

Immigration Enforcement, Policing, and Profiling

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3 When does immigration enforcement cause police to target Latino
4 drivers in traffic stops? We address this question with two
5 studies. First, using a dataset covering dozens of large counties and
6 evaluating the staggered onset of Secure Communities (increasing
7 enforcement) and sanctuary policies (decreasing enforcement), we
8 study the effect of immigration enforcement intensity—defined as
9 the likelihood of deportation—on racially disparate traffic stops. We
10 find no evidence that increased immigration enforcement intensity
11 leads to more traffic stops of Latino drivers (either proportionally
12 or in absolute terms) or that decreased immigration enforcement
13 leads to fewer criminal arrests of noncitizens. But, neither Secure
14 Communities nor sanctuary policies directly engage local police.
15 By contrast, in our second study, we evaluate the effect of a 2014
16 Texas initiative, Operation Strong Safety, in which state police
17 explicitly adopted immigration enforcement goals. We find strong
18 evidence that Operation Strong Safety discontinuously increased
19 traffic stop rates of Latino drivers and decreased the rate at
20 which stops of Latino drivers produced citations and seizures
21 of contraband. The contrast between these two results—a null
22 effect of changing federal enforcement intensity and a large effect
23 of a state program targeting immigrants—suggests a hypothesis
24 that we hope other scholars will test systematically: that police
25 respond to organizational incentives, and therefore that more direct
26 involvement in immigration enforcement produces more racially
27 disparate policing.

Significance

28 Consistent with anecdotal and
29 journalistic accounts, we provide
30 evidence that directly involving local
31 police in immigration enforcement
32 increases racially disparate policing
33 against Latinos. Where federal
34 immigration enforcement relies
35 indirectly on local arrests, by contrast,
36 we find no effect on policing. Our
37 findings have implications for
38 policymakers deciding whether to
39 involve local police in street-level
40 immigration enforcement.

41 Immigration Enforcement | Police Behavior | Bureaucratic Behavior | Racial Profiling

42 The second Trump administration has made cooperation with local police
43 a lynchpin of its mass deportations effort (1). A field of legal scholarship,
44 known as “crimmigration,” is devoted to the overlap between criminal and
45 immigration law (2, 3). Yet the relationship between traffic stops, arrests, and
46 immigration enforcement has nonetheless received relatively little empirical
47 study. Existing studies have found that immigration enforcement does
48 affect policing in the (until very recently) few jurisdictions with explicit
49 cooperation agreements (“287(g)” and/or intergovernmental service agreements)
50 with federal immigration authorities (4–8). Counties and states with those
51 agreements choose to have their police forces enforce immigration laws, and
52 scholars have found evidence that that choice leads to abusive police practices
53 in immigrant communities (4, 7) (but not always, see (9)).

54 We conduct two empirical studies of immigration enforcement programs that
55 depended on arrests by state or local police. Together, our results generate
56 a hypothesis, which we hope that future research will test, about *under what*
57 *conditions* immigration enforcement affects policing. We expect immigration
58 enforcement goals to affect police behavior where police departments *themselves*
59 pursue immigration-related goals.

60 Our first study examines the effects of changes in federal enforcement
61 intensity—the chance that a local arrest will lead to a deportation—on
62 traffic stops and local arrests. Scholars have noted—but not tested—the
63 possibility that increased federal immigration enforcement creates incentives
64 for police to stop and arrest people who they suspect are noncitizens (10–12).

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To the extent that the relationship between federal immigration enforcement and local police behavior has been tested, it has not directly focused on arrests against Latinos, only focuses on policing in a specific state (e.g. North Carolina), and examines policies that do not directly mandate local police increase the intensity of street-level stops to enforce immigration (13, 14).

The hypothesis is that, when the probability of transfer from local criminal custody to federal immigration custody rises, police officers may attempt to place more noncitizens in local criminal custody. We test that hypothesis in our first study, and we find no evidence that, when the number of local deportations rises (or falls), police are more (or less) likely to stop Latino motorists or to arrest noncitizens. This conclusion—that variation in the intensity of federal enforcement has little effect on police behavior—follows from three findings. First, we combine data on the staggered rollout of the federal Secure Communities program with traffic stop data from the Stanford Open Policing Project to evaluate whether the Secure Communities program increased traffic stops of Latino drivers. Second, we use the same traffic stop data to evaluate the effect of sanctuary policies, which constrain transfers from local to federal custody and thereby reduce deportations (15) and are intended to build trust between police and immigrant communities (16). 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) (17).

We find no evidence that the Secure Communities program, or the sanctuary policies that counteracted it, affected police behavior in making traffic stops of Latino drivers. Nor do we find evidence that sanctuary policies affected police decisions to arrest noncitizens. Finally, we confirm that these null results did not depend on the local political environment: the results are similar in counties that favored 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 a change in the already-small chance that a local arrest will lead to deportation does not affect police behavior. But the lack of an effect of this back-end change hardly suggests that immigration enforcement cannot lead to disparate policing. And our second study shows that it can.

