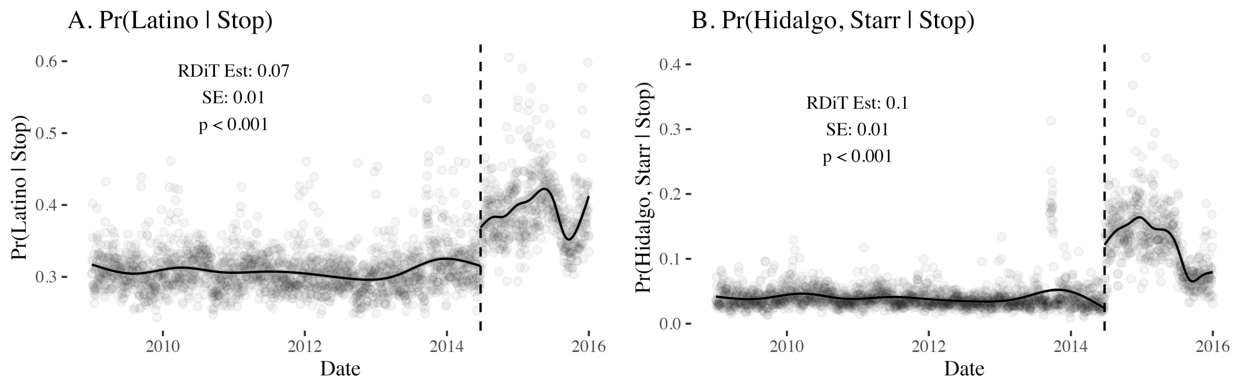

Figure 3: *OSS* discontinuously increased Latino stops. Panels A-B characterize the proportion of Texas DPS stops that are Latino and occurred in Hidalgo/Starr counties. Dashed vertical line denotes *OSS* onset. Loess lines fit on each side of the *OSS* discontinuity. Annotations denote mean-squared optimal bandwidth RDiT estimate (polynomial = 1, uniform kernel).

activity by race/ethnicity, weather, the ethno-racial distribution of the driving population). Continuity in driver characteristics by race/ethnicity is important for estimating shifts in anti-Latino bias because we are not necessarily interested in the effect of being Latino on our outcomes of interest (e.g. the citation, consent, and contraband recovery rate), but the *change* in the effect of being Latino discontinuously after OSS is implemented due to shifts in the operational priorities of the Texas DPS. Given the unanticipated and sudden nature of OSS, we assume that the effect of Latino ethnicity would have been the same after June 23rd as before OSS if OSS had not occurred. We present mean-squared optimal bandwidth RDiT estimates [Calonico et al., 2015], with the running variable (days to *OSS*) to the 1st polynomial and a uniform kernel.

2.3 Results

Figure 3 displays RDiT estimates characterizing the effect of *OSS* on the outcomes of interest in addition to the daily outcome level between Jan. 2009-Dec. 2015. Consistent with our hypothesis, *OSS* discontinuously increased the proportion of DPS traffic stops across Texas that involved Latino drivers by 7 percentage points (pp., $p < 0.001$), a substantively large effect equivalent to 2.2 standard deviations of the pre-*OSS* daily outcome distribution (Figure 3, Panel A). As expected, the increase in the proportion of Latino stops across the Texas DPS was driven by a discontinuous 9 pp. increase in the proportion of traffic stops in Hidalgo and Starr counties ($p < 0.001$), equivalent to an extremely large 5 standard deviations of the pre-*OSS* outcome distribution (Figure 3, Panel B).

Moreover, the increase in the Latino stop proportion accompanied an increase in stops of

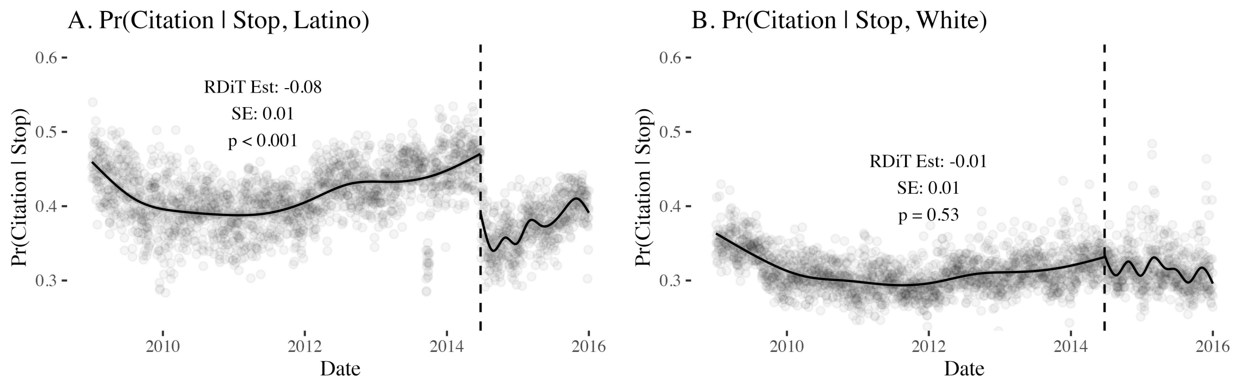

Figure 4: *OSS* discontinuously reduced the citation rate. Panels A-B characterize the proportion of stops that led to citations (instead of warnings) for Latinos and whites. Annotations denote mean-squared optimal bandwidth RDiT estimates (polynomial = 1, uniform kernel).

Latino drivers that yielded neither citations nor contraband. Across Texas, *OSS* discontinuously decreased the *Latino citation rate* by 8 pp. post-*OSS* ($p < 0.001$), a substantively large 2 standard deviations of the pre-*OSS* daily outcome distribution (Figure 4, Panel A). At the same time, *OSS* did not reduce the white citation rate (Figure 4, Panel B), suggesting that *OSS* disparately affected the Latino population and did not shift officer behavior toward white drivers.

OSS also discontinuously increased the *Latino consent search rate* by 10 pp. ($p < 0.01$) while reducing the *Latino hit rate* by 10 pp. ($p < 0.001$), equivalent to 0.72 and 1 standard deviation of the pre-*OSS* outcome distributions respectively (Figure 5, Panels A-B). But *OSS* did not change the *consent search rate* or *hit rate* for whites (Figure 5, Panels C-D). These results further suggest that *OSS* reduced the legal threshold to initialize a search conditional on a stop for Latinos but not whites. Commensurately, the decrease in probable cause for search initialization of Latinos post-*OSS* resulted in a decline in the identification of relevant contraband.

Finally, we use the approach proposed by Knox et al. [2020b] and Knox et al. [2020a] to assess whether *OSS* increased biased stop-and-searches against Latinos. RDiT estimates suggest that *OSS* discontinuously increased the rate of biased stop-and-searches (the difference between the Latino and white hit rate, normalized by the white hit rate) by 21 pp. ($p < 0.001$), nearly 88% of the pre-*OSS* rate of biased stop-and-searches (Figure 6).

