Immigration Enforcement, Policing, and Race

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Abstract

When does immigration enforcement lead to racially disparate policing? We address this question with two studies. First, evaluating the staggered onset of Secure Communities and sanctuary policies, we study the effect of immigration enforcement intensity on traffic stops. We find no evidence that increased enforcement leads to more traffic stops of Latino drivers or that decreased enforcement reduces non-citizen criminal arrests. However, neither policy directly engages local police. Our second study examines a 2014 Texas state police initiative, Operation Strong Safety (OSS), which increased border enforcement via traffic stops in two border counties. We find that OSS, which increased stops in two overwhelming Latino counties, mechanically led to more stops of Latino drivers statewide, while decreasing statewide citation and contraband seizure rates and providing no public safety benefits. Our results show that institutional contexts shape the relationship between immigration enforcement policies and racially disparate policing.

Short Title: Immigration Enforcement, Policing, and Race

Teaser: Different immigration enforcement policies have distinct effects on racially disparate local policing.

Introduction

The second Trump administration has made cooperation with local police a linchpin of its mass deportations effort (Castellano, 2025). A 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 (until very recently) 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, see Tiem (2023)).

We conduct two empirical studies of immigration enforcement programs that depended on arrests by state or local police. Together, our results generate a hypothesis, which we hope that future research will test, about *under what conditions* immigration enforcement produces racially disparate effects. We 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—the possibility that increased federal immigration enforcement creates incentives for police to stop and arrest people they suspect are noncitizens (Eagly, 2010; Kubrin, 2014; Kohli, Markowitz, and Greenberg, 2011). The only existing study of this question finds results consistent with ours, but lacks a measure of policing outcomes for Latinos (Treyger, Chalfin, and Loeffler, 2014).[1]

The hypothesis is that, when the probability of transfer from local criminal custody to

[1] An unpublished thesis also finds consistent results in North Carolina alone (Willoughby, 2015).

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 local to federal custody and thereby reduce deportations (D. K. 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 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) (Ciancio and García-Jimeno, 2022).

We find no evidence that Secure Communities or counteracting sanctuary policies affected police traffic stops of Latino drivers or decisions to arrest non-citizens. These null results do not depend on the local political environment: results are similar in counties that favored Republican presidential candidates in 2008 or 2016 relative to those that favored Democratic presidential candidates. 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 changes in the already-small chance of a local arrest leading to deportation do not affect police behavior. But, this null effect hardly implies that immigration enforcement cannot produce disparate policing. Our second study illustrates how immigration-focused policing can mechanically generate disparate effects by shifting police resources to areas with large Latino populations.

Our second study evaluates the effect of Texas's Operation Strong Safety (OSS) program, adopted in 2014 in response to a spike in arrivals at the southern border. Prior research (including one of us) examined OSS's effect on consent searches in the two counties where it operated (Dias et al., 2024); here, we extend that work to evaluate its *statewide* effect on stops of Latino drivers. The program moved Texas Department of Public Safety officers to two heavily Latino counties (Hidalgo and Starr) along the southern border, ostensibly to fight human and drug trafficking. Taking advantage of the program's sudden implementation (announced only two days in advance), we observe an immediate, large jump in the proportion of stops involving Latino drivers statewide—reflecting the diversion of resources to overwhelmingly Latino counties and illustrating how immigration-focused enforcement can mechanically increase racially disparate policing. Expanding on Dias et al. (2024), we also show that OSS *decreased policing quality*, producing sudden drops in citation and contraband recovery rates.

Our two studies together suggest a hypothesis, which we hope others will test, about when and how immigration enforcement may lead to racially disparate 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, racially disparate policing can result.

Existing Literature

Our 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 growing evidence suggesting that police officers are sensitive to incentives set by 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 faced an incentive to engage in lower-quality policing—and were required to begin making stops in these heavily Latino counties, leading mechanically to a racially disparate effect.

Second, we add to work on the drivers of immigration enforcement (Cox and Miles, 2013; D. K. 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 existing 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); see also Magazinnik (forthcoming).

Our findings on the (non)effects of Secure Communities and sanctuary policies add to the literature on the harms of deportations, suggesting that those harms are imposed directly, through threatened and actual deportations, rather than indirectly, through changes in police behavior. This is likely both for the observed political effects of enforcement and its observed economic effects.

First, our results suggest that the political effects of increased immigration enforcement reflect increased deportations rather than changes in policing. Political scientists have found that immigration enforcement, as well as immigrant-hostile laws and proximate experiences with the deportation system, have a mobilizing effect. Deportations, immigrant-hostile laws, and proximate experiences with the deportation system have been found to increase Latino voter turnout (White, 2016), political knowledge (Pantoja, Ramirez, and Segura, 2001; Pantoja and Segura, 2003), Democratic party support (Bowler, Nicholson, and Segura, 2006; Israel-Trummel, Shortle, and Bracic, 2025), and protest participation (Zepeda-Millán, 2017; Walker, Roman, and Barreto, 2020) (but see Altema McNeely, D. Kim, and M. Kim (2022), who find proximate experiences with deportation depress turnout). Additionally, qualitative evidence suggests that immigration-related policing can play a key mobilizing role for social movements: immigrant communities in Maricopa County, for example, organized to counter abusive police practices there that targeted Latino citizens and noncitizens (Abrams, 2022).

Similarly, the economic and health costs of enforcement likely reflect the effects of deportations rather than changes in policing. These costs are well established: scholars show that increasing local immigration enforcement causes reductions in employment (East et al., 2018), student achievement (Bellows, 2019), school enrollment (Dee and Murphy, 2020), social service uptake (Alsan and Yang, 2019; Watson, 2014), and birth weight (Amuedo-Dorantes, Churchill, and Song, 2020). These findings depend on variation in the level of immigration enforcement of the type 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 by testing a mechanism through which immigration enforcement might produce these many effects. Because Secure Communities relies on local police arrests, it could harm immigrant communities either through increased deportations or 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). Harms to citizens could reflect changes in policing: some 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, Markowitz, and Greenberg, 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, LeBrón, and Pedraza, 2018), and many scholars suggest that the political effects of immigration enforcement reflect the "racialized threat" of that enforcement (Nichols and Valdéz, 2020). We test that hypothesis by examining the effects of variation in immigration enforcement on Latino traffic stops and noncitizen arrests.

Our results are consistent with those of other studies finding little effect of immigration enforcement on criminal justice outcomes. Some research, Treyger, Chalfin, and Loeffler (2014) and Hines and Peri (2019), for example, finds 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, Chalfin, and Loeffler, 2014; Masterson and Yasenov, 2021)

Finally, our results also add to the 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 high and 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. Our second study's result complicates this picture: Operation Strong Safety was highly politicized, with real effects.

Together, our findings contribute to scholarship on how 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 resource allocation and behavior, by contrast, arise when police directly pursue immigration aims. As the second Trump administration pursues more collaboration with local police, our results should spur research on the effects of that collaboration on policing.

Study 1: Federal Enforcement and Local Policing

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

Context

In order to find and deport noncitizens living within the United States—as opposed to noncitizens who recently crossed the border—the federal government relies overwhelmingly on arrests by local police (Cantor, Ryo, and Humphrey, 2019). That means that the large majority of Immigration and Customs Enforcement (ICE) arrests take place in jails and prisons, rather than at large.[2] This reliance on criminal arrests for interior deportations means that immigration and criminal enforcement are necessarily linked. 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 (D. K. 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 an arrested person by local police, their fingerprints are automatically sent to the FBI, where they are matched against FBI databases and the Department of Homeland Security's Automated Biographic Identification System (IDENT) (DHS, 2011). The IDENT database is drawn principally from Custom and Border Protection (CBP) records of noncitizens' entry into the U.S., including apprehensions of people attempting to cross the border between ports of entry (DHS, 2012). IDENT also contains 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 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 (DHS, 2011). This process produces the database matches that we treat as a proxy for the number of noncitizen arrests

[2] This pattern may be changing with increasing use of ICE raids under the second Trump administration, but (as mentioned above) the new administration has also focused on increasing arrests through local cooperation.

in each county and month.

If ICE decides—after receiving a database match from the FBI—to attempt to deport an arrestee, it typically issues a detainer request (ACLU, n.d.). Such a request asks the county jail to continue to imprison the noncitizen for up to 48 hours beyond when they otherwise would be released. Detainers are intended to make ICE arrests (transfers from local criminal custody to federal immigration custody) easier: when county jails comply with these requests, ICE has more 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 (D. K. Hausman, 2020). The details of sanctuary policies vary from jurisdiction to jurisdiction; following D. K. Hausman (2020), we code counties as sanctuary counties if their policies include refusals to comply with ICE detainer requests.

Finally, critically, 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, local officials can perform immigration arrests outside of jails as well (Pham and Van, 2022, pp. 469–70).

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 have entered into cooperative agreements with federal immigration enforcement authorities. Perhaps the best known example involves Maricopa County.[3] There, soon after Sheriff Joe Arpaio entered a 287(g) agreement with ICE, sheriffs' deputies began to organize saturation patrols, which resulted in disproportionate traffic stops and arrests of Latino drivers.

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.

Data

Secure Communities Data

We merge data on the county-level onset of Secure Communities (S-Comm) with traffic stop data from the Stanford Open Policing Project (SOPP) to evaluate whether S-Comm shifted

[3]Melendres v. Arpaio, 989 F.Supp.2d 822 (2013).

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 a department/county. Therefore, we include only counties/departments in which S-Comm activation occurred after July 2009. Third, consistent with our sanctuary policy data detailed in the Sanctuary Policy Data section, we use information from the top 10% of Latino counties (by population proportion) in 2010.

These criteria construct our sample. Because the SOPP data is relatively limited in time, we only have data on 10 states, 12 police departments (including 6 state highway patrols: MA, NC, SC, TN, TX, VA), and 58 counties. But these data capture a significant proportion of the Latino population. These data cover 8.6 million Latinos based on 2010 ACS estimates, equivalent to roughly 17% of the Latino population.[4] Our data includes demographically relevant counties: Los Angeles (CA), San Francisco (CA), Tarrant (TX), Cameron (TX), and Kern (CA). See Appendix Figure A1 for a map characterizing geographic coverage of the Secure Communities traffic stop data. For each county/department/month, we count the number of stops of Latinos, non-Latinos, and whites. See Appendix Table B1 for summary statistics of our S-Comm data.

Sanctuary Policy Data

We merge county sanctuary policy onset data from D. K. Hausman (2020) with traffic stop data from the SOPP to evaluate whether sanctuary policies change police behavior. The sanctuary data includes information from all but 12 of the 314 largest 10% of counties by Latino population between 2010-2015. After merging the sanctuary and SOPP data, we have a 72 month panel including 141 unique counties and 29 unique police departments (11 state patrols: CA, CO, FL, MA, NY, NC, OH, SC, TN, TX, VA). These counties cover

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

51% of the Latino population and include localities with significant Latino populations: Los Angeles, Houston, Dallas, San Antonio, and San Diego.[5] See Appendix Figure C2 for a map characterizing geographic coverage of the Sanctuary traffic stop data. For each county/department/month, we count Latino, non-Latino, and white stops. See Table D2 for summary statistics from our Sanctuary and stops data. Much of our traffic stop data comes from state patrols instead of local police, but state police are nonetheless likely to have relationships with local county jails given individual state police primarily operate within a single county (see Appendix Figure E3).

We merge the sanctuary policy data with data from the Department of Homeland Security's IDENT database. The database includes information on the number of noncitizen arrestees whose information was submitted to ICE to verify immigration status and the number of noncitizen arrestees whose information was matched to an ICE database after submission (i.e. 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 data merged with the IDENT data captures 80% of the overall Latino population in 2010. See Appendix Figure F4 for a map characterizing geographic coverage of the IDENT data. We construct two outcomes from this data. The first is the logged count of ICE database matches (+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 that the police are arresting more noncitizens. See Appendix Table G3 for summary statistics of our Sanctuary and stops data.

Estimation Strategy

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

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

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

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) in 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 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 standard errors clustered by county (Model 1) or state (Models 2-4); some sanctuary policies were adopted directly by state governments or many counties within a state simultaneously, and state patrol stop behavior may also change at the state level (D. K. 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:

$$Y_{cdm} = \sum_{k \neq -1}^{k} \beta^k \text{P}^k_{cdm} + \alpha_{cd} + \gamma_m + \delta_{sm} + \varepsilon_s$$

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 when 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 are equal to one. For the IDENT analysis, cd is simply c for county.

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 S-Comm under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 cluster SEs by state instead of county/department (Model 1). Model 3 adjusts for state × month fixed effects. Model 4 adjusts for a county/department trend. Panels A-B display S-Comm effect estimates on logged Latino stops and the probability that a stop involves a Latino driver respectively. Figure 1 effects are from column 3.

Results

Secure Communities and Traffic Stops

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

First, we find an imprecise null effect of S-Comm on Latino stops. Our preferred difference-in-differences estimate suggests that S-Comm insignificantly decreases Latino traffic stops by 4% ($p = 0.49$, see Table 1, Panel A, Model 3). The effect is equivalent to 12 fewer stops within a given county/department/month relative to a pre-treatment baseline of 383 traffic stops. A standardized effect of 0.20 is often considered "small" (Cohen, 2013). Here, the 95% CIs of the standardized effect of S-Comm on logged Latino stops do not overlap with ± 0.2 (Lakens, Scheel, and Isager, 2018): the standardized S-Comm effect on logged Latino stops is -0.05, and the standardized confidence intervals cover -.17-0.07. This equivalence

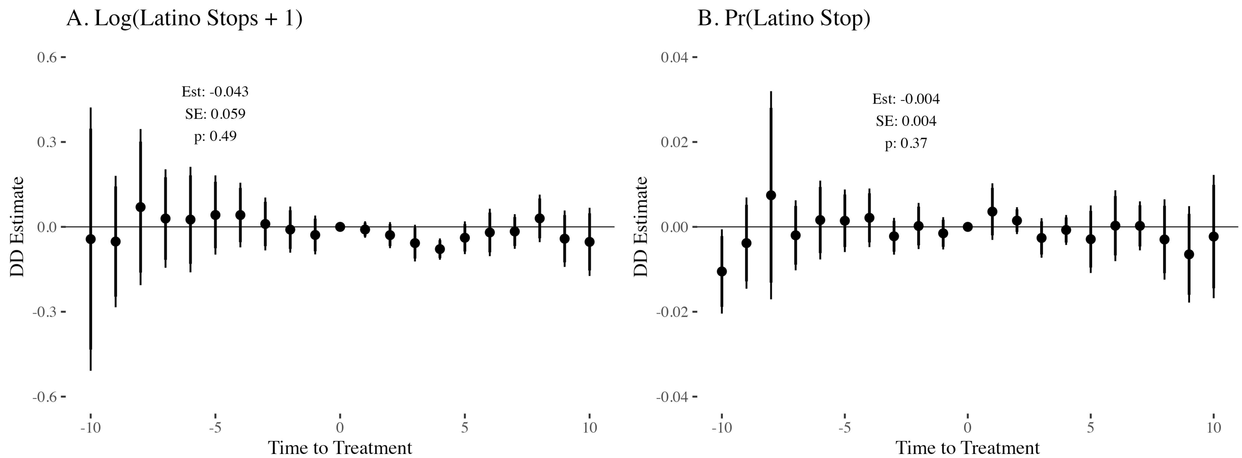

Figure 1: Event study estimates characterizing effect of Secure Communities (S-Comm). See Table 1, Column 3 for corresponding difference-in-differences regression results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm. 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.

test bolsters our confidence in this null result.