Our second study evaluates the effect of Texas’s Operation Strong Safety program, adopted in 2014 in response to a spike in arrivals at the southern border. (One of us previously studied the effect of Operation Strong Safety on consent searches (18); here, we evaluate its effect on stops of Latino drivers). That program moved Texas Department of Public Safety resources to two heavily Latino counties (Hidalgo and Starr) along the southern border for the stated purposes of combating human and drug trafficking. We take advantage of the sudden implementation of this immigration-focused enforcement program (announced only two days ahead of time) to evaluate its effects. We observe a sudden, large jump in the number of stops of Latino drivers, with an

accompanying sudden drop in the citation rate and the rate at which police discover contraband. Using methods proposed by (19) and (20), we argue that these findings imply an increase in racial profiling in traffic stops in Hidalgo and Starr counties.

Our two studies together suggest a hypothesis, which we hope others will test, about when and how immigration enforcement may lead to racial profiling in traffic stops. Our first study finds that increases in the chance of deportation conditional on arrest do not, absent something more, change police behavior. But our second study shows that when police agencies dedicate themselves directly to immigration enforcement, racial profiling can result. When the enforcement mandate came from the police agency itself, police officers responded.

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 the incentives set by their supervisors (21–23). In counties and states not working directly with ICE, officers have little incentive to pursue traffic stops that might lead to deportations, particularly given that deportations are a rare consequence of arrests. By contrast, where a police agency adopts an immigration purpose explicitly, as during Operation Strong Safety in Hidalgo and Starr counties, officers do face an incentive to profile Latino drivers.

Second, we add to work on the drivers of immigration enforcement (15, 24) by clarifying that Secure Communities and sanctuary policies produced their effects on deportations directly, not by causing police to arrest more (or fewer) noncitizens. The lack of an effect on policing in our first study contrasts with the more established finding, which matches our second study, that federal-local immigration enforcement (“287(g)”) agreements do shape police behavior and lead to racial profiling (4–7).

Our findings on the (non)effects of Secure Communities and sanctuary policies add to the large literature in political science and economics on the harms of deportations, suggesting that those harms are imposed directly, through threatened and actual expulsions, rather than indirectly, through changes in police behavior.

First, the political effects of increased immigration enforcement likely reflect increased deportations rather than changes in policing. Political scientists have typically found that immigration enforcement, as well as immigrant-hostile laws and proximate experiences with the deportation system, have a mobilizing effect. Deportations, immigrant-hostile laws, and proximate experiences with the deportation system have been found to increase Latino voter turnout (25), increase political information among newly naturalized Latinos (26, 27), decrease support for the Republican party among Latinos (28), and increase protest participation among Latinos (29) (but see (30), who find proximate experiences with deportation depress turnout). Additionally, extensive qualitative work suggests that immigration-related policing can play a key mobilizing role for social movements: immigrant communities in Maricopa County, for example, organized

251 to counter abusive police practices there that targeted
252 Latino citizens and noncitizens (31).

253 Scholars have also found that these political effects were
254 accompanied by economic and health costs. Research finds
255 that increasing local immigration enforcement causes sev-
256 eral harms, including reduced employment (32), reduced
257 student achievement (33), reduced school enrollment (34),
258 reduced use of public benefits (35, 36), and reduced birth
259 weight (37). All of these findings depend on variation in
260 the type of immigration enforcement at issue in this study:
261 deportations that begin with an arrest by a local police
262 officer, rather than a federal immigration officer.

263 Our first study adds to this literature in political
264 science and economics by testing a mechanism through
265 which immigration enforcement might produce these many
266 effects. Because Secure Communities relies on arrests by
267 local police, it could harm immigrant communities either
268 through increased deportations or through increased police
269 stops of Latinos (or both). Harm through policing is
270 plausible given that many studies of Secure Communities
271 have found that the program harmed Latino *citizens* as
272 well as noncitizens (32, 34–36). These harms to citizens
273 could reflect changes in policing: some advocates and
274 scholars have suggested that local police might use race
275 as a proxy for immigration status and therefore stop
276 Latino drivers more often when they know that an arrest
277 could lead to deportation (4, 12, 38–40); indeed, some
278 scholars describe the variation in Secure Communities
279 enforcement as variation in “immigrant policing” (41),
280 and many scholars suggest that the political effects of
281 immigration enforcement reflect the “racialized threat”
282 of that enforcement (42). We test that hypothesis
283 by examining the effects of variation in immigration
284 enforcement on traffic stops of Latino drivers and arrests
285 of noncitizens.

286 Our results are consistent with those of other studies
287 finding little effect of immigration enforcement on admin-
288 istrative outcomes in the criminal justice system. Some
289 research, (13, 43), for example, finds no effect of Secure
290 Communities on criminal arrests or police efficiency. Our
291 results are also consistent with the large body of evidence
292 finding no relationship between immigration enforcement
293 and crime (13, 43–45).