In summary, *OSS*, an explicit mandate to pursue immigration-related policing from the Texas State Governor and Texas Department of Public Safety Chief, resulted in clear, discontinuous shifts in the Latino stop rate, citation rate, and hit rate. Under reasonable assumptions, this shift in policing priorities led to more biased policing of the Latino popu-

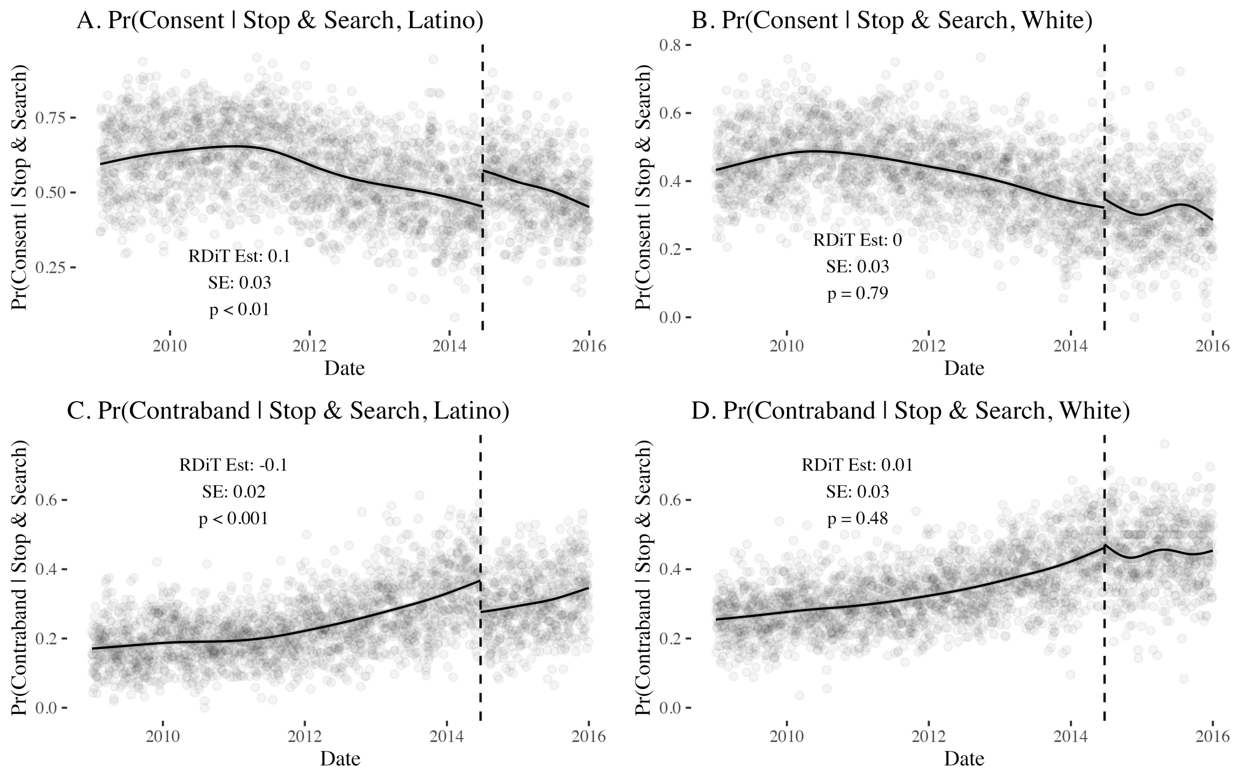

Figure 5: *OSS* discontinuously increased the consent search rate and decreased the hit rate. Panels A-B show the consent search rate for Latinos and whites over time, Panels C-D show the hit rate for Latinos and whites over time. Dashed vertical line marks *OSS* onset. Loess lines are fit on each side of the *OSS* discontinuity. Annotations show mean-squared optimal bandwidth RDiT estimates (polynomial = 1, uniform kernel).

lation in Texas.

2.4 Robustness Checks

Our estimates are substantively and statistically similar if we use alternative polynomial (quadratic, cubic), kernel (triangular, Epanechnikov), and bandwidth (dividing optimal bandwidth by half) specifications (Figure G12), suggesting that our results are not driven by researcher choice in model specification. Our estimates are larger than the vast majority of fake pre-*OSS* temporal placebo discontinuities (Figure G13), suggesting that our results are not a function of statistical chance. Our estimates are the same after removing observations near the discontinuity subject to anticipatory effects (i.e. a "donut-hole" RDiT) [Bajari et al., 2011], suggesting that our results are not affected by potential anticipatory effects (Figure G14).

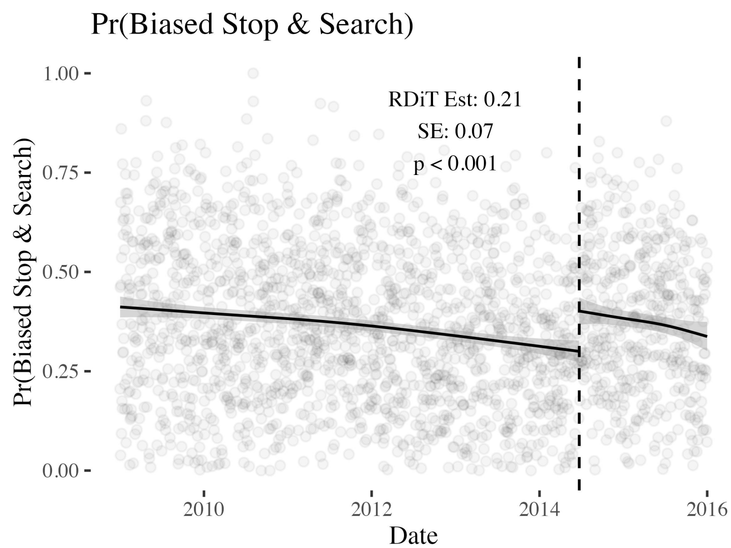

Figure 6: OSS discontinuously increased discriminatory stop-and-searches against Latinos (relative to whites). Dashed vertical line denotes OSS onset. Loess lines fit on each side of the *OSS* discontinuity. Annotations denote mean-squared optimal bandwidth RDiT estimate (polynomial = 1, uniform kernel).

Discussion

We present two studies of the effect of immigration enforcement on policing in two different contexts, with widely differing results.

In our first study, we evaluate whether heightened federal 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.

In our second study, we evaluate the effect of a state program with immigration-enforcement-related goals. Texas's Operation Strong Safety, which shifted Department of Public Safety resources toward two overwhelmingly Latino counties, also dramatically increased stops of Latino drivers overnight. As numbers of Latino stops jumped, citation and hit rates suddenly fell, suggesting that the new stops were less effective than those that came before. Using an approach with (we think) reasonable assumptions, we conclude that biased searches of Latino drivers increased.

Why did immigration enforcement drive biased policing in one context but not another? We look to differing organizational incentives for police officers. When federal enforce-

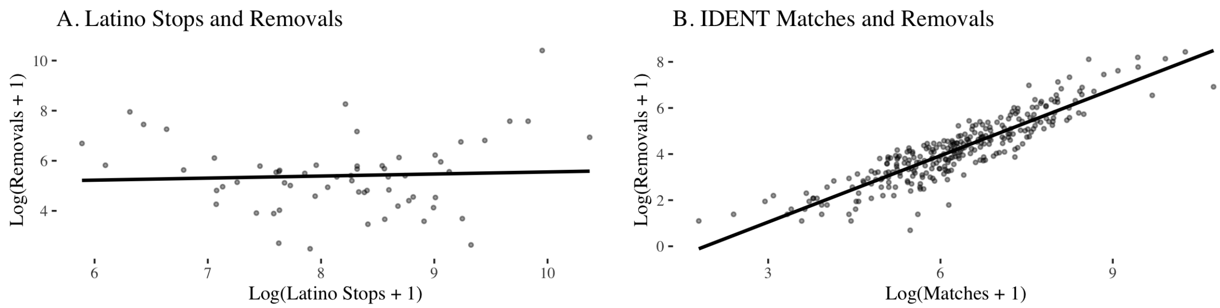

Figure 7: Association between Latino stops, IDENT matches, and removals.

ment intensity changes—even if that change reflects a local jail's refusal to honor detainer requests—police officers receive no automatic mandate to change their behavior. By contrast, Texas's Operation Strong Safety involved exactly that: a mandate to focus policing on immigration goals.