Second, we find no evidence that S-Comm changes the chance that a traffic stop involves a Latino driver. S-Comm insignificantly decreases the proportion of stops that are Latino by 0.4 percentage points ($p = 0.37$)—1.3% of the pre-treatment mean (22%) and zero given rounding in Table 1, Panel B, Model 3. 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. These estimates are precise: a single percentage point change in the proportion of stops involving Latinos is barely inside the 95% confidence interval (-0.012-0.004). Given that the standardized effect of S-Comm on the proportion of stops that are Latino is -0.02 and the standardized confidence interval for the effect is -0.07-0.03, an equivalence test assuming a small effect of ± 0.20 standard deviations rules out large effects. This equivalence test also increases our confidence in the null result.

Event study estimates corroborate our main results (Figure 1). First, treated counties/departments and untreated counties/departments possess largely similar outcome trends prior to S-Comm for Latino stops (Panel A) and the Latino stop proportion (Panel B), suggesting that yet-to-be-treated county/departments serve as a valid counterfactual.[6] Second, consistent with the main findings, post-treatment coefficients are largely statistically null.

Given S-Comm's staggered implementation across U.S. counties, we address the risk that heterogeneous treatment effects by county activation cohort bias our results (through comparisons between not-yet-treated and already-treated county/departments). We use the difference-in-differences estimator by Callaway and Sant'Anna (Callaway and Sant'Anna, 2021), which limits the usage of problematic comparisons with already-treated units, and derive statistical results that are similar to our main specification (Appendix Figure H5). Our null effects also replicate using only state patrol traffic stop data with all California and North Carolina counties, suggesting that our results are not driven by sample truncation to the top 10% Latino U.S. counties (Appendix Figures I6 and J7). Prior research shows that police may misreport the race of Latinos they stop to mitigate racial bias patterns in traffic stop data (Luh, 2022). The Texas state patrol SOPP data includes names of those stopped, allowing SOPP to correct Latino race/ethnicity misclassification with name and geography data. We replicate our approach using only the corrected Texas state patrol data in all Texas counties and still uncover null results for Latino stops,[7] suggesting that our overall results are not driven by racial misclassification from police after SComm implementation (Appendix Fugre K8). Our results are not driven by outcome measurement: the null effects of S-Comm replicate if we use the inverse hyperbolic sine of Latino stops, raw Latino stops, and the number of Latino stops normalized over the 2010 Census Latino population as the outcome (Appendix Table L4).

Secure Communities, by integrating FBI and ICE databases, increased the chance that

[6] Control counties/departments are "yet-to-be-treated" since Secure Communities is eventually activated in all U.S. counties.

[7] The Texas state patrol results for the Latino stop proportion outcome are problematic due to visible pre-SComm differential trends in the event study, so we do not attach significant weight to them.

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 general difference-in-differences effect of sanctuary policies without higher dimensional fixed effects. Models 2-5 use state instead of county clustered SEs. Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county-specific trend. Model 5 adjusts for a S-Comm indicator. Panels A and B display effect estimates of sanctuary policies on logged IDENT matches and the probability a submission is a match respectively. Model with S-Comm indicator not available for Panel B since the outcome depends on S-Comm activation.

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. As a substantial additional robustness check, we also study the effect of sanctuary policies, which decreased transfers to immigration custody, on stops of Latino drivers, and we find a similar (though less precise) null result. We show this analysis in the Appendix (see Appendix Section N, Appendix Table N6, Appendix Figure N9).

Sanctuary Policies and Arrests of Noncitizens

To obtain a more precise estimate of the effect of changing immigration enforcement intensity on police behavior, we assess whether sanctuary policies affected the number of police arrests of noncitizens. As a measure of these arrests, we use IDENT matches; 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, which provide variation in enforcement intensity after that rollout. If sanctuary policies caused widespread changes in police officers' stop

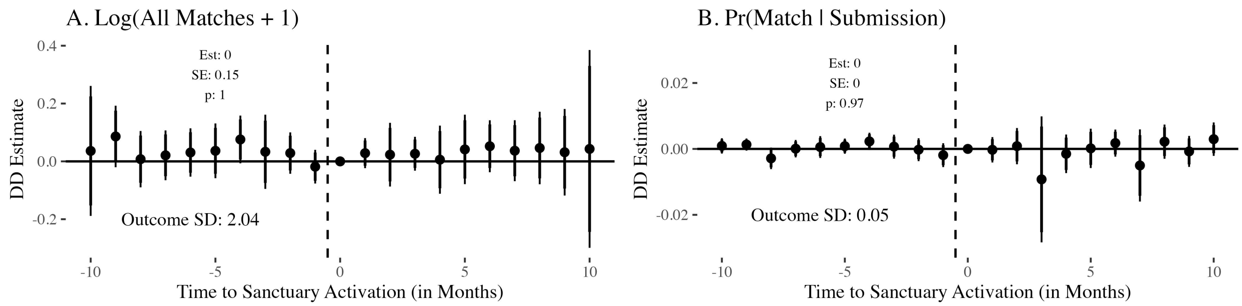

Figure 2: Event study estimates characterizing effect of sanctuary policy on IDENT outcomes. See Table 2, Column 3 for corresponding difference-in-differences results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary 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.

behavior, we would expect to see changes in the number of noncitizen arrests.

We find no evidence that sanctuary policies changed the number of arrests of noncitizens (i.e. IDENT matches, Table 2, Panel A, Model 3) or the proportion of arrests involving noncitizens (i.e. IDENT matches as a proportion of IDENT submissions, Table 2, Panel B, Model 3). The effect on the IDENT match proportion is precise, with changes of no 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 sanctuary policy onset, there are no differential outcome trends in counties that are about to adopt sanctuary policies and those that are not (see Figure 2). Nor is there 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. The event study estimates, at least for the logged IDENT matches outcome, are also robust to re-estimation using the Callaway and Sant'Anna difference-in-differences estimator (Callaway and Sant'Anna, 2021) (Appendix Figure O10).[8] Finally, our IDENT match outcome results also hold regardless of the seriousness of the crime

[8] We cannot re-estimate our models using the Callaway and Sant'anna estimator when the outcome is the proportion of IDENT matches as a proportion of IDENT submissions since the outcome necessitates the usage of an imbalanced panel (the outcome depends on S-Comm activation).

for which a noncitizen was arrested (Appendix Table P7).

Taking these three sets of results together, we find no evidence of any systematic effect of enforcement intensity on police stops or arrests. Our null result is not masking heterogeneous effects by politics or demography. In Appendix Section R (see also Appendix Tables S9-U14), we test for heterogeneous effects of S-Comm or sanctuary policies conditional on county Republican vote share and demographics (% Latino, % immigrant, % non-citizen). We find limited heterogeneous effects, solidifying our null result.

Study 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 does not examine policies that *do* explicitly mandate and encourage shifts in street-level policing. Do state or local policies with explicit immigration goals facilitate disparate policing of immigrant ethnic groups? Studies of federal-local enforcement agreements suggest that the answer is yes (Armenta, 2017; Donato and Rodriguez, 2014; Coon, 2017; Pham and Van, 2022; Muchow, 2024). In Study 2, we add to this evidence. We test the effect of "Operation Strong Safety (OSS)," a Texas state policy implemented by the Texas Governor and Texas Department of Public Safety (DPS) Chief to increase traffic enforcement at the border for the stated purpose of fighting human smuggling, drug trafficking, and undocumented immigration. Prior research including one of us examined this program to study the effect of consent searches (not considering race) in Hidalgo and Starr counties (Dias et al., 2024); we extend that work to study the *statewide* effect of OSS on the disparate policing of Latino drivers, which followed from the sudden diversion of policing resources into two overwhelmingly Latino counties.

Context

OSS began on June 23, 2014, when the Texas DPS moved highway patrol officers from other counties to Hidalgo and Starr counties, along the Texas-Mexico border (see Dias et al. (2024), Figure C1 for a map of the OSS area of operations, Figure C2 for state spending trends on border enforcement pre- and post-OSS). The policy only became public two days before taking effect, and both news coverage and information-seeking related to OSS was limited; we therefore do not believe that there were opportunities for drivers to anticipate the new policy. For a more detailed discussion of OSS and the lack of media and Google search activity ahead of the policy onset, see pp. 50-51, 89-90 in (Dias et al., 2024).

Although OSS did not formally target undocumented immigration itself, journalists and other observers reported that officers began to focus on unauthorized immigration—by making far more stops in Hidalgo and Starr counties (Bosque, 2018; Schladen, 2015; Schladen, 2016; Aguilar, 2014). In Hidalgo and Starr counties, which are overwhelmingly Latino, the number of stops more than doubled overnight (see Dias et al. (2024), Figure C3). Moreover, prior research by one of us showed that Hidalgo and Starr counties saw a jump in the use of consent (instead of probable cause) searches and an accompanying decline in the rate at which those searches yielded contraband (the hit rate) (Dias et al., 2024). We build on that prior work by estimating the effect of OSS on stop, search, and hit rates by race statewide. These effects are driven by the changes in Hidalgo and Starr counties that Dias et al. (2024) uncovered; our new contribution here is to show how the large changes in those two counties led to racially disparate and lower quality policing against Latino drivers statewide.

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.

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 01/01/2009 to 12/31/2015 ($N = 15,753,883$). Importantly, SOPP re-coded the Latino race variable so that stops that are reported as non-Latino are re-classified as Latino stops 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 county-of-stop (Imai and Khanna, 2016).[9] This adjustment is appropriate since prior research finds the Texas DPS often incorrectly classifies Latinos as "white" to manipulate stop statistics (Luh, 2022).

We measure several daily outcomes from the DPS stop data. To assess whether OSS increased disparate policing of Latinos, we measure the proportion of traffic stops where the subject is Latino (*Pr(Latino)*). To assess whether the statewide increase in *Pr(Latino)* reflects increased policing in Hidalgo and Starr counties, we measure the daily proportion of stops occurring there (*Pr(HS)*).

We use several measures to assess whether OSS increased unwarranted policing against Latinos. We measure two daily-level citation rates: 1) the proportion of Latino traffic stops resulting in a citation rather than a warning (*Latino citation rate*) and 2) the corresponding proportion for non-Latino stops (*non-Latino citation rate*). Citation rates proxy for disparate and unwarranted policing: a decrease in the *Latino citation rate* post-OSS without a commensurate shift in *non-Latino citation rates* may suggest that DPS increasingly stopped Latinos for reasons unrelated to actual traffic violations. Note that this pattern may emerge without disparate decisions at the individual stop level if police begin making many unwarranted stops in predominantly Latino counties. Whether the disparate policing occurred individually or through decisions to shift resources geographically, the result is still more unwarranted stops of Latino drivers.

We also measure two daily consent stop-and-search rates: 1) the proportion of searches of

[9] SOPP uses Census data to do this.

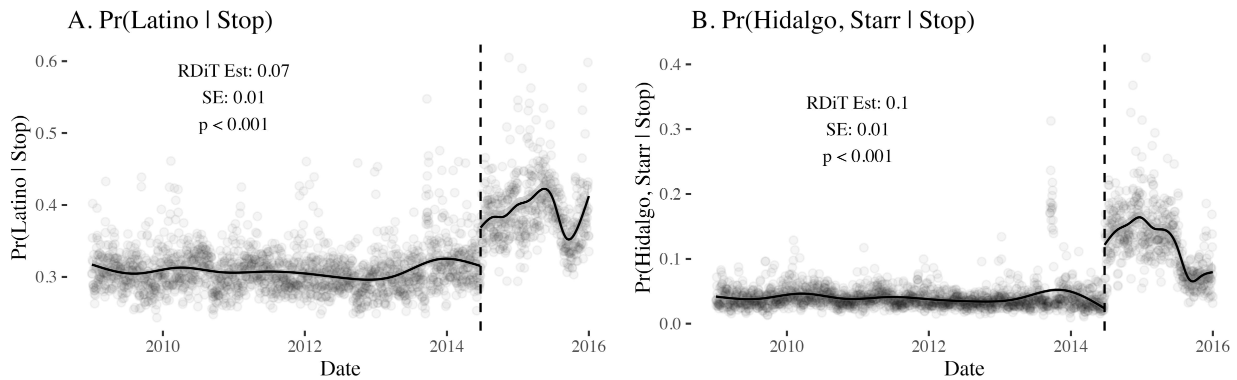

Figure 3: *OSS* discontinuously increased Latino stops in Texas. Panel A shows the statewide proportion of DPS stops that were of Latino drivers, and Panel B shows the proportion of statewide stops that occurred in Hidalgo/Starr counties. Dashed vertical line = *OSS* onset. Loess lines fit on each side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

Latinos conducted on the basis of driver consent rather than probable cause (*Latino consent rate*) and 2) the same proportion for non-Latino drivers (*non-Latino consent rate*). These consent search measures (as well as the hit rate measure below) are similar to the measures in Dias et al., 2024, but we calculate them by driver race at the state level rather than for those two counties alone. Unlike probable cause searches, consent searches require no justification beyond asking for permission to search. Consequently, consent searches are less likely to uncover criminal activity (Dias et al., 2024), and drivers rarely refuse them even when officers have limited cause to search (Dias et al., 2024; Sommers and Bohns, 2024).

We also measure daily contraband recovery rates for Latino and non-Latino stop-and-searches (*hit rate*). Given that OSS was meant to identify drug trafficking, human smuggling, and unauthorized immigration, we define contraband as weapons, drugs, (illicit) money, and evidence of human smuggling. Lower post-OSS hit rates for Latino (relative to non-Latino) stop-and-searches would suggest searches became increasingly unwarranted toward Latino drivers.

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

Since the outcomes are measured at the day-level and the unit of analysis is the day ($N = 2556$ days between January 1, 2009-December 31, 2015), our independent variable 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*). For summary statistics on our daily OSS data, see Appendix Table V15.