294 Finally, our results also add to the small but growing
295 literature on the partisan politics of local immigration
296 enforcement. Our first study’s null finding is consistent
297 across partisan environments: it persists in counties with
298 both very high and very low shares of the population
299 voting for Trump in 2016. This first study’s result is
300 consistent with that of (46), who shows that Democratic
301 sheriffs (elected in close races) were no more or less likely
302 than their Republican counterparts to enact local sanctu-
303 ary policies. Our second study’s result complicates this
304 picture: Operation Strong Safety was highly politicized,
305 with real effects.

306 Together, our findings contribute to the scholarship
307 on the ways in which immigration enforcement and
308 local policing are, and are not, intertwined. Secure
309 Communities deportations produce their political and
310 economic effects directly, through deportation, rather
311 than indirectly, through changes in police behavior.
312 Changes in police behavior, by contrast, arise when police
313 departments directly pursue immigration aims. As the
314 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.

Context. In order to find and deport noncitizens living
within the United States—as opposed to noncitizens who
have recently crossed the border—the federal government
relies overwhelmingly on arrests by local police (47). That means that the large majority of Immigration and
Customs Enforcement (ICE) arrests take place in jails and
prisons, rather than at large.* This reliance on criminal
arrests for interior deportations means that immigration
and criminal enforcement are necessarily linked. 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 (35),
and local sanctuary policies, which decreased them (15). We
use this variation over time and across counties to
test whether increased or decreased deportations affected
traffic stops of Latino motorists or arrests of noncitizens.

The Secure Communities program, which dates to 2008,
linked U.S. Immigration and Customs Enforcement (ICE)
and FBI databases. Since the (staggered) onset of that
program, whenever a county jail books in a person arrested
by local police, 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) (48). The
IDENT database is drawn principally from Custom
and Border Protection (CBP) records of noncitizens’
entry into the United States, including apprehensions
of people attempting to cross the border between ports
of entry (49). IDENT also contains at least some U.S.
citizens’ fingerprints, such as those of noncitizens who
have naturalized and of citizens who have opted into
trusted traveler programs. The FBI nonetheless uses an
IDENT match as 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 (48). This
process produces the database matches that we treat as an
imperfect proxy for the number of arrests of noncitizens
in each county and month.

If ICE decides—after receiving a database match from
the FBI—to attempt to deport an arrestee, they typically
issue a detainer request (50). 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
(35), and that interoperability was rolled out over time

*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.

379 to different counties, creating an opportunity for causal
380 inference. We exploit that opportunity, as many have
381 done before us; by investigating the effect of Secure
382 Communities on traffic stops, we test one of the possible
383 mechanisms by which immigration enforcement imposes
384 the harms that previous studies have demonstrated.
385 Similarly, our sanctuary results take advantage of the
386 fact that state and county sanctuary policies, which
387 counteracted Secure Communities, were implemented at
388 different times. These policies reduced deportations by
389 about a third, on average (15). The details of sanctuary
390 policies vary from jurisdiction to jurisdiction; following
391 (15), we code counties as sanctuary counties if their policies
392 include refusals to comply with ICE detainer requests.

393 Finally, critically, we do not study 287(g) agreements:
394 agreements between the federal government and local
395 governments to cooperate on immigration enforcement.
396 In states and localities that sign such agreements, state
397 and local officers are actually deputized to act as federal
398 officers: in so-called jail enforcement agreements, local
399 officials question inmates about their immigration status
400 and perform immigration arrests in the jail, and in so-
401 called task force agreements, local officials can perform
402 immigration arrests outside of jails as well (7, 469-70).

403 **Hypotheses.** We test the hypothesis that, when local
404 criminal arrests become more likely to result in transfers
405 to federal immigration custody, police will become more
406 likely to stop Latino motorists. We also test the converse
407 of this hypothesis: when local criminal arrests become less
408 likely to lead to transfers to federal immigration custody,
409 police will become less likely to stop Latino motorists.

410 These hypotheses are plausible in the light of prominent
411 examples of increased policing of immigrant communities
412 when counties have entered into cooperative agreements
413 with federal immigration enforcement authorities. Per-
414 haps the best known example involves Maricopa County.[†]
415 There, soon after Sheriff Joe Arpaio entered a 287(g)
416 agreement with ICE, sheriffs' deputies began to organize
417 saturation patrols, which resulted in disproportionate
418 traffic stops and arrests of Latinos.[‡] Under Sheriff's
419 Office's 287(g) agreement with the federal government,
420 the office was authorized to engage in immigration
421 enforcement and explicitly aimed to target noncitizens for
422 stops.[§] The Sheriff's Office also explicitly (and unlawfully)
423 considered race as a factor in making such stops, targeting
424 Latino motorists.[¶] The Sheriff continued these practices
425 even after the federal government ended the cooperative
426 agreement, doing all it could to cause more deportations.^{||}

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

443 policies lowered the chance that an arrest would lead to
444 deportation and might have made police less likely to
445 make such stops and arrests.