Police officers' attention to organizational incentives is consistent with our 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 7, Panel B, shows that correlation. Even though arrest rates are highly associated with deportation rates, rising deportation rates do not lead police officers to make more arrests, absent an organizational incentive to do so. Typically, deportations depend on arrests, not vice versa.

Our two findings matter 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—and Operation Strong Safety—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.[14].

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 addition, a key limitation of our first 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 arrests. 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 7, 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.

Of course, when a police agency makes traffic stops a key component of an immigration-related campaign, immigration goals can lead to disparate policing even absent a strong link between traffic stops and deportations. That is exactly what we observe in Operation Strong

[14]Examples include the Maricopa County's Sheriffs Office (discussed above) in addition to Operation Strong Safety.

Safety, and what other scholars have found in the context of cooperative agreements between federal and local agencies [Armenta, 2017, Donato and Rodriguez, 2014, Coon, 2017, Pham and Van, 2022, Muchow, 2024].

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Appendices

Contents

A County Distribution of State Patrol Officers

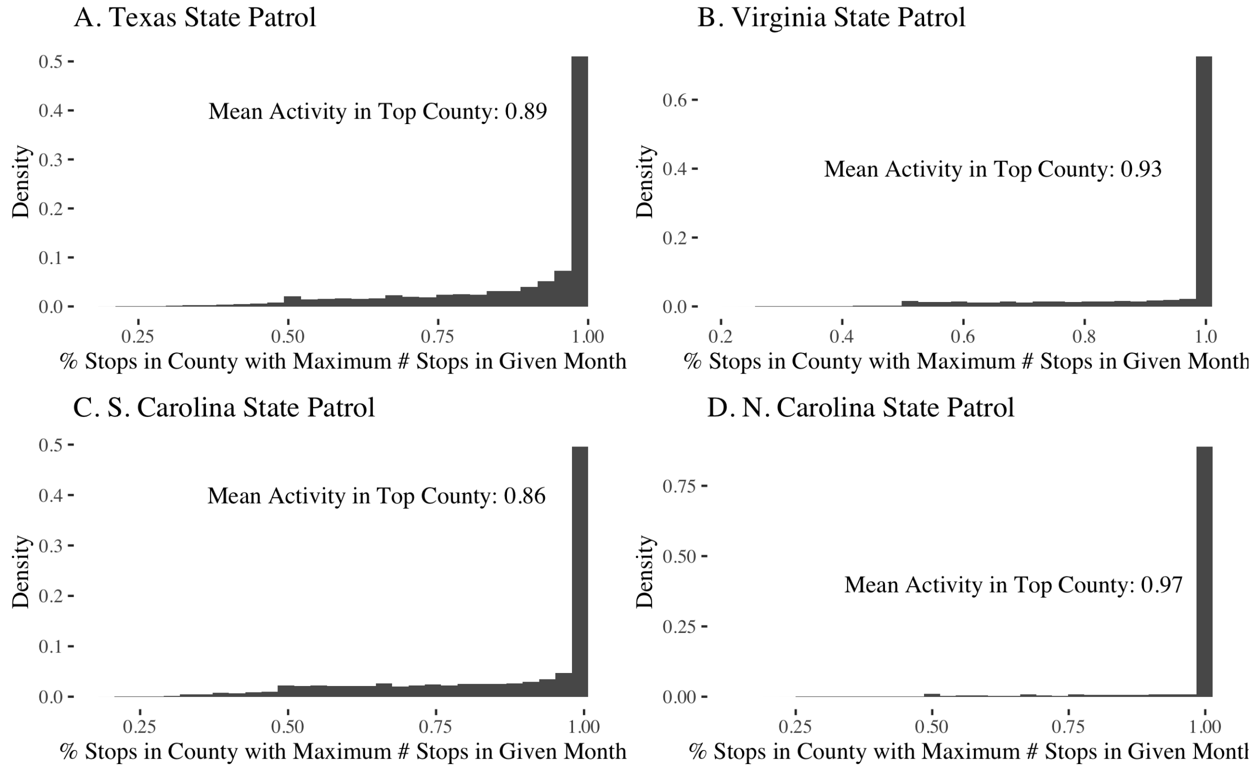

Figure A1: Histogram characterizing proportion of time spent in county with maximum level of stop activity (x-axis) at the officer/month level for State Patrol departments (2008-2015).