We use a regression discontinuity-in-time (RDiT) design to assess the discontinuous, immediate effect of *OSS* on our outcomes. The RDiT is advantageous relative to other designs (e.g. difference-in-differences) since coefficient estimates evaluating the immediate effect of *OSS* are less likely to be affected by secular differential time trends independent of *OSS*. The core identification assumption for estimating causal effects using an RDiT design is the *continuity assumption*: factors that could affect the outcomes should be statistically similar immediately before and after *OSS* independent of *OSS* (Lee and Lemieux, 2010; C. Hausman and Rapson, 2018). Two pieces of evidence support this assumption. First, *OSS* was announced only two days before implementation and both media coverage and information-seeking of *OSS* was highly limited around implementation (again, see pp. 50-51, 89-90 in (Dias et al., 2024)). Thus, it is reasonable to assume that driver characteristics or other factors associated with our outcomes across Texas did not change immediately before and after *OSS* in anticipation of the policy (e.g. the propensity to engage in criminal activity or traffic violations by race/ethnicity, weather, the ethno-racial distribution of the driving population). Continuity in driver characteristics by race/ethnicity pre/post-OSS is important for estimating shifts in the disparate policing of Latinos because we are not measuring the overall effect of being Latino on our outcomes of interest (e.g. the citation, consent, and contraband recovery rate), but rather 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 can assume that the effect

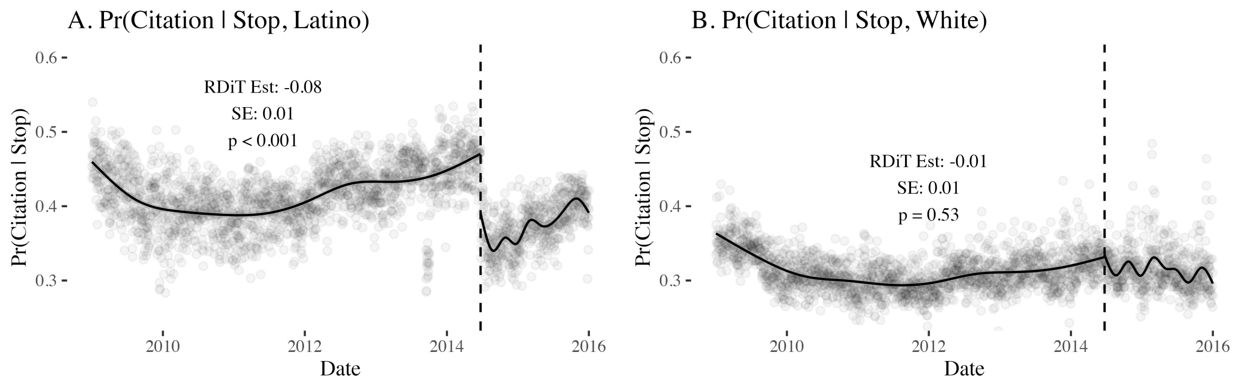

Figure 4: *OSS* discontinuously reduced the statewide Latino citation rate. Panels A-B show the proportion of stops that led to citations (instead of warnings) for Latinos and whites.

of Latino ethnicity would have been the same after June 23rd as before OSS if OSS had not occurred. Second, we provide statistical evidence in support of the *continuity assumption.* Covariates that could theoretically affect our outcomes of interest, including crime, traffic accidents, and weather are statistically balanced immediately before and after *OSS* (see Dias et al. (2024) at 54-55 and Appendix Figures W11, 20, and 21). In sum, our RDiT estimates are unlikely to reflect omitted variable bias independent of OSS.

We present mean-squared optimal bandwidth RDiT estimates (Calonico, Cattaneo, and Titiunik, 2015), with the running variable (days to *OSS*) to the 1st polynomial and a uniform kernel.

Results

Figure 3 displays RDiT effects of *OSS* on *Pr(Latino)* and *Pr(HS)*. Consistent with expectations, given the sudden shift of policing into two predominantly Latino counties, *OSS* discontinuously increased the proportion of DPS traffic stops across Texas involving Latinos by 7 percentage points (pp., $p < 0.001$), 2.2 standard deviations of the pre-*OSS* daily outcome (Figure 3, Panel A). 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 (Figure 3, Panel B). Additional RDiT estimates show that OSS discontinuously increased raw Latino stops, while producing no change in non-Latino stops (see Appendix Figure X12).

The increase in the Latino stop proportion was accompanied by an increase in Latino stops that yielded neither citations nor contraband. Across Texas, *OSS* discontinuously decreased the *Latino citation rate* by 8 pp. ($p < 0.001$), 2 standard deviations of the pre-*OSS* daily outcome (Figure 4, Panel A). Conversely, *OSS* did not change the non-Latino citation rate (Figure 4, Panel B). A formal coefficient difference test shows the RDiT *OSS* effect on the *Latino citation rate* is statistically distinguishable from the RDiT effect on the *non-Latino citation rate* ($p < 0.001$). These results suggest that *OSS* disparately increased the superfluous policing of Latinos, but not non-Latinos.

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

In summary, *OSS*, an explicit mandate to pursue immigration-related policing from the Texas Governor and Texas DPS Chief, resulted in clear, discontinuous shifts in the Latino stop, citation, and hit rate. The evidence appears to suggest the shift in policing priorities due to OSS led to more disparate, superfluous, policing of the Texas Latino population.

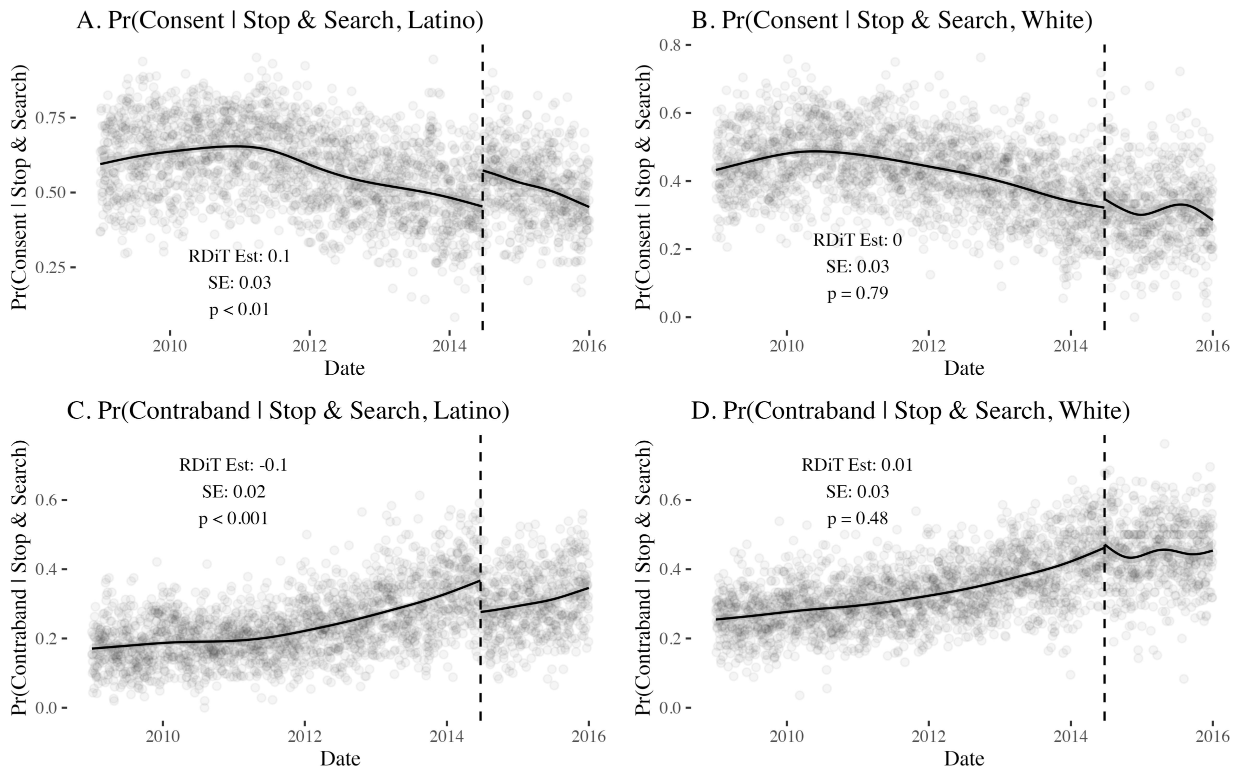

Figure 5: *OSS* discontinuously increased the statewide consent search rate and decreased the statewide hit rate. Panels A-B show the consent search rate for Latinos and whites over time, and Panels C-D show the hit rate for Latinos and whites over time. The dashed vertical line marks *OSS* onset. Loess lines are fit on each side of the *OSS* discontinuity.

Did OSS Mitigate Crime?

Although *OSS* appears to increase inefficient and racially disparate policing, it may have reduced crime by increasing DPS presence. We demonstrate that *OSS* did not reduce crime. First, there is no, or at least no sustained, increase in raw citations or contraband recovered post-*OSS* (Appendix Figure Y13-Z14), implying that *OSS* did not lead to a *raw* increase in the identification of citation-worthy activity or contraband despite a reduction in the citation or hit *rate*. Second, see also Dias et al. (2024) at 54-55 for a comprehensive analysis finding no effect of *OSS* on crime in the *OSS* area of operations.

Was OSS Effective at Identifying Undocumented Immigrants?

One of the implicit and explicit purposes of *OSS* was to prevent human smuggling and undocumented migration during the 2014 child migrant crisis (Dias et al., 2024). In Appendix Section AA, we demonstrate that *OSS* was ineffective at identifying undocumented migration or human smuggling. Thus, *OSS* increased racially disparate, inefficient, policing without meeting policy goals.

Robustness Checks

Our RDiT estimates are similar when we use alternative polynomial (quadratic, cubic), kernel (triangular, Epanechnikov), and bandwidth (dividing optimal bandwidth by 2) specifications (Appendix Figure 16), meaning our results are not driven by model specification choices. Consistent with the notion our RDiT coefficients are causally identified, the *OSS* RDiT effects on our outcomes are statistically and substantively similar adjusting and not for the aforementioned balance covariates (Appendix Figure 17). Our estimates are larger than the vast majority of fake pre-*OSS* temporal placebo discontinuities (Appendix Figure 18), suggesting that our results are not due to 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 driven by anticipatory effects (Appendix Figure 19).

We assess the effect of *OSS* on 8 primary outcomes across Figures 3-5. Thus, we assess whether our main statistically significant effects are robust to a Bonferroni p-value correction that divides 0.05 by 8 ($p < 0.00625$). The p-value for all statistically significant outcome tests is less than 0.001, so our results are robust to false positives driven by random statistical chance.

We also rule out the possibility that our results are driven by Latino drivers hiding contraband in response to *OSS*. We believe this alternative explanation is unlikely since *OSS* was sudden and unanticipated, and our RDiT estimates characterize an immediate effect.

Still, we consider the possbility that Latino drivers responded swiftly and endogenously to *OSS* post-OSS by driving more carefully. If so, we would expect that careful driving to lead to a decrease in traffic accidents. We implement a falsification test and do not find that *OSS* discontinuously reduced the number of Latino or non-Latino crashes throughout Texas or in Hidalgo/Starr counties, suggesting that our results are not explained by changes in driver behavior in response to *OSS* (Appendix Figures 20-21).

Finally, we rule out the possibility that our results are driven by inexperienced officers who typically patrol other counties being reassigned to police Hidalgo and Starr counties post-*OSS*, leading to inefficient, racially disparate policing (see Dias et al. (2024), Figure C4, for the increase in the number of officers patrolling Hidalgo and Starr post-*OSS*). To do this, we subset our stop data to officers who policed Hidalgo and Starr in the 60 days pre-*OSS* and re-estimate the RDiT *OSS* effect on our outcomes. Results do not change (Appendix Figure 22), suggesting that the new overall focus on Hidalgo and Starr counties for enforcement-related goals—not an influx of inexperienced officers in particular—explains our results.

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. The same null result holds across a wide variety of difference-in-differences and event study specifications, in addition to different political and demographic 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 Latino stops increased, citation and hit rates suddenly fell, suggesting that the new stops were less effective than those that came before. In summary, Operation Strong Safety increased disparate and inefficient policing against Latino (relative to non-Latino) drivers.

Why did immigration enforcement drive racially disparate policing in one context but not another? Our results themselves cannot answer this question, but they do generate hypotheses for future work. The results point to differing organizational incentives for police officers. When federal enforcement intensity changes, even if that change reflects a local jail's refusal to honor detainer requests, police officers receive no mandate to change their traffic stop behavior. By contrast, Texas's Operation Strong Safety involved exactly that: a mandate to focus traffic policing on immigration goals by literally moving police into areas where traffic stops would predominantly involve Latino drivers.

Police officers' attention to organizational incentives is consistent with our precise null finding that sanctuary policies, which reduce deportations by a third (D. K. 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 also because noncitizen arrests and local deportations are (unsurprisingly) very highly cross-sectionally correlated. Figure 6, 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. Deportations depend on arrests, not vice versa.

Our two findings have implications for two lines of scholarship. First, our first study sheds light on the mechanism driving the many political, economic and human effects of increased immigration enforcement. Immigration enforcement likely imposes these effects

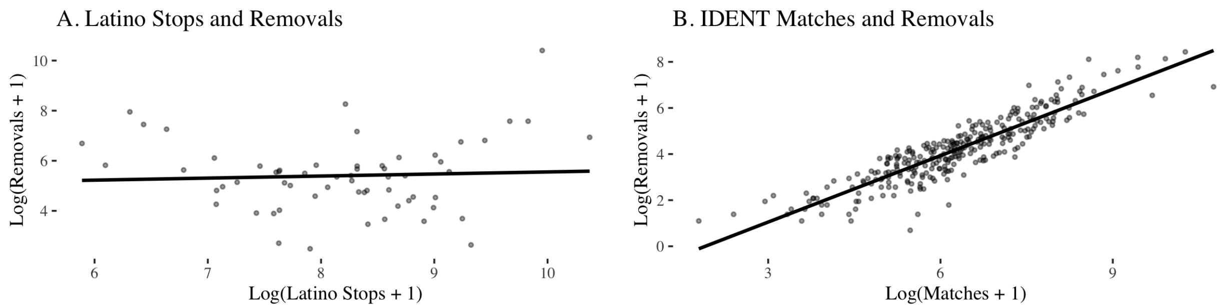

Figure 6: Association between Latino stops, IDENT matches, and removals.

directly, through detention and deportation of noncitizens, rather than indirectly, through increased police profiling in stops or arrests.

Second, both studies contribute to 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.[10]

Our analysis has limitations. Given limited 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), and 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

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

contexts/departments in our sample. Nonetheless, in the Appendix we conduct intra-state replications of our results covering the California and North Carolina highway patrols. These departments have jurisdiction over the first and twelfth largest Latino populations by state. Consistent with our broader analysis, we do not find that Secure Communities increased disparate policing against Latinos in either state (Appendix Figure I6 and J7).

In addition, a key limitation of our first study is that, while our noncitizen arrest results suggest a precise null, our null results for traffic stop outcomes are less precisely estimated. 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. Because the hypothesized effect on traffic stops depends on police officers' interest in arresting noncitizens to transfer them to ICE, it would 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 6, 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, which are far more frequent than arrests, result much 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 what we observe in Operation Strong 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). In sum, the degree to which immigration enforcement shapes police behavior depends on the immigration enforcement policy.