446 Data.

447 **Secure Communities Data.** We merge data on the county-level
448 onset of Secure Communities (S-Comm) with traffic stop
449 data from the Stanford Open Policing Project (SOPP)
450 to evaluate if S-Comm shifted police behavior. We use
451 a set of criteria to generate a balanced panel of traffic
452 stop data at the county/department/month-level. First,
453 the temporal domain must overlap with the time period
454 in which S-Comm is an active Federal program (October
455 2008-November 2014). Second, there must be at least 10
456 months of pre-treatment data, that is, ten months before
457 the onset of S-Comm in a department/county. Therefore,
458 we include only counties/departments in which S-Comm
459 activation occurred after July 2009. Third, consistent
460 with our sanctuary policy data detailed in the Sanctuary
461 Policy Data section, we use information from the top 10%
462 of Latino counties (by population proportion) in 2010.

463 These criteria construct our sample. Because the SOPP
464 data is relatively limited in time, we only have data on 10
465 states, 12 police departments (including 6 state highway
466 patrols: MA, NC, SC, TN, TX, VA), and 58 counties. But,
467 these data capture a significant proportion of the Latino
468 population. These data cover 8.6 million Latinos based
469 on 2010 ACS estimates, equivalent to roughly 17% of the
470 Latino population.^{**} Our data includes demographically
471 relevant counties: Los Angeles (CA), San Francisco (CA),
472 Tarrant (TX), Cameron (TX), and Kern (CA). See SI
473 Fig. S1 for a map characterizing geographic coverage
474 of the Secure Communities traffic stop data. For each
475 county/department/month, we count the number of stops
476 of Latinos, non-Latinos, and whites. See SI Table S1 for
477 summary statistics of our S-Comm data.

478 **Sanctuary Policy Data.** We merge county sanctuary policy
479 onset data from (15) with traffic stop data from the SOPP
480 to evaluate if sanctuary policies change police behavior.
481 The sanctuary data includes information from all but 12
482 of the 314 largest 10% of Latino counties between 2010-
483 2015. After merging the sanctuary and SOPP data, we
484 have a 72 month panel including 141 unique counties and
485 29 unique police departments (11 state patrols: CA, CO,
486 FL, MA, NY, NC, OH, SC, TN, TX, VA). These counties
487 cover 51% of the Latino population and include localities
488 with significant Latino populations: Los Angeles, Houston,
489 Dallas, San Antonio, and San Diego.^{††} See SI Fig. S2 for a
490 map characterizing geographic coverage of the Sanctuary
491 traffic stop data. For each county/department/month, we
492 count Latino, non-Latino, and white stops. See Table S2
493 for summary statistics of our Sanctuary and stops data.

494 One may be concerned that much of our traffic stop
495 data come from state patrols instead of local police
496 since state police may have weak relationships with local
497 county jails that cooperate with Federal immigration
498 authorities for the purposes of facilitating detainees and
499 deportations. However, our analysis of state patrol traffic
500 stop data demonstrates individual state police primarily
501 operate within a single county, suggesting they have strong
502 links to their local community.^{||}

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

500 [‡] *Id.* at 825-26.

501 [§] *Id.*

502 [¶] *Id.*

503 ^{||} *Id.*

505 ** In 2010, there were 50.5 million Latinos nationally.

506 †† Our sanctuary policy sample covers 26 million Latinos (2010 Census).

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Table 1. Effect of Secure Communities on Stop Outcomes

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

Note: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Model 1 evaluates the effect of Secure Communities under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-4 use state clustered SEs instead of county/department clustered SEs (Model 1). Model 3 adjusts for state \times month fixed effects. Model 4 adjusts for a county/department-specific trend. Panels A-B display effect estimates of Secure Communities on logged Latino stops—the probability that a stop involves a Latino driver respectively. Effects displayed in Figure 1 are from column 3.

relationships to local county jails since that is where they would presumably send their arrestees (See SI Fig S3).

Moreover, 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 SI Fig. S4 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 the police are arresting more noncitizens. See SI Table S3 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:

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

where Y_{cdm} is the number of logged Latino stops (+1 to facilitate identification), the proportion of stops that are Latino, the number of logged ICE database matches, or the proportion of ICE database submissions that led to matches in a given department (d) within a given county (c) on a given month (m). Policy_{cdm} is a binary indicator equal to 1 when a department operates in a county that has activated S-Comm in the S-Comm dataset or a sanctuary policy in the Sanctuary Policy dataset. τ is the coefficient of interest. If S-Comm motivates increases in policing against Latinos, τ should be positive. If sanctuary policies reduce 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 (35), we account for time-varying common shocks within state by including state-by-month fixed effects (δ_{sm}). ε_s are robust errors clustered by state since some sanctuary and S-Comm policies were either adopted directly by state governments or all counties within a state simultaneously (15).