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

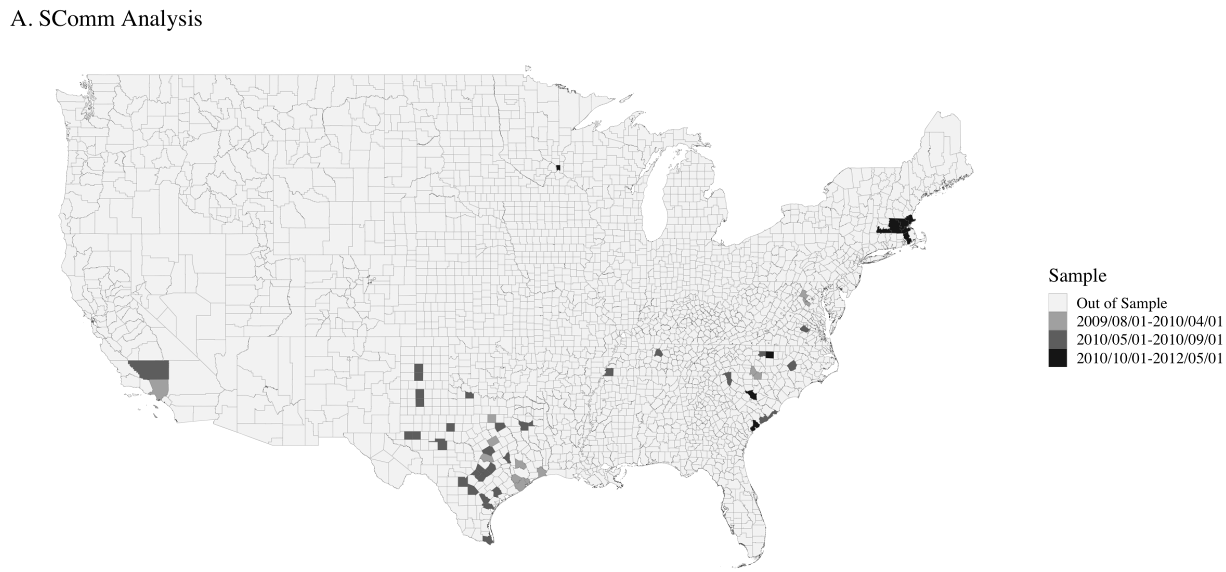

Figure B2: Map Characterizing Sample and Treatment Status Across Counties

B.2 Sanctuary Analysis

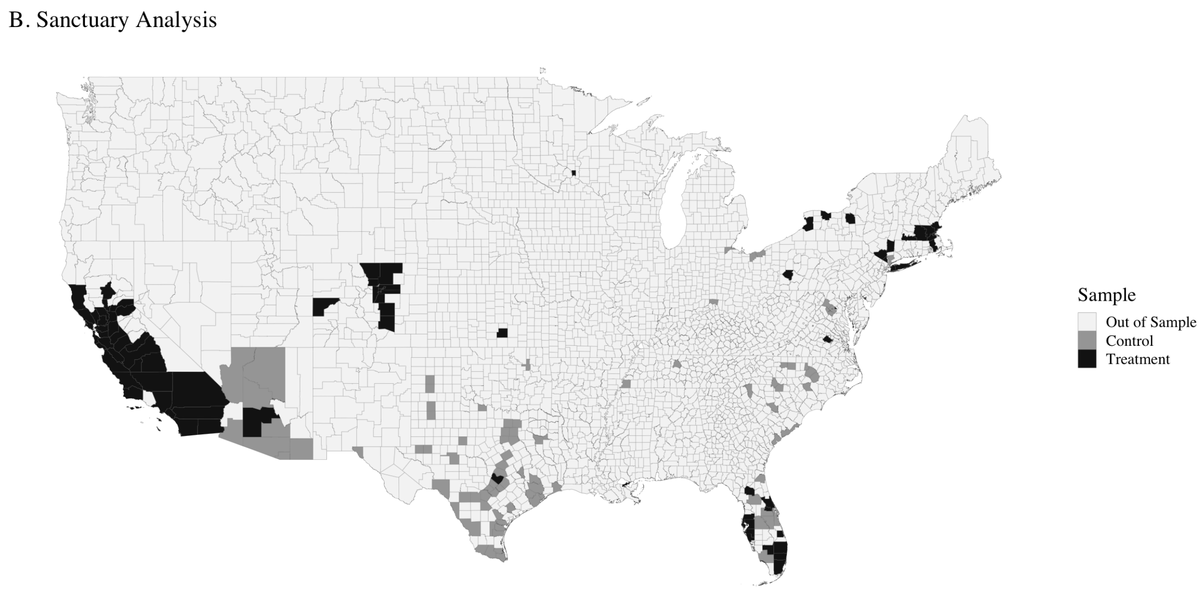

Figure B3: Map Characterizing Sample and Treatment Status Across Counties

B.3 IDENT Analysis

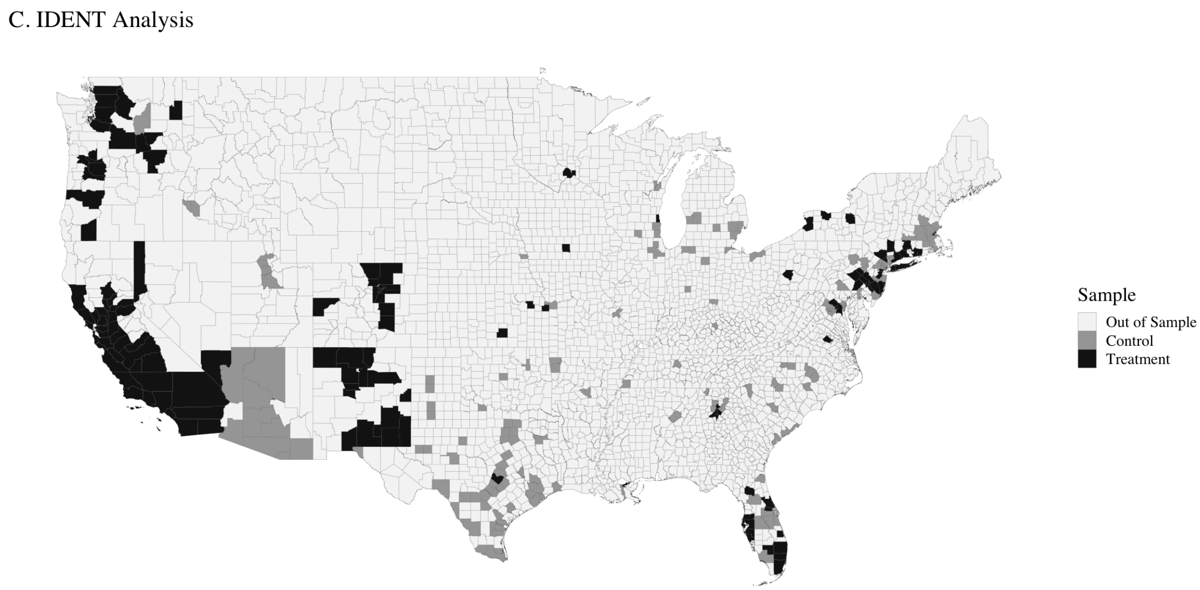

Figure B4: Map Characterizing Sample and Treatment Status Across Counties

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

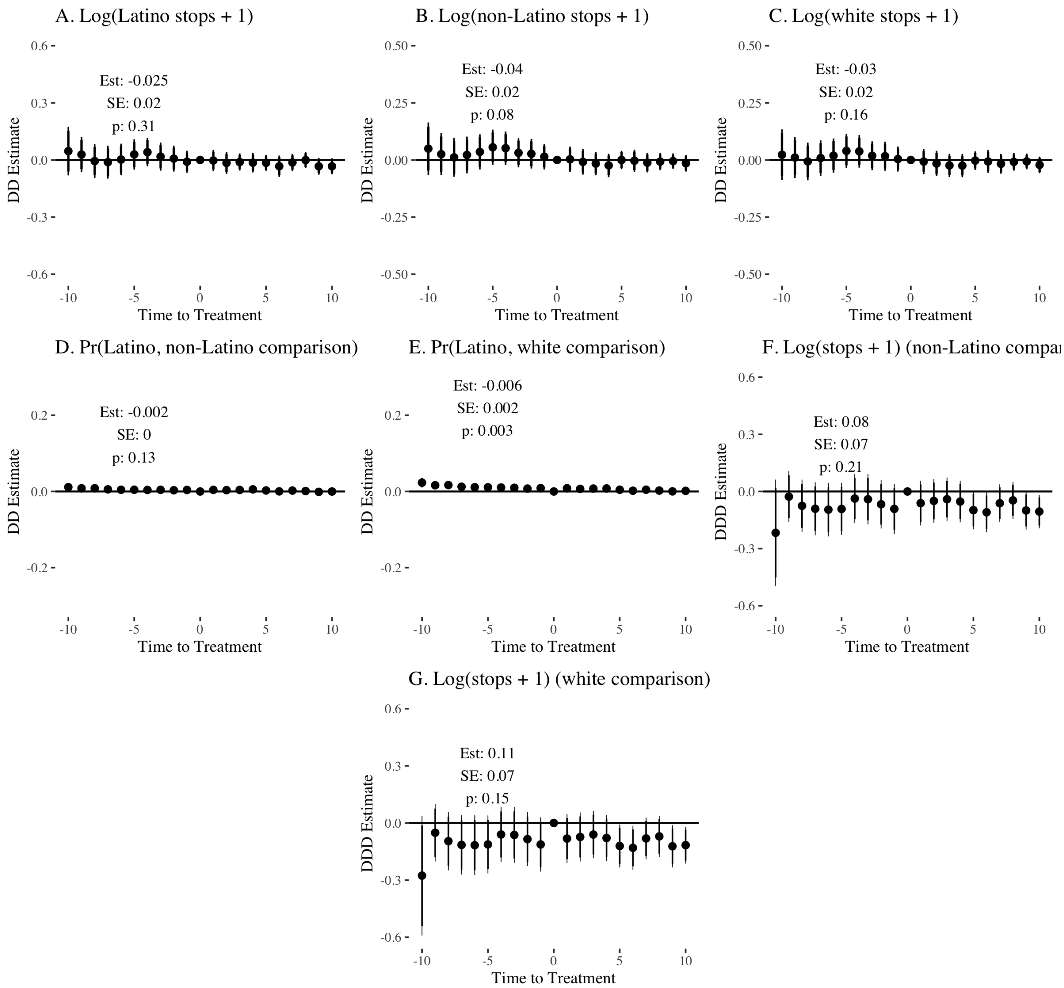

Figure C5: Event study estimates characterizing effect of Secure Communities (S-Comm) on California Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies for Panels A-E. For Panels F-G, 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