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Contents

A Maps Characterizing Sample and Treatment: S-Comm Analysis

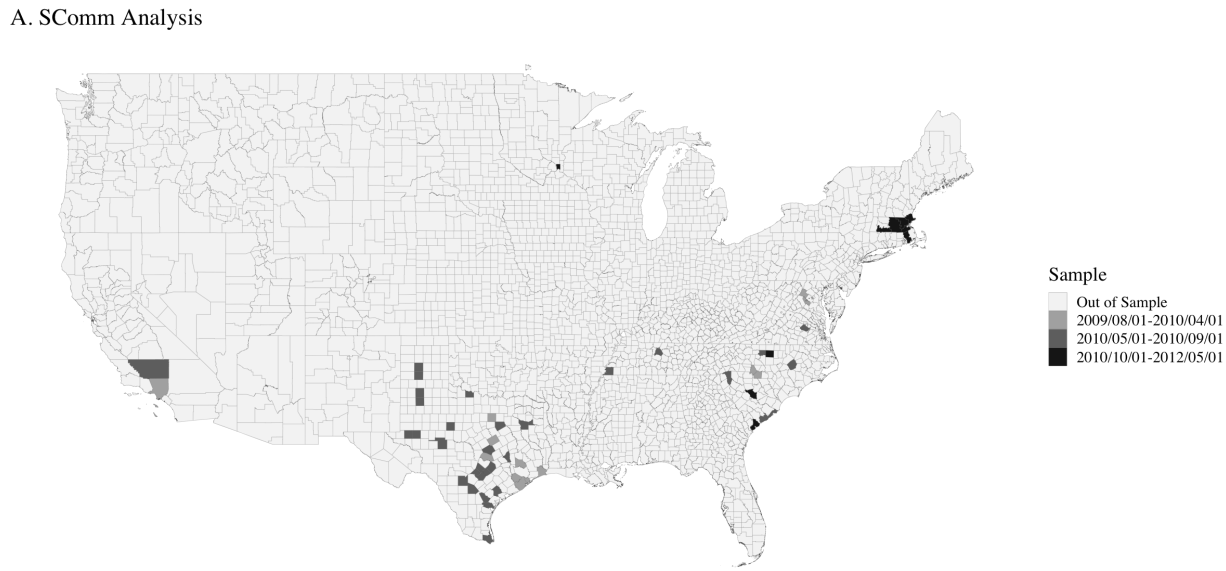

Figure A1: Map Characterizing Sample and Treatment Status Across Counties

B Summary Statistics: S-Comm Analysis

Table B1: Summary Statistics (Secure Communities and Latino Stops Data)

Variable	Minimum	1st Quartile	Median	Mean	3rd Quartile	Maximum
Latino Stops	0.00	165.00	317.00	436.76	549.00	5160.00
Non-Latino Stops	27.00	687.00	1198.00	2138.06	2535.00	15870.00
Log(Latino Stops+1)	0.00	5.11	5.76	5.66	6.31	8.55
Pr(Latino Stop)	0.00	0.09	0.16	0.21	0.29	0.88
S-Comm	0.00	0.00	1.00	0.70	1.00	1.00
% GOP (2008)	0.14	0.40	0.55	0.53	0.66	0.81
% GOP (2016)	0.09	0.38	0.55	0.52	0.66	0.80
% Latino (2010)	3.05	10.92	19.61	23.57	34.15	87.41
% Foreign (2010)	4.16	7.05	9.14	11.32	13.03	35.64
% Non-Citizen (2010)	2.48	4.21	6.29	7.01	8.29	19.67

C Maps Characterizing Sample and Treatment: Sanctuary Analysis

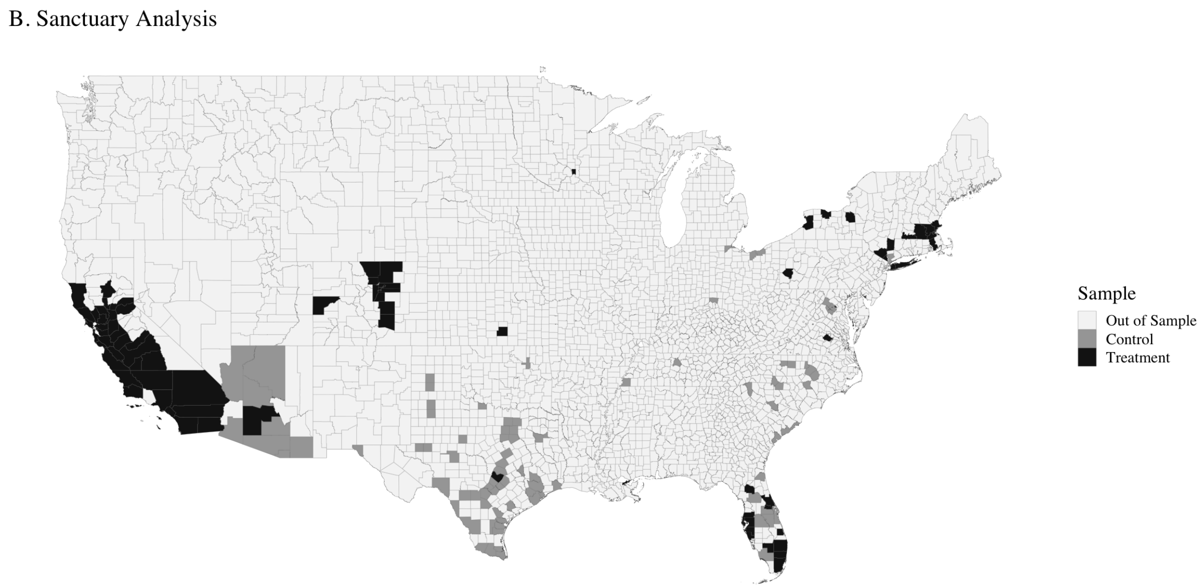

Figure C2: Map Characterizing Sample and Treatment Status Across Counties

D Summary Statistics: Sanctuary and Latino Stops Analysis

Table D2: Summary Statistics (Sanctuary Policy and Latino Stops Data)

Variable	Minimum	1st Quartile	Median	Mean	3rd Quartile	Maximum
Latino Stops	0.00	185.00	406.00	1299.09	910.00	38142.00
Non-Latino Stops	5.00	854.75	1865.00	3497.35	3574.00	51212.00
Log(Latino Stops+1)	0.00	5.23	6.01	6.06	6.81	10.55
Pr(Latino Stop)	0.00	0.11	0.22	0.72	0.52	31.55
Sanctuary	0.00	0.00	0.00	0.14	0.00	1.00
% GOP (2008)	0.14	0.36	0.45	0.46	0.55	0.81
% GOP (2016)	0.09	0.32	0.42	0.43	0.55	0.80
% Latino (2010)	1.44	11.30	20.81	27.86	39.56	98.33
% Foreign (2010)	2.83	7.48	11.75	14.60	21.31	36.99
% Non-Citizen (2010)	1.12	4.42	7.44	8.83	12.68	21.96

E County Distribution of State Patrol Officers

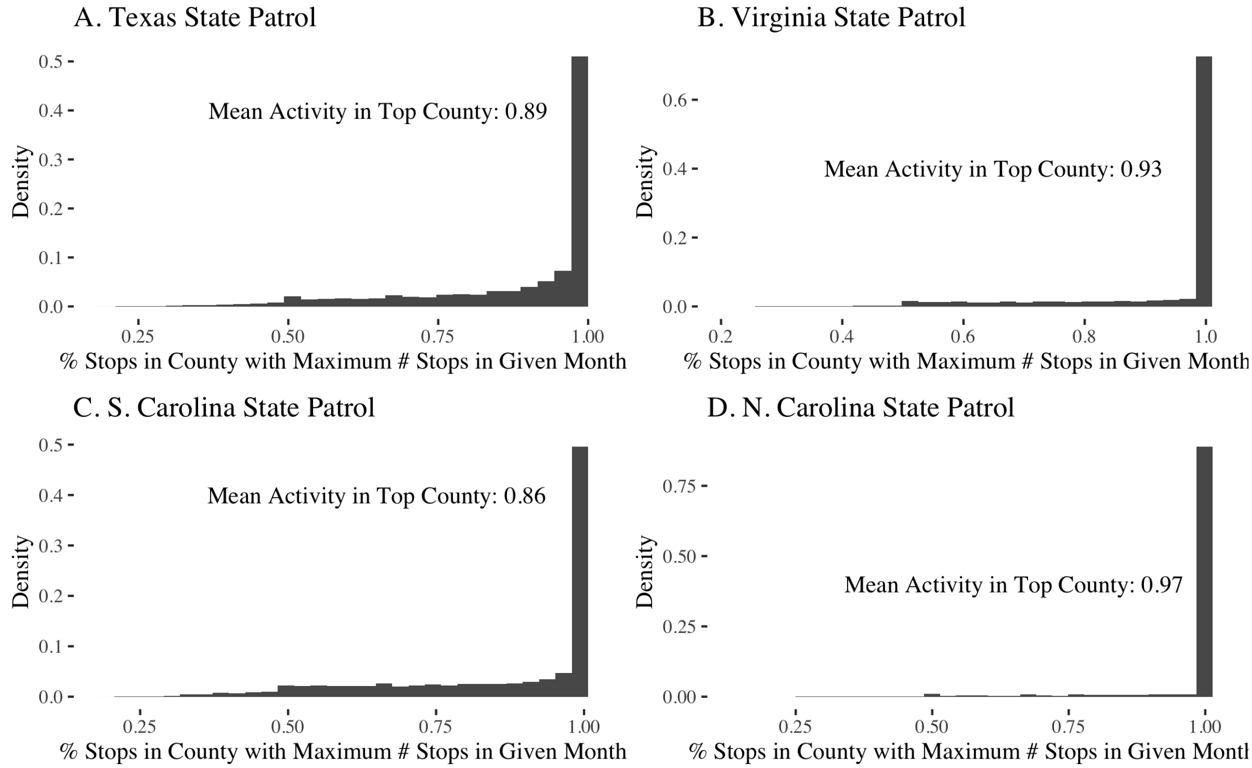

Figure E3: Histogram characterizing proportion of time spent in county with maximum level of stop activity (x-axis) at the officer/month level for State Patrol departments (2008-2015).

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 E3 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 potentially perceive changes in jail enforcement priorities and change their individual policing behavior accordingly.

F Maps Characterizing Sample and Treatment: IDENT Analysis

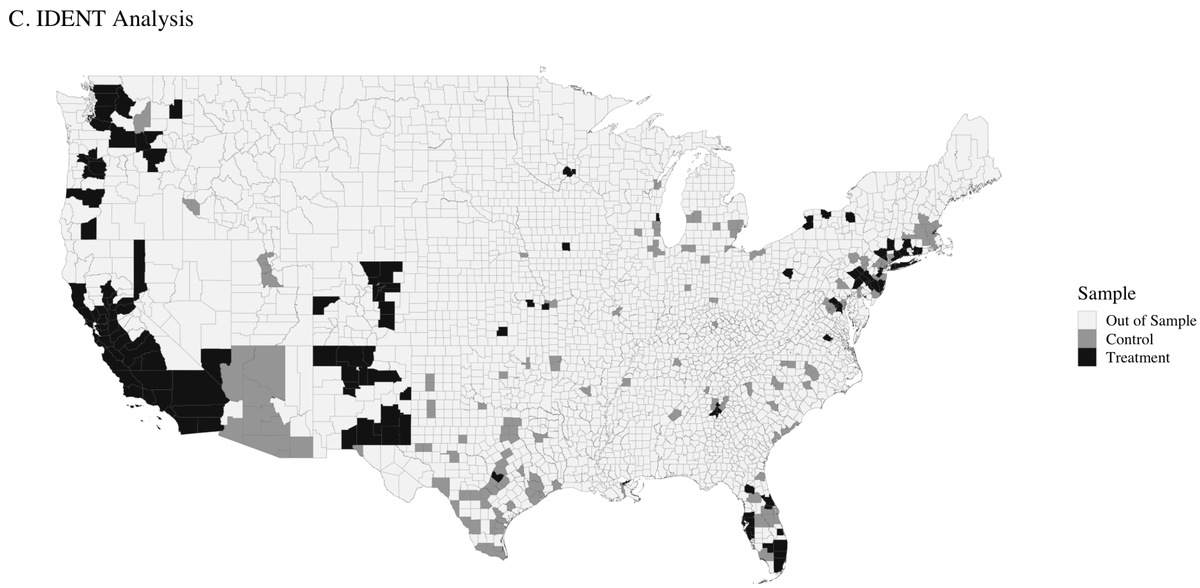

Figure F4: Map Characterizing Sample and Treatment Status Across Counties

G Summary Statistics: Sanctuary and IDENT Analysis

Table G3: Summary Statistics (Sanctuary Policy and IDENT Matches Data)

Variable	Minimum	1st Quartile	Median	Mean	3rd Quartile	Maximum
IDENT Matches	0.00	0.00	24.00	90.83	73.00	4399.00
IDENT Matches (Level 1)	0.00	0.00	5.00	25.91	18.00	1786.00
IDENT Matches (Level 2-3)	0.00	0.00	17.00	64.92	53.00	3377.00
Log(IDENT Matches+1)	0.00	0.00	3.22	2.82	4.30	8.39
Pr(IDENT Match)	0.00	0.03	0.05	0.06	0.07	1.00
Sanctuary	0.00	0.00	0.00	0.13	0.00	1.00
% GOP (2008)	0.07	0.38	0.45	0.46	0.55	0.81
% GOP (2016)	0.04	0.34	0.45	0.44	0.55	0.80
% Latino (2010)	1.44	9.34	15.80	23.73	31.78	98.33
% Foreign (2010)	2.83	7.65	10.96	13.58	18.67	51.07
% Non-Citizen (2010)	1.12	4.22	6.71	8.18	11.13	26.21

H Secure Communities Results: Callaway and Sant'Anna Estimator

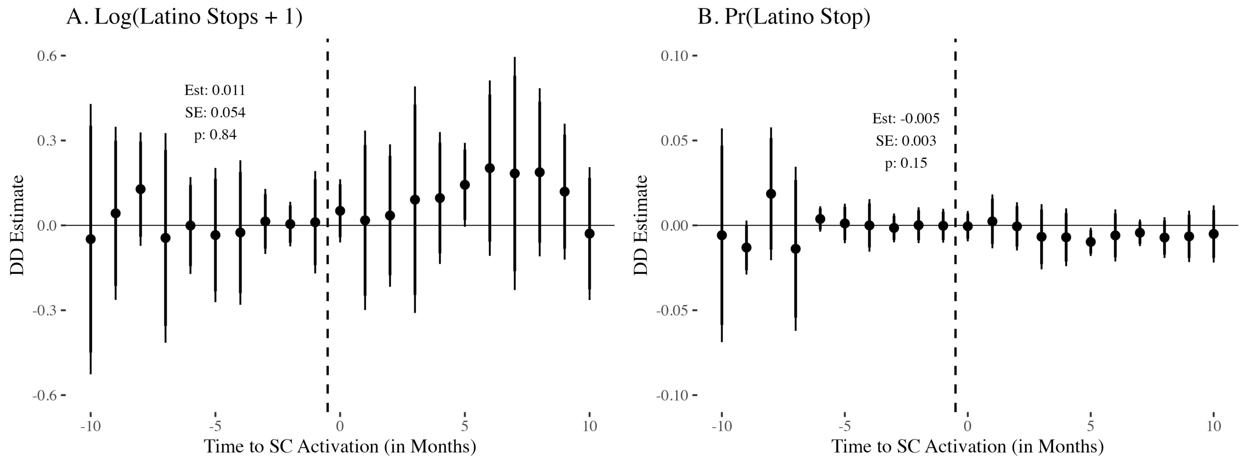

Figure H5: Event study estimates characterizing effect of Secure Communities (S-Comm), Callaway and Sant'Anna (2021) estimator Callaway and Sant'Anna, 2021. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm. Binary indicators 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.