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:

$$Y_{cdm} = \sum_{k=0}^k \beta^k P_{cdm}^k + \alpha_{cd} + \gamma_m + \delta_{sm} + \varepsilon_s$$

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

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 the number of stops of Latino drivers. Our preferred difference-in-differences estimate suggests that S-Comm *decreases* traffic stops of Latinos by 4%, a statistically insignificant effect ($p = 0.49$, see Table 1, Panel A, Model 3). That effect is equivalent to 12 fewer stops within a given county/department/month relative to a pre-treatment baseline of 383 traffic stops. The confidence interval covers -0.15-0.07, 11% in each direction. A standardized effect of 0.20 is often considered "small" (51). Here, the 95% CIs of the standardized effect of S-Comm on logged Latino stops do not overlap with ± 0.2 (52): the standardized effect of S-Comm on logged Latino stops is -0.05, and the standardized confidence intervals cover -.17-0.07. Thus, the equivalence test bolsters our null result.

Second, and more meaningfully, we find no evidence that S-Comm changes the chance that a traffic stop will involve a Latino driver. S-Comm decreases the proportion of stops that are Latino by 0.4 percentage points (pp., $p = 0.37$, see Table 1, Panel B, Model 3), a shift equivalent to 1.3% of the pre-treatment mean (22 pp.). These null effects are not sensitive to clustering by state. 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. Moreover, these estimates are quite precise: a single percentage point increase in the proportion of stops involving Latinos is barely inside the 95% confidence interval (-0.012-0.004). Again, given that the standardized effect of S-Comm on the proportion of stops that are Latino is -0.02 and

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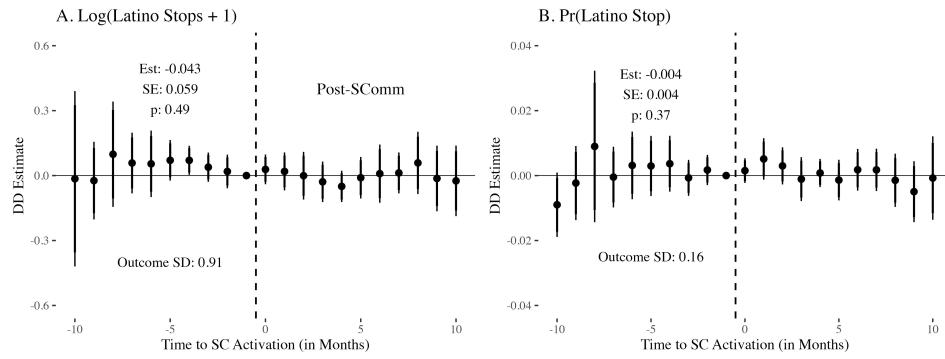


Fig. 1. Event study estimates characterizing effect of Secure Communities (S-Comm). See Table 1, Column 3 for corresponding difference-in-differences regression results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences estimate for the effect of S-Comm. 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 \times month fixed effects. Each panel uses a different outcome and/or comparison group (specified by panel title). Annotations denote generalized (non-event study) difference-in-differences estimates, standard errors, and p-values. 95% CIs displayed derived from state-clustered SEs.

the standardized confidence intervals for the effect is $-0.07\text{--}0.03$, an equivalence test assuming a small effect size of ± 0.20 standard deviations rules out large effects. The results of this equivalence test again increase our confidence in the null result.

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

Given the staggered implementation of S-Comm across U.S. counties, we address the risk that heterogeneous treatment effects by county activation cohort may 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 (53), which limits the usage of problematic comparisons with already-treated units, and derive statistical results that are similar to our main specification (SI Fig. S5). Our null S-Comm effects also replicate using a staggered difference-in-differences approach and state patrol traffic stop data with all counties in both California and North Carolina, suggesting our results are not driven by our sample truncation to the top 10% Latino U.S. counties (SI Fig. S6 and S7). Prior research shows police may misreport the race of Latinos they stop to mitigate racial bias patterns in traffic stop data (54). The Texas state patrol SOPP data includes names of those stopped, allowing SOPP to correct Latino race/ethnicity classification with name and geography data. We replicate our approach using only the corrected Texas state patrol traffic stop data in all Texas counties and still uncover null results for Latino stops,^{§§} suggesting our overall results are not driven by racial misclassification on part of police in the wake of SComm implementation (SI Fig S8). Our results are not driven by outcome measurement, the null effects of S-Comm on policing against Latinos 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 (SI Table S4). Our results do not change if we cluster standard errors at the county-level for our preferred specification (SI Table S5).

The Secure Communities program, by integrating FBI and ICE databases, increased the chance that a local arrest would lead to a transfer to federal immigration custody. These results suggest that that increasing chance of a transfer to ICE custody on the back end had little effect on police behavior. Our confidence in this result is increased by the fact that the onset of sanctuary policies—which disrupted the functioning of Secure Communities—also had no observable effect on disparate traffic stops against Latinos (see SI Section 11, SI Table S6, SI Fig S9, for more details).

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

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

^{††}Control county/departments are "yet-to-be-treated" since Secure Communities is eventually activated in all U.S. counties.