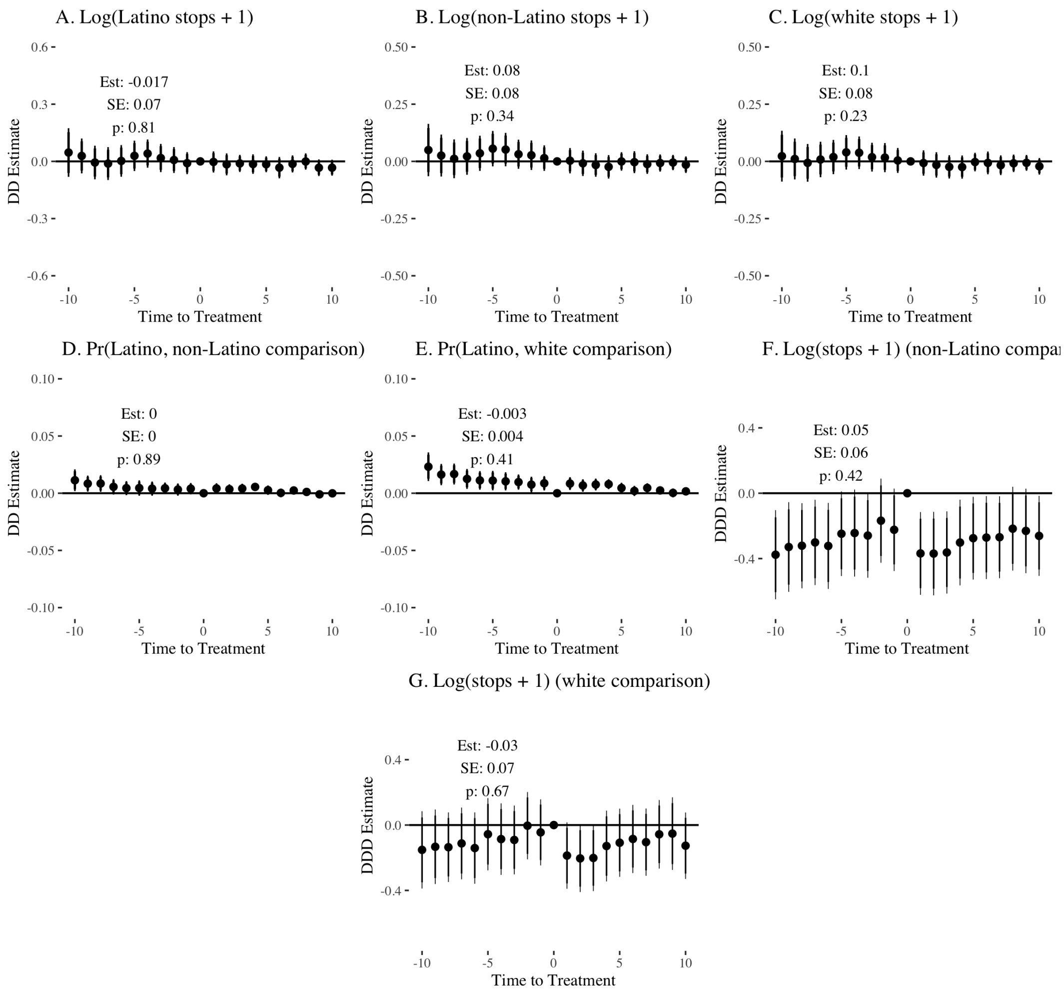

Figure C6: Event study estimates characterizing effect of Secure Communities (S-Comm) on North Carolina Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies for Panels A-E. For Panels F-G, 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,

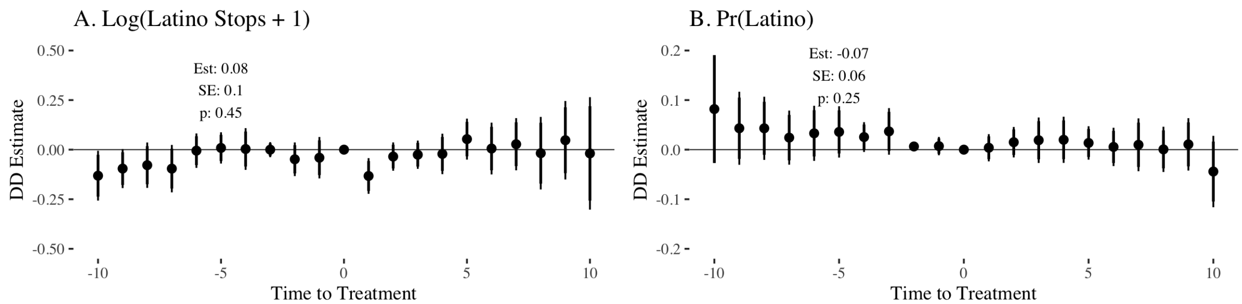

Figure D7: Event study estimates characterizing effect of sanctuary policy. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies. Binary indicators characterizing time to policy are equal to 1 on any month before/after 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).

G Study 2

G.1 OSS Salience

G.1.1 Media Coverage

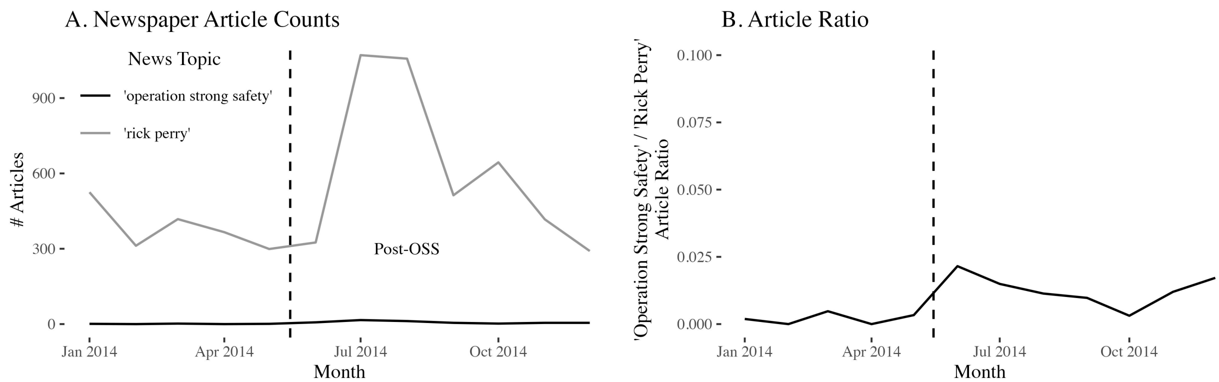

Figure G8: Media Coverage of Operation Strong Safety in 2014.