I Secure Communities Results: California Replication

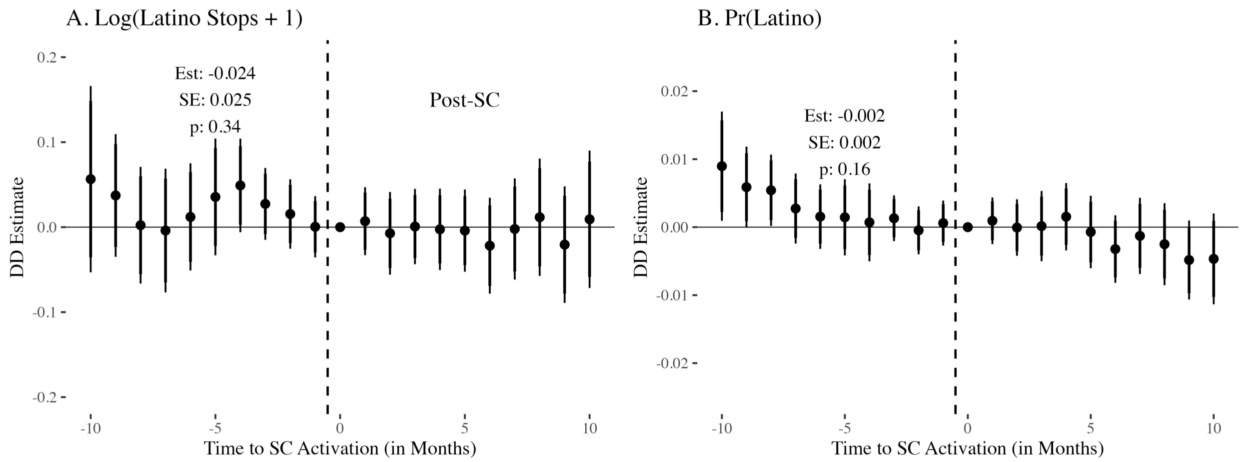

Figure I6: Event study estimates characterizing effect of Secure Communities (S-Comm) on California Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm on Latino stops. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Each panel uses a different outcome (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 county level.

J Secure Communities Results: North Carolina Replication

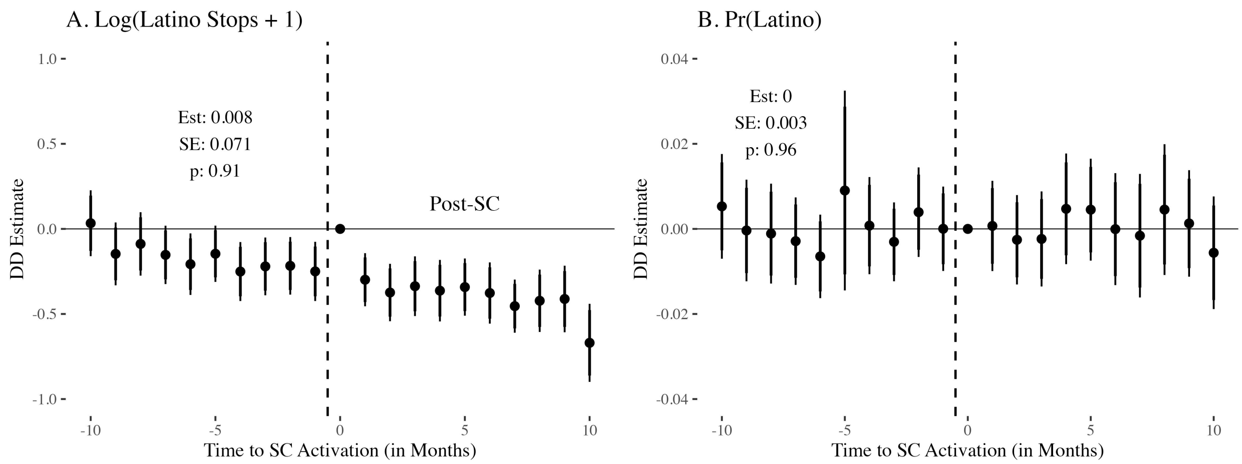

Figure J7: Event study estimates characterizing effect of Secure Communities (S-Comm) on North Carolina Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm on Latino stops. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Each panel uses a different outcome (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 county level.

K Secure Communities Results: Texas Replication

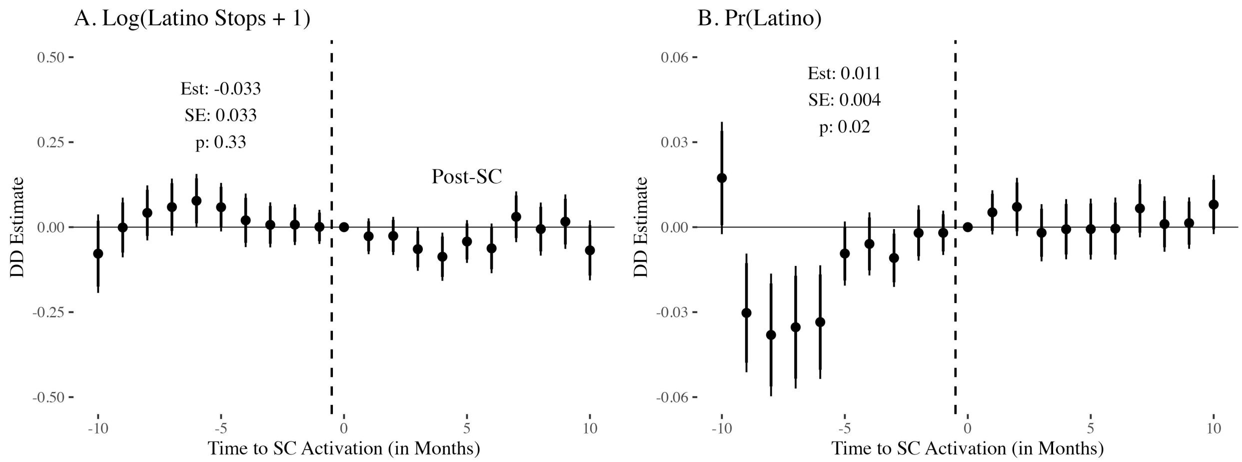

Figure K8: Event study estimates characterizing effect of Secure Communities (S-Comm) on Texas Highway Patrol behavior. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm on Latino stops. Binary indicators characterizing time to policy are equal to 1 on any month before/after 10 months before/after the policy. Each panel uses a different outcome (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 county level.

L Secure Communities Results: Alternative Outcomes

Table L4: Estimating the Effect of Secure Communities on Alternative Outcomes

	IHS(Latino Stops) (1)	**Latino Stops** (2)	**Latino Stop Rate** (3)
SComm	-0.04	9.05	-0.13
	(0.06)	(50.65)	(0.23)
Outcome SD	1.00	456.00	6.62
County/Department FE	Y	Y	Y
Month FE	Y	Y	Y
State x Month FE	Y	Y	Y
State CSE	Y	Y	Y
N	4453	4453	4453
R^2	0.90	0.88	0.84

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Model 1 characterizes the staggered difference-in-differences effect of Secure Communities on the inverse hyperbolic sine transformation for the number of Latino stops at the county/department/month-level. Model 2 characterizes the staggered difference-in-differences effect of Secure Communities on the raw number of Latino stops at the county/department/month-level. Model 3 characterizes the staggered difference-in-differences effect of Secure Communities on the Latino stop rate at the county/department/month-level, that is, the number of Latino stops divided by the 2010 Census county Latino population.

M Secure Communities Results: Alternative SE Clustering

Table M5: Alternative Clustering (County)

	Log(Latino Stops +1) (1)	**Pr(Latino)** (2)
SComm	-0.04	-0.00
	(0.05)	(0.00)
County/Department FE	Y	Y
Month FE	Y	Y
State x Month FE	Y	Y
State CSE	N	N
County CSE	Y	Y
Num. obs.	4453	4453
R^2 (full model)	0.90	0.97

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

N Sanctuary Policy Results: Sanctuary Policies and Traffic Stops

Table N6: 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 N9 are from column 3.

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 N6, 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 N6, 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 N9). In the pre-sanctuary period, 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.

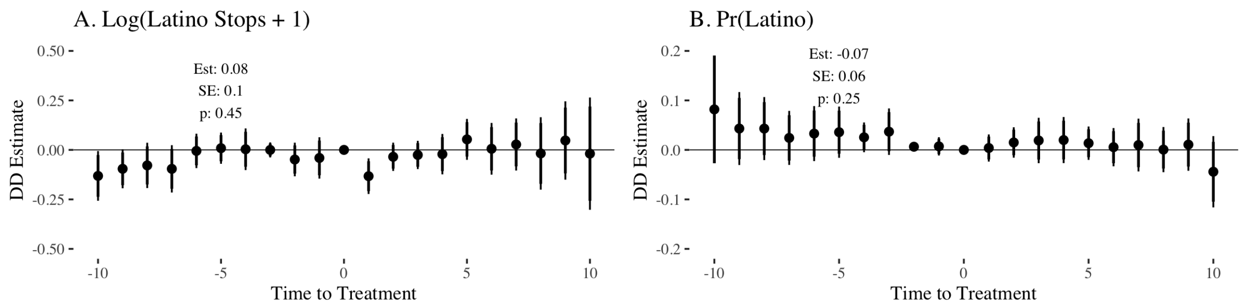

Figure N9: Event study estimates characterizing effect of sanctuary policy. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of sanctuary policies. Binary indicators characterizing time to policy are equal to 1 on any month before/after 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.

O IDENT Results: Callaway and Sant'Anna Estimator

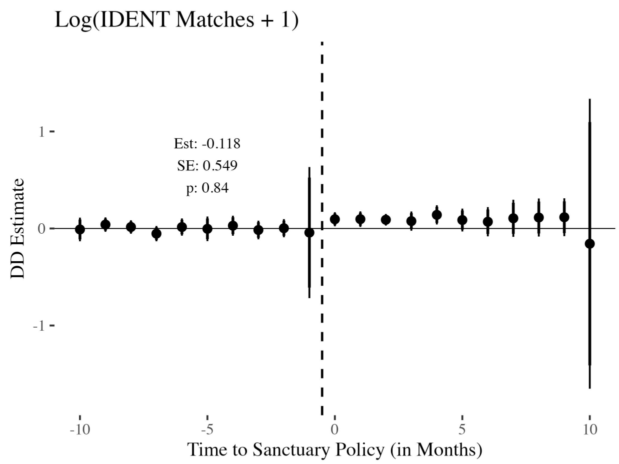

Figure O10: Event study estimates characterizing effect of sanctuary policy on logged IDENT matches outcome (Callaway and Sant'Anna (2021) estimator (Callaway and Sant'Anna, 2021))

P IDENT Results: Regression Tables

Table P7: 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). Note: crime severity is denoted by levels in Secure Communities data, Levels 1-3 (L1-3), where 3 = most severe and 1 = least severe.

Q IDENT Results: Alternative SE Clustering

Table Q8: Alternative Clustering (County)

	Log(Matches + 1) (1)	**Pr(Match)** (2)
Sanctuary	0.00	-0.00
	(0.14)	(0.00)
County FE	Y	Y
Month FE	Y	Y
State x Month FE	Y	Y
State CSE	N	N
County CSE	Y	Y
Num. obs.	26663	19932
R^2 (full model)	0.87	0.72

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

R 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 and Trump's vote share (at the county-level) in the 2016 presidential election. Given our datasets have a relatively small number of counties (58-293) due to our emphasis on the most Latino counties, we bin Republican vote share at the county-level as above or below the median. It is important to note that Trump's election is primarily post-treatment to the activation of Secure Communities and the Sanctuary policies in our datasets, so heterogeneity by Trump vote share may be subject to post-treatment bias in a way that our estimates evaluating heterogeneity by McCain vote share are not.

Generally, we find little evidence that the null effects of S-Comm and sanctuary policies on Latino stops or IDENT matches vary with the political environment. We include full results below, in Tables S9-S14. 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 (IDENT matches).

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

Additionally, we assess if Sanctuary policies or S-Comm has heterogeneous effects on Latino stops or non-citizen arrests (IDENT matches) by demography. It is possible that the effect of S-Comm or sanctuary policies may be stronger (or weaker) in counties where there are a higher proportion of Latinos, immigrants, and non-citizens. Again, given our datasets have a relatively small number of counties (58-293) due to our emphasis on the most Latino counties, we bin Latino, immigrant, and non-citizen population proportions as above or below the median. Tables S9-S14 demonstrate that there is no consistent heterogeneous effects of S-Comm or sanctuary policies on Latino stops or non-citizen arrests by the proportion of the county population that is Latino, immigrant, or non-citizen (measured using 2010 Census American Community Survey data at the county-level). These results demonstrate that our null results are not masking heterogeneous effects by ethno-racial or immigrant demography.