^{§§}The Texas state patrol results for the Latino stop proportion outcome are problematic due to visible pre-SComm trend violations in the event study, so we do not attach significant weight to them.

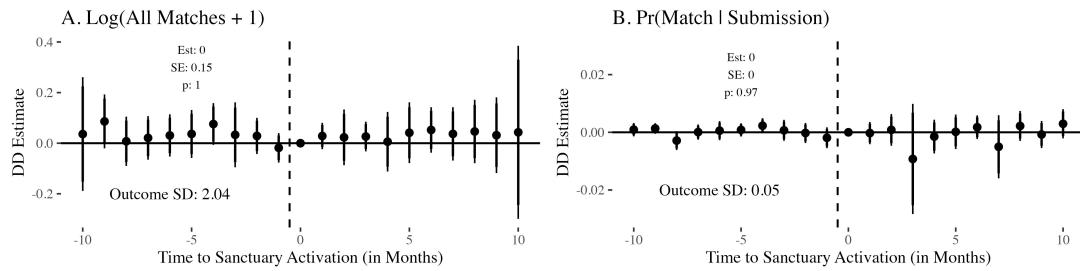
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Fig. 2. Event study estimates characterizing effect of sanctuary policy on IDENT outcomes. See Table 2, Column 3 for corresponding difference-in-differences results. The x-axis is time to policy activation (in months). The y-axis is the differences-in-differences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

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

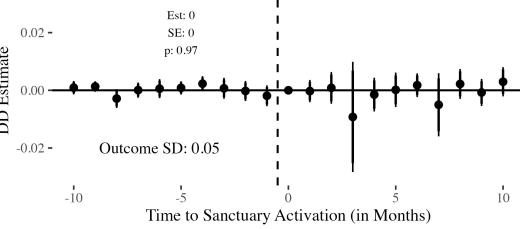
Note: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Model 1 evaluates the effect of sanctuary policies under a general difference-in-differences approach without higher dimensional fixed effects. Models 2-5 use state clustered SEs instead of county clustered SEs. Model 3 adjusts for state x month fixed effects. Model 4 adjusts for a county-specific trend. Model 5 adjusts for an additional Secure Communities 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 they are not identified (the outcome depends on S-Comm activation).

The null 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 (53) (SI Fig. S10). Additionally, our IDENT match outcome results are not driven by disaggregation by severity of crime perpetrated by arrested immigrant (Level 1-3, 3 = most severe, 1 = least severe). Regardless of crime severity, there is no increase in IDENT matches due to sanctuary policy activation (SI Table S7). Our null results replicate using county, instead of state clustered standard errors (SI Table S8).

Taking these three sets of results together, we find no evidence of any systematic effect of enforcement intensity on police stop or arrest behavior. Moreover, our null result is not masking heterogeneous effects by political or demographic factors. In SI Section 18 (see also SI Tables S9-S14), we test whether there are heterogeneous effects of S-Comm or sanctuary policies between politically conservative or liberal counties in addition to between counties with more or less Latinos and immigrants. We find no evidence of such heterogeneity, solidifying our null result.

[¶]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).

B. Pr(Match | Submission)



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 bureaucratic behavior. 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; in Study 2, we add to this evidence. We test the effect of “Operation Strong Safety (OSS),” a Texas state policy jointly implemented by the Texas Governor and Chief of the Texas Department of Public Safety (DPS) to increase traffic enforcement at the border for the stated purpose of fighting human smuggling, drug trafficking, and undocumented immigration. Prior research examined this program to study the effect of consent searches (without considering race) in Hidalgo and Starr counties (18); we build on that work to study the statewide effect of OSS on the disparate policing of Latino drivers.

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 SI Fig. S11 for a map of the OSS area of operations, see SI Fig S12 for state spending trends on border enforcement pre- and post-OSS). The policy only became public two days before taking effect, and news coverage 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 (18) and SI Fig. S13-S15.

Although OSS did not formally target undocumented immigration itself, journalists and other observers reported that officers began to focus on unauthorized immigration—and stops of Latino drivers (55–58). In Hidalgo and Starr counties, which are overwhelmingly Latino, the number of stops more than doubled overnight (see SI Fig. S16) Moreover, prior research shows 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) (18). We build on prior work by estimating the effect of OSS on stop, search, and hit rates by race.

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

Data and Design. We use Texas DPS highway patrol data from SOPP to evaluate whether OSS increased disparate policing of Latinos statewide. We use data on all DPS traffic stops between January 1, 2009–December 31, 2016 ($N = 15,753,883$). Importantly, SOPP has re-coded the Latino race variable so that stops that are reported by the DPS as non-Latino are re-classified as stops of Latinos if the subject stopped has a more than 75% chance of being Latino based on the joint probability of being Latino conditional on their surname and location-of-stop (county) (59).*** This adjustment is appropriate given that prior research has found that the Texas DPS often incorrectly classifies Latinos as “white” to manipulate traffic stop statistics by race/ethnicity (54).