G.1.2 Search Interest

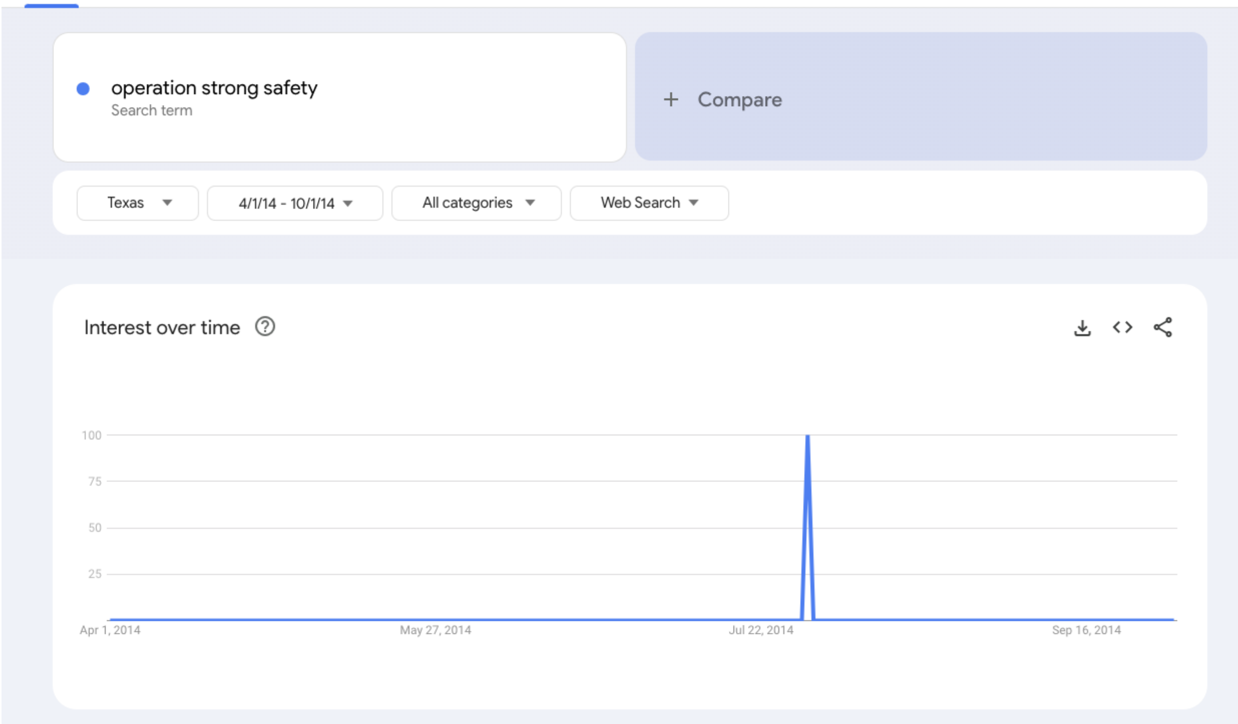

Figure G9: There was very limited search interest in Operation Strong Safety in the three months before and after the onset of the policy in Texas State.

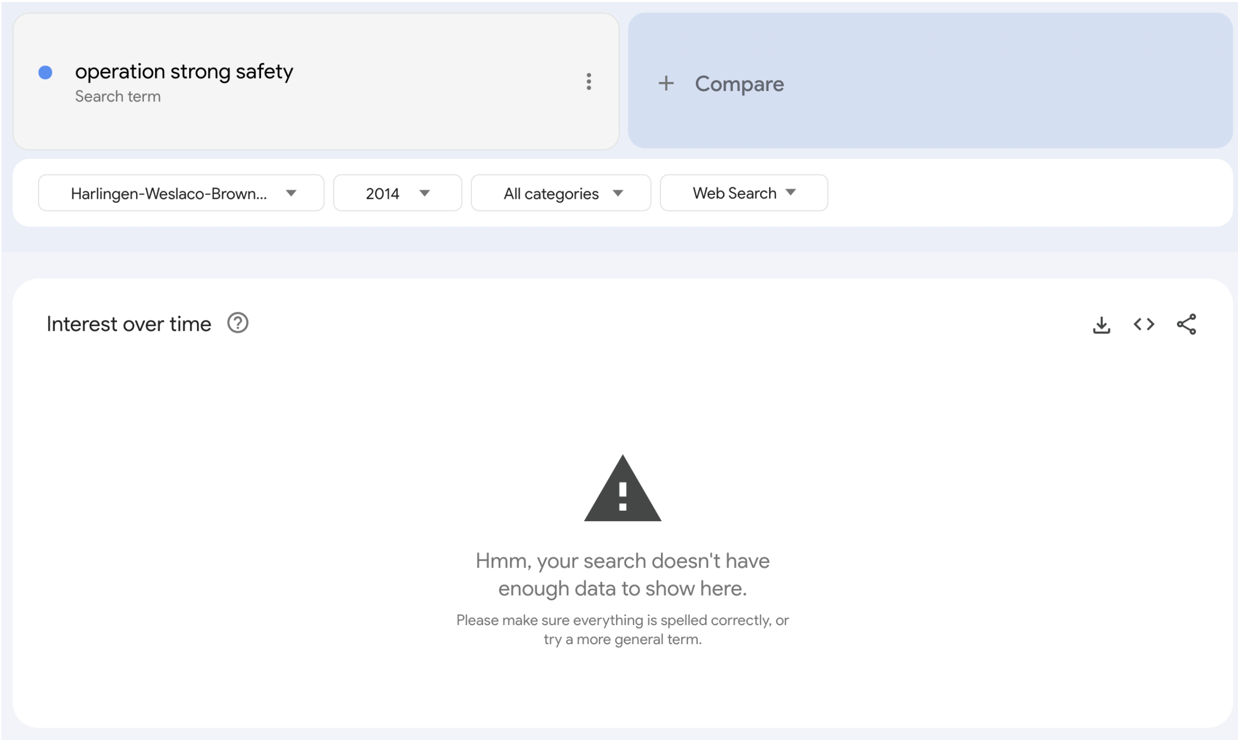

Figure G10: There was very limited search interest in Operation Strong Safety in the three months before and after the onset of the policy in Texas State.