S Heterogeneity: Secure Communities Results

Table S9: Heterogeneity (Logged Latino Stops Outcome)

	Log(Latino Stops + 1)				
	(1)	(2)	(3)	(4)	(5)
SComm	-0.03	-0.00	0.02	-0.04	-0.07
	(0.06)	(0.07)	(0.03)	(0.08)	(0.07)
SC x > Median (% GOP '08)	-0.02				
	(0.06)				
SC x > Median (% GOP '16)		-0.07			
		(0.03)			
SC x > Median (% Latino)			-0.11		
			(0.07)		
SC x > Median (% Foreign)				-0.01	
				(0.06)	
SC x > Median (% Non-Citizen)					0.05
					(0.04)
County/Department FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	Y	Y	Y	Y	Y
State CSE	Y	Y	Y	Y	Y
Num. obs.	4453	4453	4453	4453	4453
R^2 (full model)	0.90	0.90	0.90	0.90	0.90

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table S10: Heterogeneity (Pr(Latino) Outcome)

	Pr(Latino)				
	(1)	(2)	(3)	(4)	(5)
SComm	−0.00	−0.00	−0.01	−0.00	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
SC x > Median (% GOP '08)	−0.00				
	(0.01)				
SC x > Median (% GOP '16)		−0.01			
		(0.00)			
SC x > Median (% Latino)			0.01		
			(0.01)		
SC x > Median (% Foreign)				−0.00	
				(0.01)	
SC x > Median (% Non-Citizen)					−0.00
					(0.00)
County/Department FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	Y	Y	Y	Y	Y
State CSE	Y	Y	Y	Y	Y
Num. obs.	4453	4453	4453	4453	4453
R^2 (full model)	0.97	0.97	0.97	0.97	0.97

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

T Heterogeneity: Sanctuary Results

Table T11: Heterogeneity (Logged Latino Stops Outcome)

	Log(Latino Stops + 1)				
	(1)	(2)	(3)	(4)	(5)
SComm	0.07	0.06	0.06	0.07	0.06
	(0.09)	(0.08)	(0.08)	(0.09)	(0.09)
Sanctuary x > Median (% GOP '08)	0.01				
	(0.05)				
Sanctuary x > Median (% GOP '16)		0.05			
		(0.06)			
Sanctuary x > Median (% Latino)			0.04		
			(0.06)		
Sanctuary x > Median (% Foreign)				0.01	
				(0.06)	
Sanctuary x > Median (% Non-Citizen)					0.01
					(0.06)
County/Department FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	Y	Y	Y	Y	Y
State CSE	Y	Y	Y	Y	Y
Num. obs.	11304	11304	11304	11304	11304
R^2 (full model)	0.95	0.95	0.95	0.95	0.95

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table T12: Heterogeneity (Pr(Latino) Outcome)

	Pr(Latino)				
	(1)	(2)	(3)	(4)	(5)
SComm	-0.07	-0.07	-0.07	-0.07	-0.06
	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)
Sanctuary x > Median (% GOP '08)	0.02				
	(0.01)				
Sanctuary x > Median (% GOP '16)		0.02			
		(0.01)			
Sanctuary x > Median (% Latino)			-0.00		
			(0.03)		
Sanctuary x > Median (% Foreign)				-0.00	
				(0.02)	
Sanctuary x > Median (% Non-Citizen)					-0.00
					(0.02)
County/Department FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	Y	Y	Y	Y	Y
State CSE	Y	Y	Y	Y	Y
Num. obs.	11304	11304	11304	11304	11304
R^2 (full model)	0.96	0.96	0.96	0.96	0.96

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

U Heterogeneity: IDENT Results

Table U13: Heterogeneity (Logged IDENT Matches Outcome)

	Log(IDENT Matches + 1)				
	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.02	0.05	−0.09	−0.19	−0.20
	(0.17)	(0.19)	(0.14)	(0.14)	(0.14)
Sanctuary x > Median (% GOP '08)	−0.06				
	(0.13)				
Sanctuary x > Median (% GOP '16)		−0.13			
		(0.13)			
Sanctuary x > Median (% Latino)			0.21		
			(0.20)		
Sanctuary x > Median (% Foreign)				0.35^{**}	
				(0.11)	
Sanctuary x > Median (% Non-Citizen)					0.40^{**}
					(0.15)
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	Y	Y	Y	Y	Y
State CSE	Y	Y	Y	Y	Y
Num. obs.	26663	26663	26663	26663	26663
R^2 (full model)	0.87	0.87	0.87	0.87	0.87

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table U14: Heterogeneity (IDENT Match Proportion Outcome)

	Pr(IDENT Match | Submission)				
	(1)	(2)	(3)	(4)	(5)
Sanctuary	0.00	0.00	−0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Sanctuary x > Median (% GOP '08)	−0.00				
	(0.00)				
Sanctuary x > Median (% GOP '16)		−0.00			
		(0.00)			
Sanctuary x > Median (% Latino)			0.00		
			(0.00)		
Sanctuary x > Median (% Foreign)				0.01^{**}	
				(0.00)	
Sanctuary x > Median (% Non-Citizen)					0.01^{**}
					(0.00)
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
State x Month FE	Y	Y	Y	Y	Y
State CSE	Y	Y	Y	Y	Y
Num. obs.	19932	19932	19932	19932	19932
R^2 (full model)	0.72	0.72	0.72	0.72	0.72

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

V OSS Summary Statistics

Table V15: Statewide Summary Statistics (OSS Data)

Variable	Minimum	1st Quartile	Median	Mean	3rd Quartile	Maximum
OSS	0.00	0.00	0.00	0.22	0.00	1.00
Stops	1577.00	4784.00	5861.00	6161.08	7078.00	22019.00
Latino Stops	540.00	1582.00	1858.00	1971.38	2188.00	7173.00
Non-Latino Stops	759.00	3133.00	4031.00	4189.70	4966.00	14846.00
Hidalgo/Starr Stops	33.00	194.00	258.00	322.16	370.00	1215.00
Pr(Latino)	0.24	0.29	0.31	0.33	0.35	0.61
Pr(Hidalgo/Starr)	0.01	0.03	0.04	0.06	0.06	0.41
Latino Citations	223.00	640.00	742.00	801.65	894.00	3026.00
Non-Latino Citations	304.00	1029.00	1299.00	1353.13	1562.00	4525.00
Latino Citation Rate	0.28	0.38	0.41	0.41	0.44	0.54
Non-Latino Citation Rate	0.22	0.31	0.32	0.33	0.34	0.51
Latino Contraband	0.00	7.00	10.00	11.42	14.00	48.00
Non-Latino Contraband	4.00	19.00	26.00	27.65	34.00	104.00
Latino Hit Rate	0.00	0.18	0.24	0.25	0.31	0.61
Non-Latino Hit Rate	0.12	0.29	0.35	0.36	0.42	0.71
Latino Consent Rate	0.08	0.47	0.57	0.57	0.67	0.95
Non-Latino Consent Rate	0.00	0.34	0.42	0.42	0.50	0.74
Anti-Latino Bias	-4.60	0.11	0.30	0.26	0.48	1.00

W OSS RDiT Balance Test (Continuity Assumption)

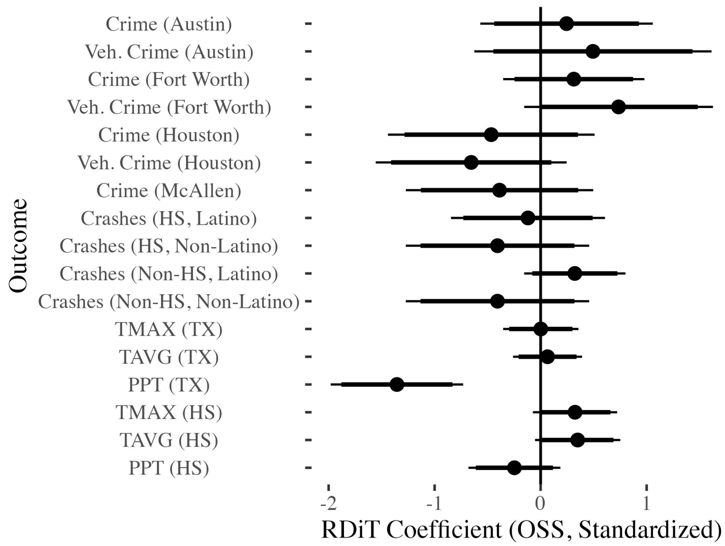

Figure W11: Outcomes that could affect our outcomes are statistically balanced before and after *OSS*. The x-axis is the regression discontinuity-in-time effect of OSS on the standardized balance covariate of interest (specified on the y-axis). RDiT estimates use mean-squared optimal bandwidths with a uniform kernel and polynomial equal to 1. 95% CIs displayed derived from robust standard errors.

Figure W11 characterizes regression discontinuity-in-time effects of Operation Strong Safety on various daily-level balance covariates that could affect our outcomes of interest, such as crime, vehicular crime ("veh. crime"), traffic crashes, and weather. (Note that this analysis overlaps and is consistent with the analysis of crime in (Dias et al., 2024) at pp. 82 & 91; we draw on several of the same data sources for measuring crime and traffic accidents.) All balance covariates are measured for the year 2014 (01/01/2014 to 12/31/2014).

Austin crime data are from the Austin Open Data website: `https://data.austintexas.gov/Public-Safety/Crime-Reports/fdj4-gpfu/about_data`. Crimes in Austin include all reported daily-level crimes from the Austin Police Department (APD). Vehicular crimes using APD data are those that involve vehicles in the APD data (DWI, AUTO THEFT, VEHICLE BURGLARY, THEFT FROM AUTO, AGGRAVATED ASSAULT WITH MOTOR VEHICLE, EVADING VEHICLE PURSUIT, CRASH/MANSLAUGHTER). Fort Worth crime data are from the Fort Worth Open Data website: `https://data.fortworthtexas.gov/datasets/5acdb612b4654c3abb30e6987db2408b_0/explore`. Crimes in Fort Worth include all reported daily-level crimes from the Fort Worth Police Department (FWPD). Vehicular crimes using FWPD data are those that involve vehicles in the FWPD data (AUTO PART THEFT, TRAFFIC VIOLATION, UNAUTHORIZED USE OF MOVING VEHICLE, OBSTRUCTING HIGHWAY, VEHICLE VIOLATION, MOTOR VEHICLE VIOLATION, BURGLARY OF VEHICLE, AUTO THEFT). Houston crime data are from the Houston Open Data website: `https://data.houstontx.gov/`. Crimes in Houston include all reported daily-level crimes from the Houston Police Department (HPD). Vehicular crimes using HPD data are those that involve vehicles in the HPD data (AUTO THEFT). McAllen crime data are from the McAllen Police Department website: https://www.mcallen.net/departments/pd/records-bureau/crime-reports. The McAllen crime data are in pdf format, which we convert to tabular data. Unlike crime data from other cities, the data are at the monthly level. We examine all crimes, but cannot disaggregate by vehicular crimes since McAllen only releases aggregate crime statistics and does not release incident-level data like the other cities.

Crash data are from the Texas Department of Transportation: https://www.txdot.gov/data-maps/crash-reports-records.html. The different crash outcomes we examine are 1) the daily number of car crashes in Hidalgo and Starr counties that involve a Latino driver (Crashes, HS, Latino); 2) the daily number of car crashes in Hidalgo and Starr counties that involve a non-Latino driver (Crashes, HS, Non-Latino); 3) the daily number of car crashes throughout Texas NOT in Hidalgo and Starr counties that involve Latino drivers (Crashes, HS, Latino); and 4) the daily number of car crashes throughout Texas NOT in Hidalgo and Starr counties that involve non-Latino drivers (Crashes, HS, non-Latino).

Weather data are from the PRISM Group, which leverages data from over 25,000 precipitation and temperature stations: https://www.aaronsmithagecon.com/download-us-weather-data. The different weather outcomes are: 1) maximum temperature across Texas (TMAX, TX); 2) average temperature across Texas (TAVG, TX); 3) precipitation across Texas (PPT, TX); 4) maximum temperature in Hidalgo and Starr (TMAX, HS); 5) average temperature in Hidalgo and Starr (TAVG, HS); 6) precipitation in Hidalgo and Starr (PPT, HS).

X Effect of OSS on # Stops By Race

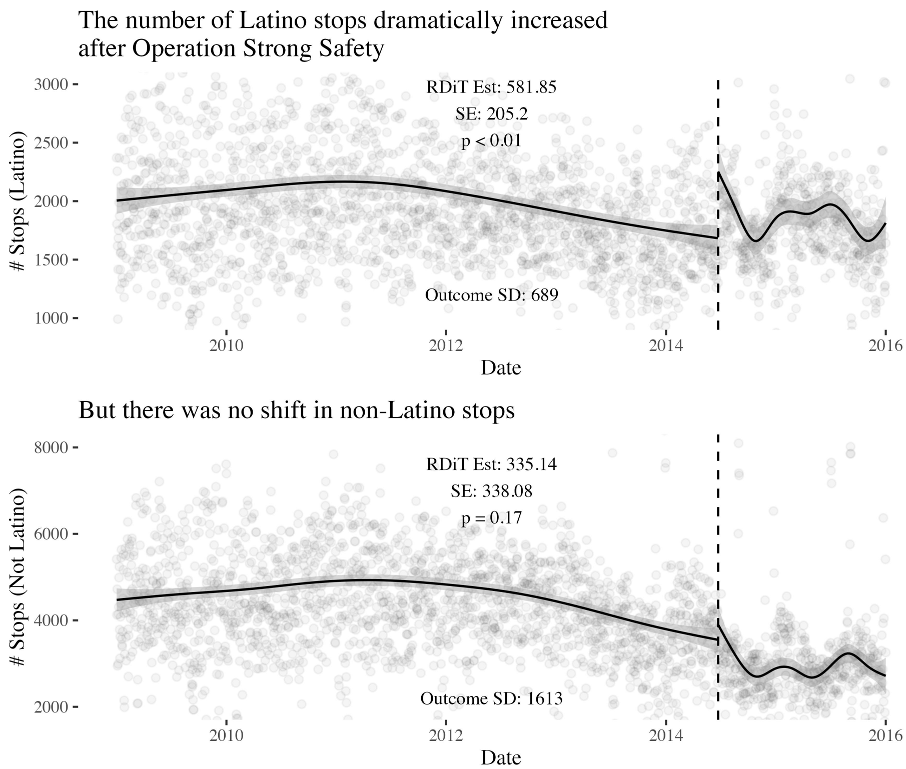

Figure X12: Statewide Latino stops discontinuously increase after OSS, but non-Latino stops do not change after OSS. The top panel characterizes the number of Latino stops (y-axis) throughout the state of Texas initiated by the Texas Department of Public Safety traffic patrol over time at the daily-level between 2009-2016 (x-axis). The bottom panel characterizes the number of non-Latino stops (y-axis) throughout the state of Texas initiated by the Texas Department of Public Safety traffic patrol over time at the daily-level between 2009-2016 (x-axis). Annotations denote mean-squared optimal bandwidth regression discontinuity-in-time effects of OSS on stops (polynomial = 1). Dashed vertical line denotes the onset of Operation Strong Safety.