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

We use several measures to assess whether OSS increased unwarranted policing against Latinos. We measure two citation rates at the daily-level: 1) the proportion of Latino traffic stops that led to a citation instead of a warning (*Latino citation rate*) and 2) the proportion of non-Latino traffic stops that led to a citation as opposed to a warning (*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 the DPS increasingly stopped Latinos for superfluous reasons unrelated to actual traffic violations.

We also measure two daily-level consent stop-and-search rates: 1) the proportion of searches of Latinos that were conducted on the basis of driver consent as opposed to probable cause (*Latino consent rate*) and 2) the same consent search proportion for non-Latino drivers (*non-Latino consent rate*).†† Unlike probable cause searches, where officers must have reasonable suspicion to initiate a search, a consent search requires no justification as long as the officer asks for consent to search. Unsurprisingly, consent searches are less likely than probable cause searches to lead to identification of criminal activity (18). Drivers rarely say no to consent searches even if the officer has limited cause to conduct a search (18, 60).

In addition to consent stop-and-search rates, we measure daily contraband recovery stop-and-search 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, and/or (illicit) money in addition to the identification of human smuggling. Lower hit rates after OSS may suggest that searches became increasingly superfluous and unwarranted after OSS.

Finally, we measure the proportion of daily Latino stop-and-searches that are the result of racially disparate policing. To do this, we follow (19) and (20) and rely on three assumptions: 1) mandatory reporting (police report stop-and-searches when they happen); 2) mediator monotonicity (there are no circumstances in which a white person would be stopped-and-searched but an identically situated Latino would be allowed to not be searched conditional on a stop); and 3) treatment ignorability (no factors jointly affected the onset of Operation Strong Safety and the *Latino/white hit rate* (more on this later)). The quantity of interest (*anti-Latino bias*) is the difference in the Latino and white hit rate normalized by the white hit rate:

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

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

Given we expect OSS to increase disparate policing of Latinos, we expect OSS to: increase $Pr(Latino)$; increase $Pr(HS)$; reduce the *Latino citation rate* while having no commensurate effect on the *white citation rate*; increase the *Latino consent rate* while having no commensurate effect on the *white consent rate*; decrease the *Latino hit rate* while having no commensurate effect on the *white hit rate*; and increase *anti-Latino bias*. We focus on comparing the Latino hit rate to the white, instead of non-Latino, hit rate when measuring *anti-Latino bias*, since we are interested in differential bias against Latinos versus whites.

Since the outcomes are measured at the day-level and the unit of analysis is the day ($N = 2557$ 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 SI Table S15.

We use a regression discontinuity-in-time (RDiT) design to assess the discontinuous, immediate effect of OSS on our outcomes. The RDiT is an advantageous design because our coefficient estimates are less likely to be affected by secular differential time trends independent of OSS, and the unanticipated nature of OSS makes it reasonable to assume that driver characteristics or other factors associated with our outcomes of interest did not change immediately before and after OSS (e.g. the propensity to engage in criminal activity by race/ethnicity, weather, the ethno-racial distribution of the driving population). Continuity in driver characteristics by race/ethnicity is important for estimating shifts in anti-Latino bias because we are not necessarily interested in the effect of being Latino on our outcomes of interest (e.g.

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

††These consent search measures (as well as the hit rate measure below) are similar to the measures in (18), except we calculate them by driver race.

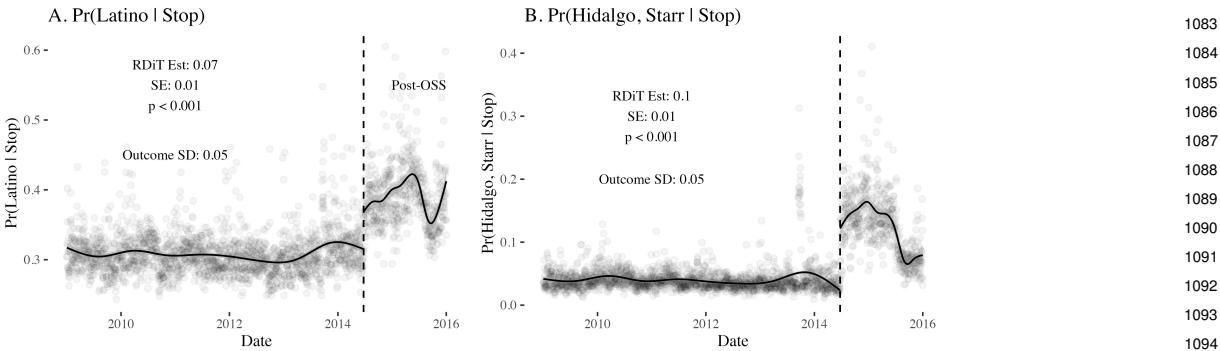


Fig. 3. OSS discontinuously increased Latino stops. Panels A-B characterize the proportion of DPS stops that are Latino and occurred in Hidalgo/Starr counties. Dashed vertical line = OSS onset. Loess lines fit on each side of the OSS discontinuity. Annotations = mean-squared optimal bandwidth RDiT estimate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