G.2 Effect of OSS on # Stops in Hidalgo and Starr

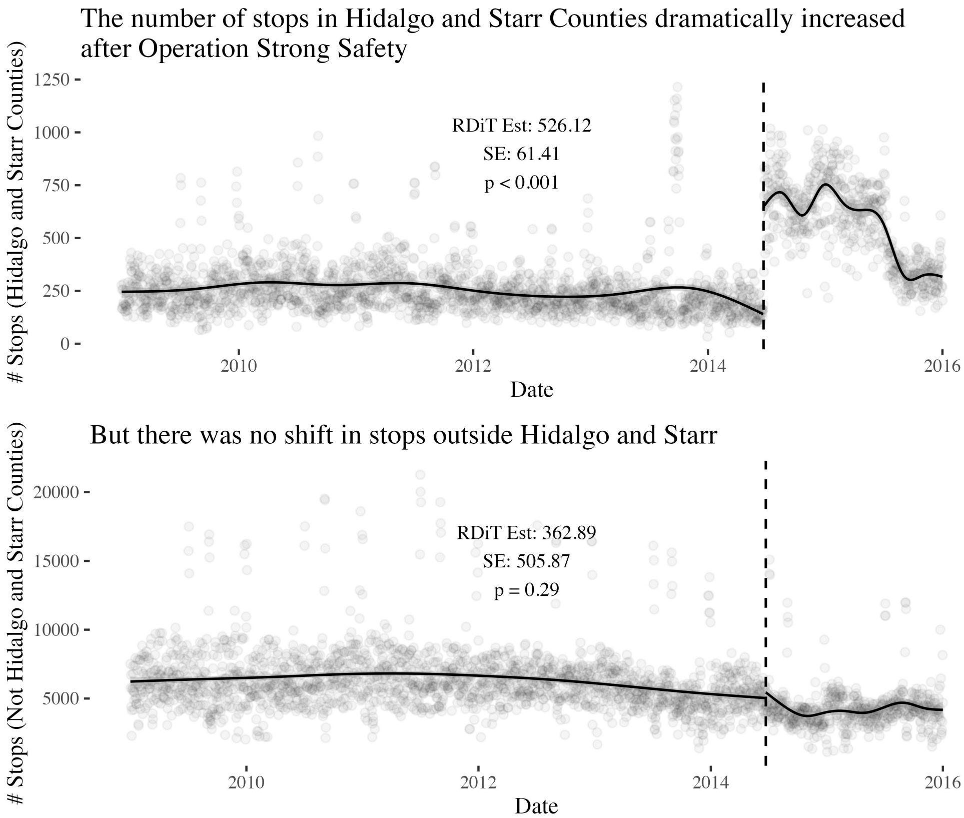

Figure G11: *OSS* dramatically increased the number of stops in Hidalgo and Starr counties.

G.3 Alternative RDiT Specifications

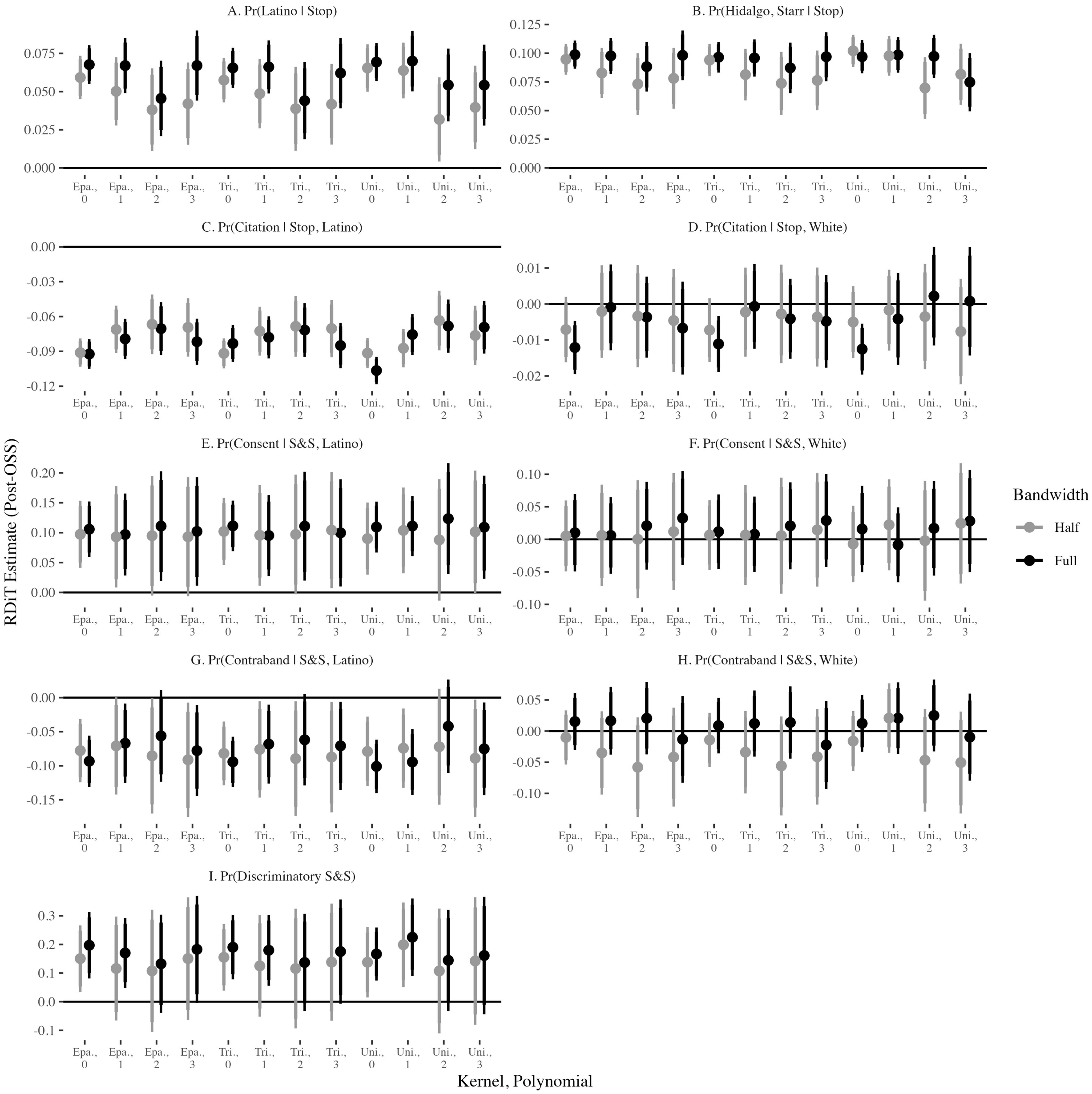

Figure G12: Alternative RDiT specifications by kernel, polynomial (x-axis) and bandwidth (denoted by color). Each panel denotes a different outcome analyzed. 95% CIs displayed from robust SEs