Y OSS: Effect on Raw Citation Count

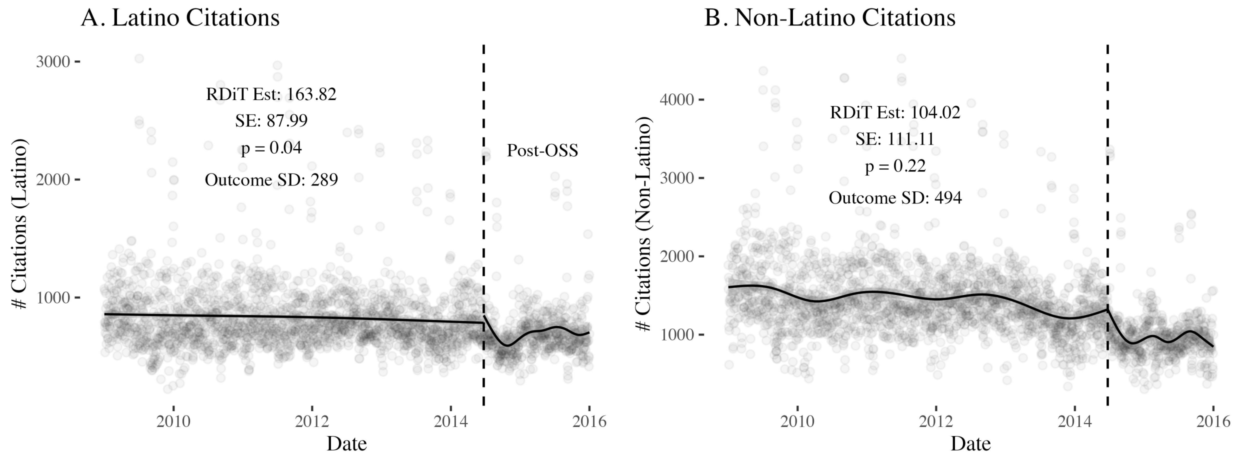

Figure Y13: Regression discontinuity-in-time evidence shows that Operation Strong Safety did not durably increase or decrease the statewide number of citations for either Latinos (Panel A) or non-Latinos (Panel B). Panel A shows the number of citations given to Latinos over time at the daily-level between 2009-2016. Panel B shows the number of citations issued to non-Latinos over time at the daily-level between 2009-2016. Annotations denote mean-squared optimal bandwidth regression-discontinuity-in-time effects (polynomial = 1) of Operation Strong Safety on the citation count. There appears to be a very short-term increase in the number of citations issued to Latinos, but the loess fit on each side of the OSS discontinuity in Panel A suggests that the number of citations given to Latinos *precipitously declined* after the implementation of OSS.

Z OSS: Effect on Raw Contraband Recovery Count

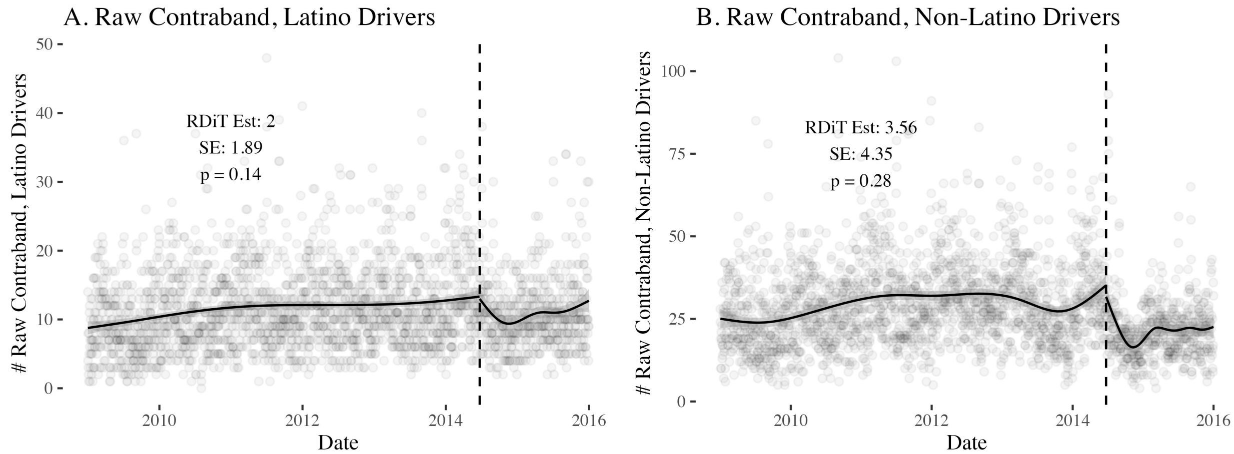

Figure Z14: Regression discontinuity-in-time evidence shows that Operation Strong Safety did not durably increase the number of times that raw contraband was recovered from Latino (Panel A) or non-Latino (Panel B) drivers statewide. Panel A characterizes the amount of raw contraband recovered from Latino drivers that were stopped over time at the daily-level between 2009-2016. Panel B characterizes the amount of raw contraband recovered from non-Latino drivers that were stopped over time at the daily-level between 2008-2016. Annotations denote mean-squared optimal bandwidth regression-discontinuity-in-time effects (polynomial = 1) of Operation Strong Safety on the contraband recovery count.

AA OSS: Effect on Identifying Undocumented Immigrants

Given the purpose of OSS was meant to prevent human smuggling and undocumented migration (Schladen, 2015; Schladen, 2016; Aguilar, 2014; Bosque, 2018), we evaluate if OSS was effective in mitigating undocumented migration or human smuggling. We do this in two ways. First, we examine the "violation" that Texas Department of Public Safety officers report in the aftermath of individual traffic stops. Two violations are relevant here, whether or not the traffic stop led to the identification of someone who had "federal immigration warrants," and whether or not the traffic stop led to the identification of someone engaged in the "smuggling of persons." We deem these two violations "immigration violations." In the six months prior to OSS, 26 immigration violations were identified. In the 6 months after OSS, 32 immigration violations were identified. The daily-level difference-in-prevalence t.test for the number of immigration violations before and after OSS is statistically insignificant at p ¡ 0.19 (see Table . These statistics led us to two conclusions: a) the Texas DPS, despite engaging in hundreds of thousands of traffic stops, identifies incredibly few immigration violations at baseline and b) OSS does not statistically or substantially increase the identification of immigration violations by Texas DPS officers. We further explore the totality of our 15.7 million traffic stop dataset and evaluate how many of these stops are immigration violations (see the note on Table S9). Of the 15.7 million stops in our dataset, only 413 between 2009-2016 are instances where immigration violations are identified, equivalent to an *incredibly small* 0.0003% of the overall traffic stop dataset.

Second, we measure a proxy for the identification of undocumented immigrants in the traffic stop data: the proportion of Latino stops that led to "no driver's license" violations (No ID). This is because undocumented immigrants do not qualify for driver's licenses in Texas. If OSS is effective in identifying undocumented immigrants, we should expect the proportion of Latino stops that lead to the identification of "no driver's license" to increase post-OSS. We do not find this to be the case. In fact, we identify a discontinuous 2 percentage point *decrease* in the proportion of stops leading to the identification of "no driver's license," consistent with our other results that OSS resulted in inefficient policing behavior (Fig. S15, Panel A). However, there is an increase in the raw number of "no driver's license" violations for Latino drivers immediately after OSS, but it is clear from the local linear regression models fit on each side of the OSS discontinuity that this increase is short-term, further suggesting OSS did not effectively increase the potential identification of undocumented immigrants (Fig. S15, Panel B). Taken together, we believe our analysis of Texas DPS traffic stop data suggests OSS was ineffective at significantly increasing the identification of undocumented migration or human smuggling.

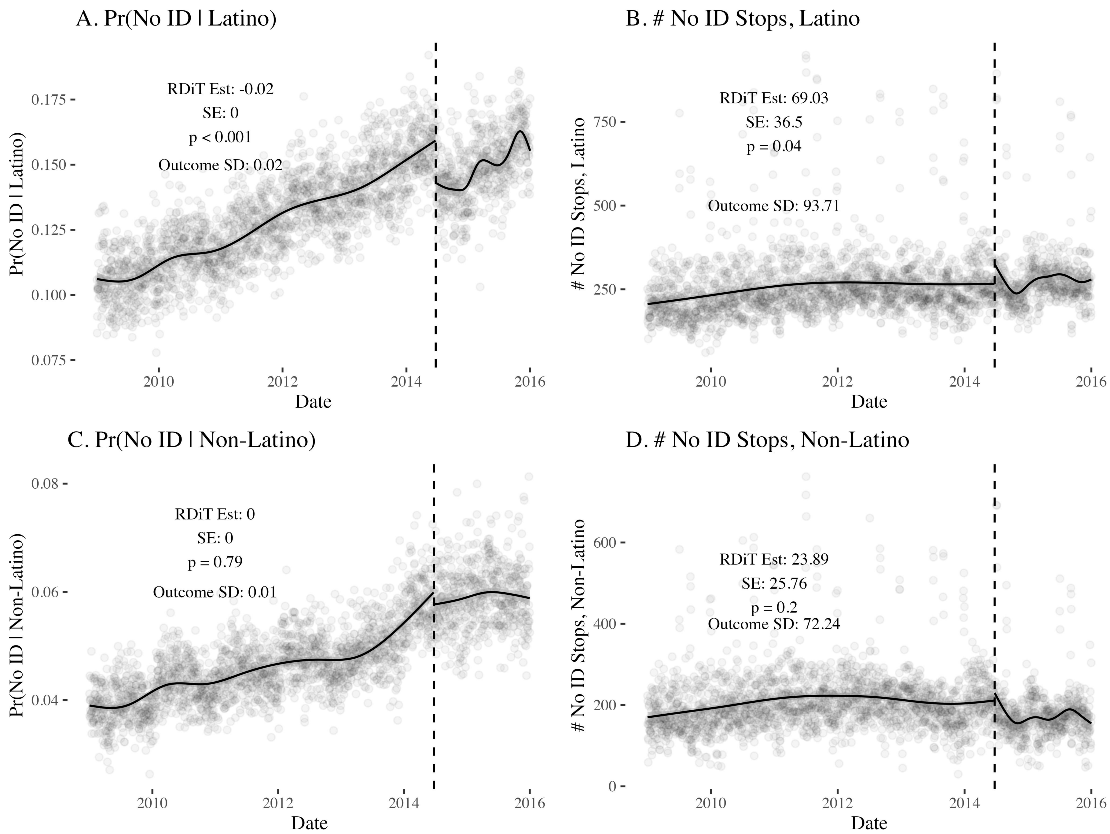

Figure 15: Identification of "no driver's license" among drivers in traffic stops over time in Texas. Panel A characterizes the proportion of stops of Latinos that led to the identification of a "no driver's license" violation (y-axis) over time at the daily-level between 2009-2016 (x-axis). Panel B characterizes the raw number of Latino "no driver's license" violations (y-axis) over time at the daily-level between 2009-2016 (x-axis). Panel C characterizes the proportion of stops of non-Latinos that led to the identification of a "no driver's license" violation (y-axis) over time at the daily-level between 2009-2016 (x-axis). Panel D characterizes the raw number of non-Latino "no driver's license" violations (y-axis) over time at the daily-level between 2009-2016 (x-axis).

Table AA16: Number of immigration violations identified by Texas Department of Public Safety officers through traffic stops before and after Operation Strong Safety

Period	**# of Immigration Violations**
Six Months Pre-OSS	26.00
Six Months Post-OSS	32.00
Difference	6.00
t-test p-val	0.19

Note: State patrol is inefficient at immigration enforcement. Of 15.7 million stops, **only 413** between 2009-2016 are identification of immigration violations (i.e. “federal immigration warrants” or “smuggling of persons”).

AB Alternative RDiT Specifications

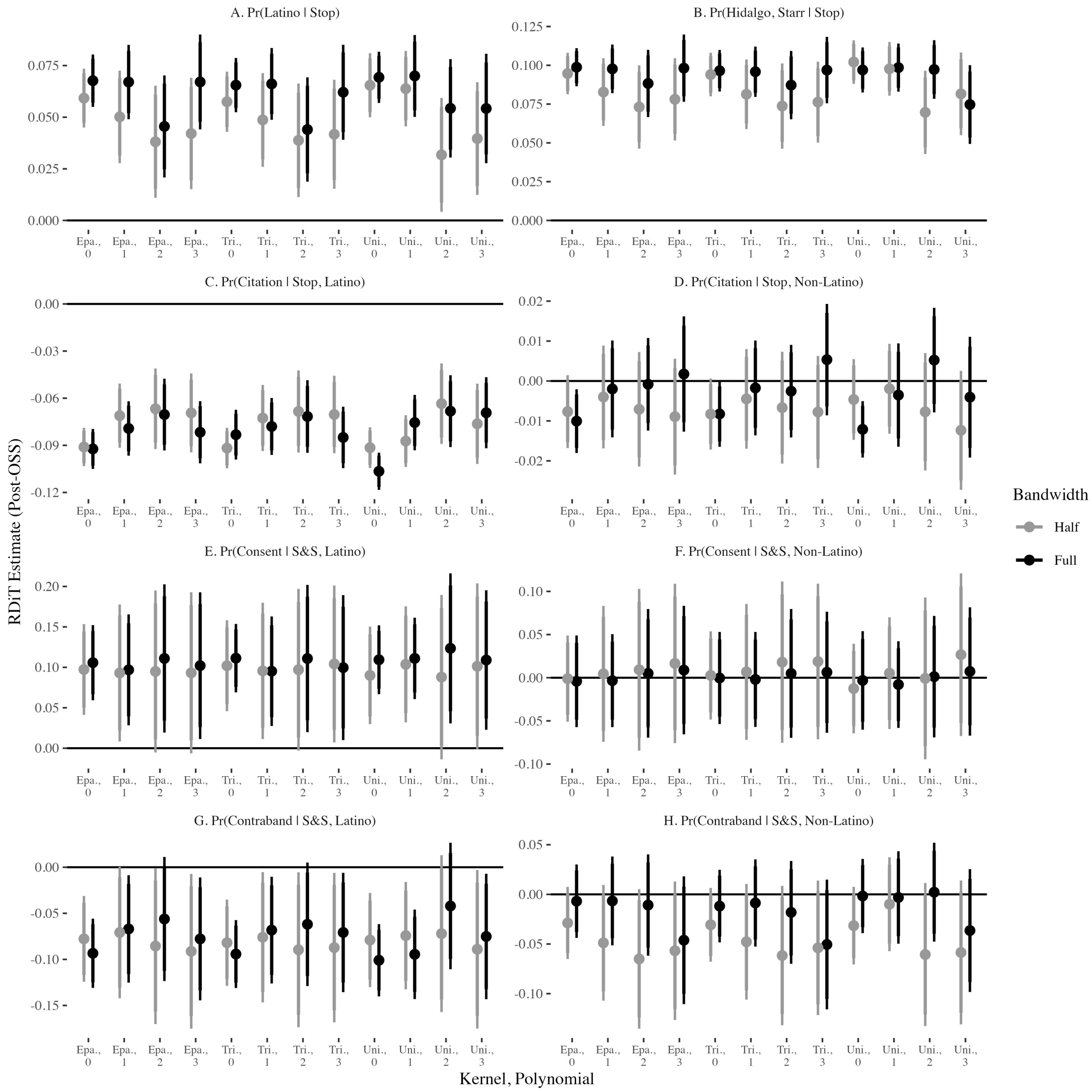

Figure 16: Alternative RDiT specifications by kernel, polynomial (x-axis) and bandwidth (denoted by color). Each panel denotes a different outcome analyzed. 95% CIs displayed from robust SEs