G.4 Temporal Placebos

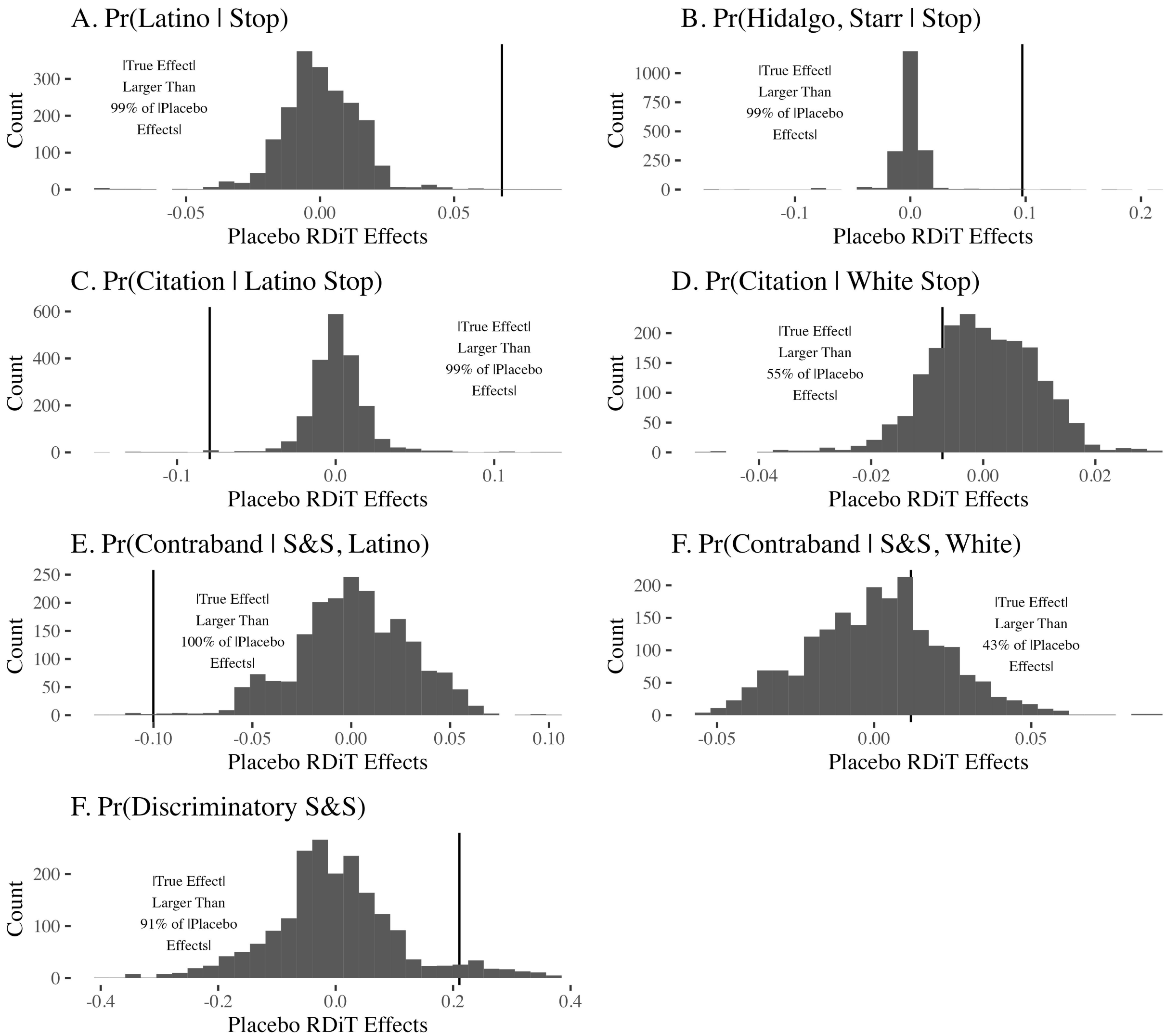

Figure G13: Pre-*OSS* temporal placebo effects. Each panel denotes a different outcome analyzed. Dashed vertical line denotes the true post-*OSS* effect. Annotation denotes the proportion of placebo effects (in absolute value) the true post-OSS effect (in absolute value) is larger than.

G.5 Donut Hole Re-estimation

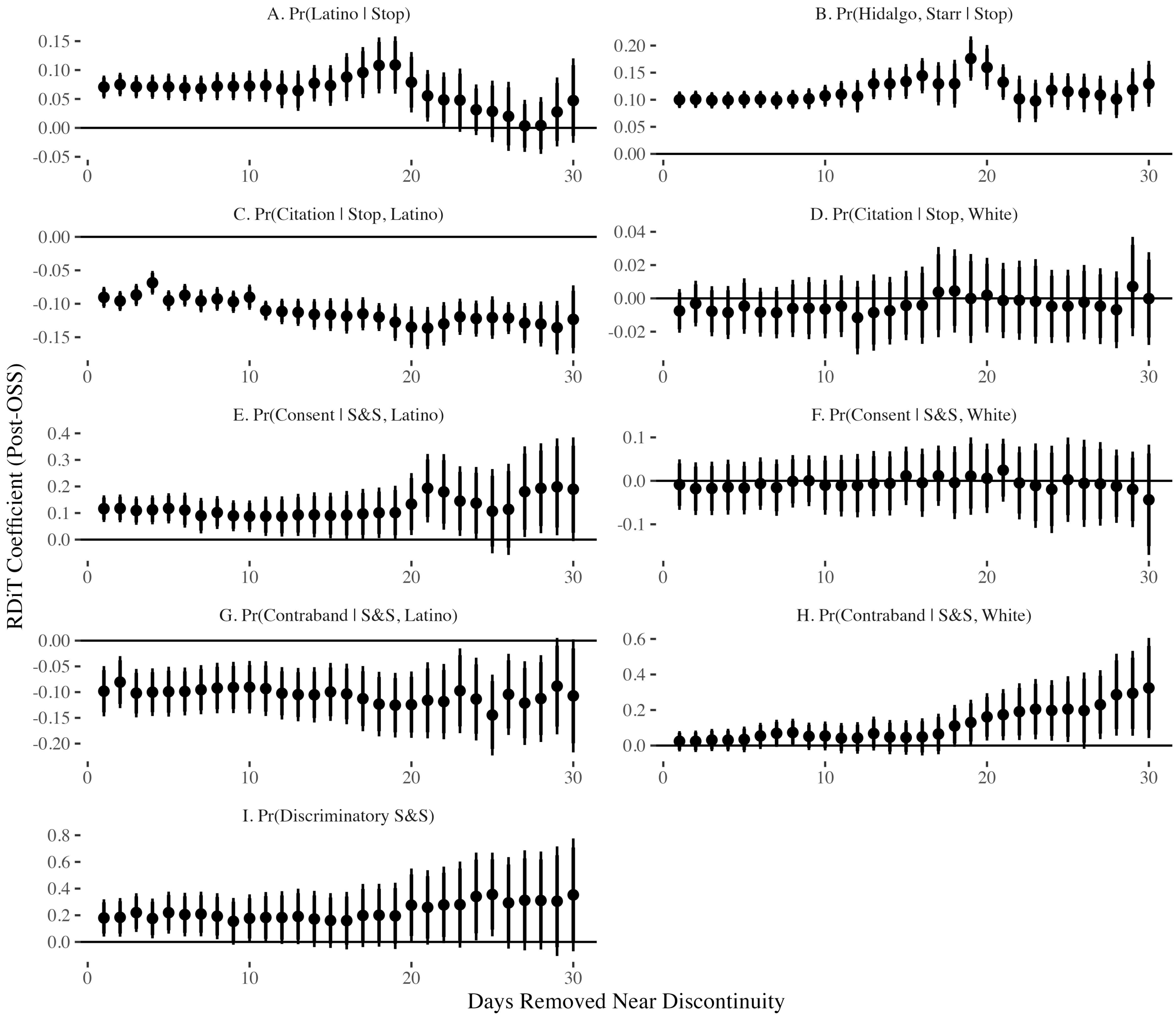

Figure G14: Donut hole RDiT effect (x-axis) re-estimation after removing days near discontinuity (y-axis). Each panel denotes a different outcome analyzed. 95% CIs displayed from robust SEs