AC Re-Estimation Adjusting For Balance Covariates

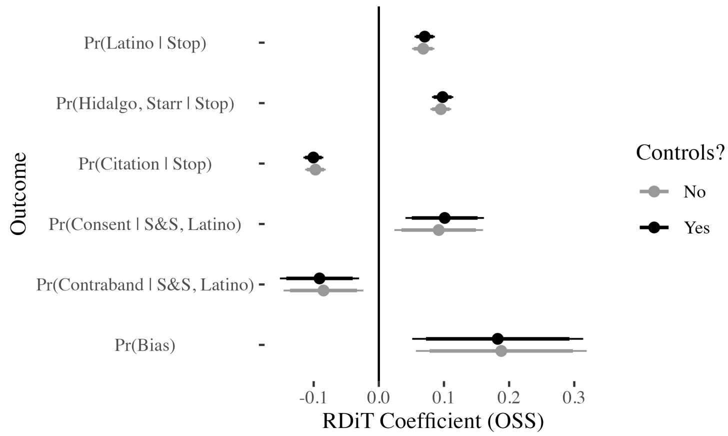

Figure 17: RDiT Effects of *OSS* (y-axis) on outcomes (x-axis) adjusting for balance covariates (denoted by color). Mean-squared optimal bandwidth used with uniform kernel and polynomial equal to 1. Consistent with the temporal domain of the balance tests, we use data from 01/01/2014 to 12/31/2014 here. 95% CIs displayed from robust SEs

AD Temporal Placebos

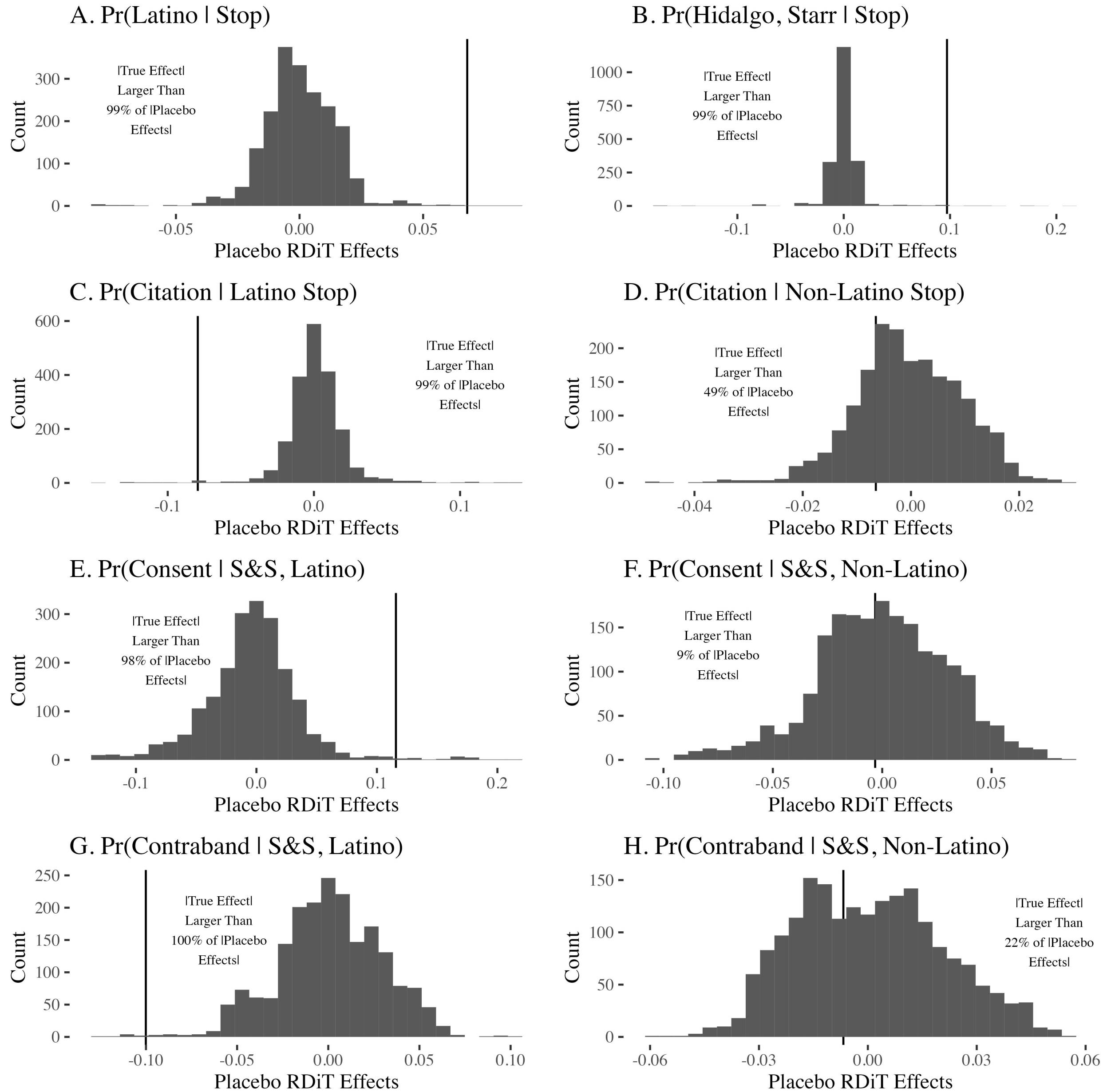

Figure 18: Pre-*OSS* temporal placebo effects. Each panel denotes a different outcome analyzed; all are measured statewide. Dashed vertical line denotes the true post-*OSS* effect. Annotation denotes the proportion of placebo effects (in absolute value) the true post-OSS effect (in absolute value) is larger than.

AE Donut Hole Re-estimation

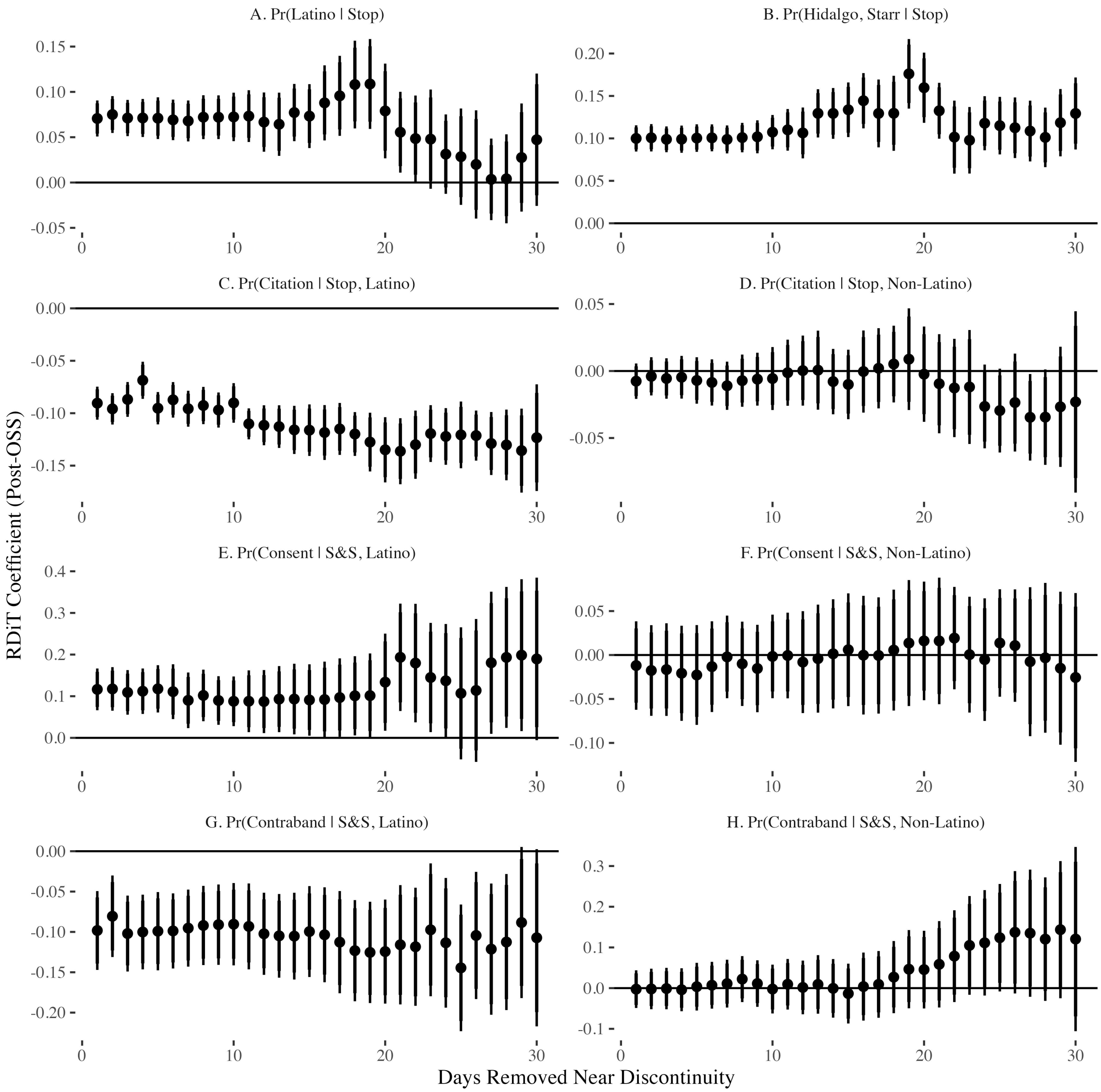

Figure 19: Donut hole RDiT effect (x-axis) re-estimation after removing days near discontinuity (y-axis). Each panel denotes a different outcome analyzed. 95% CIs displayed from robust SEs

AF OSS: Crashes Falsification Test (Texas)

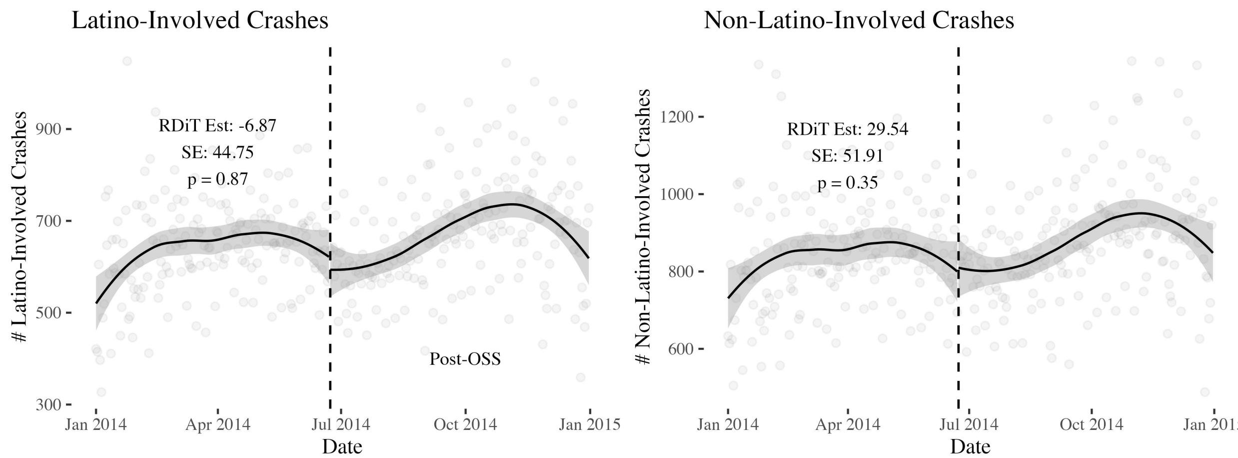

Figure 20: OSS did not increase traffic crashes involving Latinos or non-Latinos throughout the state of Texas. The left panel characterizes the daily number of traffic crashes between January 2014-January 2015 that involve a Latino individual. The right panel characterizes the daily number of traffic crashes between January 2014-January 2015 that involve a non-Latino individual. The dashed vertical line denotes the onset of Operation Strong Safety. Annotations denote the mean-squared optimal bandwidth regression-discontinuity-in-time effect of Operation Strong Safety on the number of crashes (polynomial = 1). Compare with Figure C19 in Dias et al. (2024), which shows a similar pattern for crashes for all motorists in Hildalgo/Starr counties.

AG OSS: Crashes Falsification Test (Hidalgo/Starr)

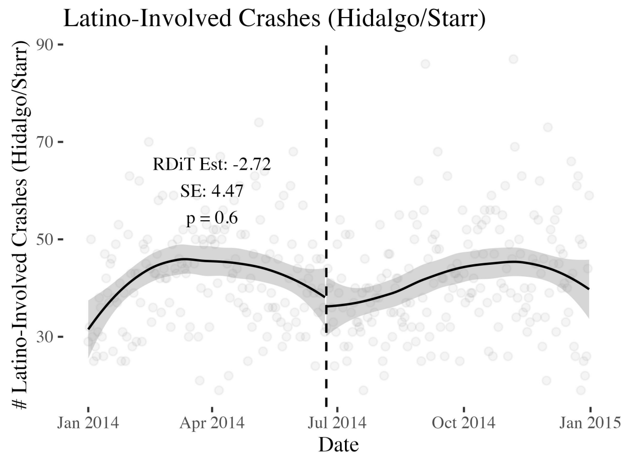

Figure 21: OSS did not increase traffic crashes involving Latinos in Hidalgo and Starr counties specifically. The plot characterizes the daily number of traffic crashes between January 2014-January 2015 that involve a Latino individual in Hidalgo and Starr counties only. The dashed vertical line denotes the onset of Operation Strong Safety. Annotations denote the mean-squared optimal bandwidth regression-discontinuity-in-time effect of Operation Strong Safety on the number of crashes (polynomial = 1). Compare with Figure C19 in Dias et al. (2024), which shows a similar pattern for crashes for all motorists (not only Latino motorists) in Hildalgo/Starr counties.

AH OSS: Subsetting To Experienced Police

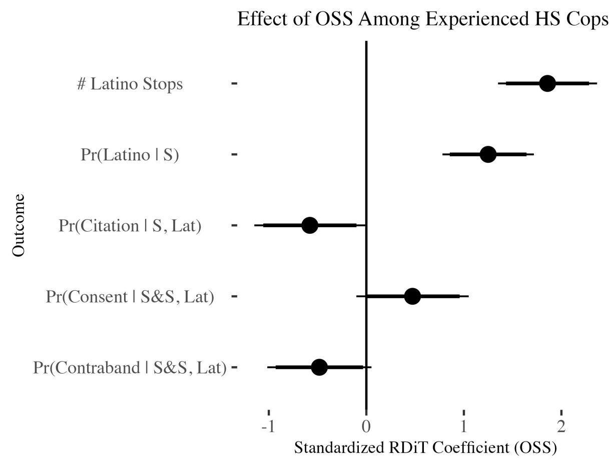

Figure 22: OSS increased racially disparate and inefficient policing even among officers who were experienced in policing Hidalgo and Starr counties. This plot characterizes mean-squared optimal bandwidth regression-discontinuity-in-time effects (x-axis) of operation strong safety on our outcomes of interest (y-axis) subsetting the Texas Department of Public Safety traffic stop data only to police officers who spent time engaging in traffic stops in Hidalgo and Starr counties in the 2 months before the onset of Operation Strong Safety. We exclude police officers who newly start engaging in traffic stops after OSS from this analysis. Compare with Table C6 in Dias et al. (2024), which similarly finds that experienced police officers contributed to increasing searches and declining contraband recovery in Hildalgo and Starr counties; here, we include only stops of Latino drivers.

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