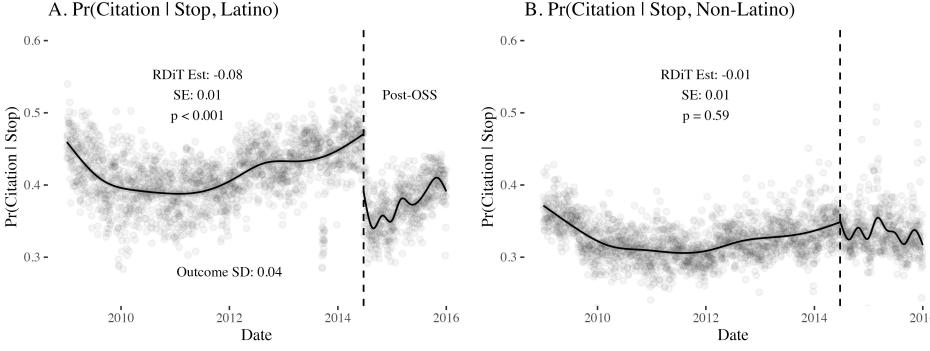


Fig. 4. OSS discontinuously reduced the Latino citation rate. Panels A-B characterize the proportion of stops that led to citations (instead of warnings) for Latinos and whites.

the citation, consent, and contraband recovery rate), but the change in the effect of being Latino discontinuously after OSS is implemented due to shifts in the operational priorities of the Texas DPS. Given the unanticipated and sudden nature of OSS, we assume that the effect of Latino ethnicity would have been the same after June 23rd as before OSS if OSS had not occurred. We present mean-squared optimal bandwidth RDiT estimates (61), with the running variable (days to OSS) to the 1st polynomial and a uniform kernel.

Results. Figure 3 displays RDiT estimates characterizing the effect of OSS on $\text{Pr}(\text{Latino})$ and $\text{Pr}(\text{HS})$. Consistent with expectations, OSS discontinuously increased the proportion of DPS traffic stops across Texas involving Latino drivers by 7 percentage points (pp., $p < 0.001$), a large effect equivalent to 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 OSS discontinuously increased the raw number of Latino stops, while producing no change in non-Latino stops (see SI Fig. S17).

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. ($p < 0.001$), a large 2 standard deviations of the pre-OSS daily outcome (Figure 4, Panel A). Conversely, OSS did

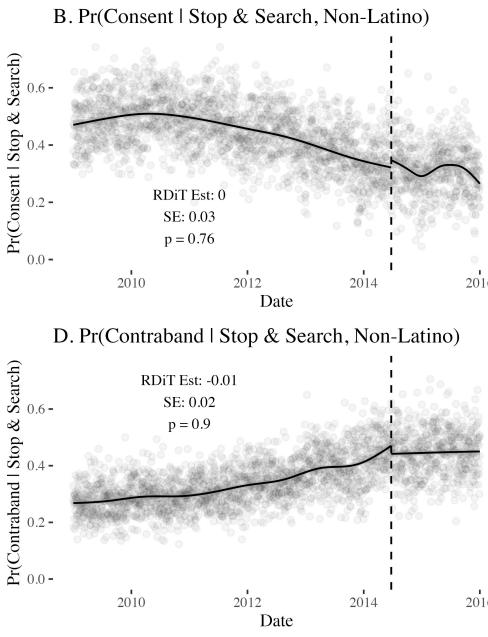
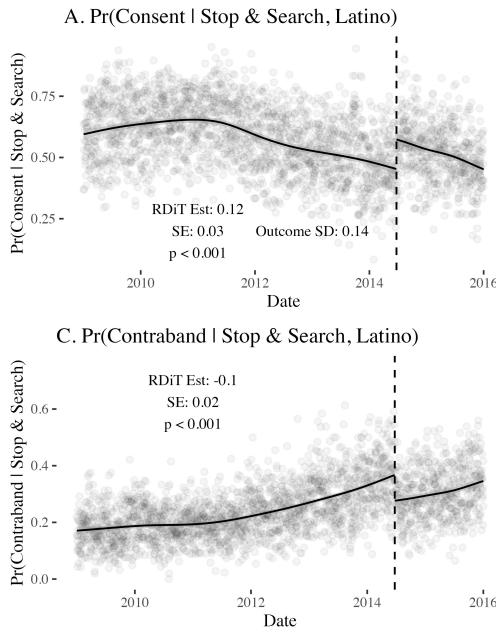
not reduce the non-Latino citation rate (Figure 4, Panel B), suggesting that OSS disparately affected Latinos and did not shift police behavior toward non-Latino drivers.

OSS also discontinuously increased the Latino consent search rate by 10 pp. ($p < 0.01$) while reducing the Latino hit rate by 10 pp. ($p < 0.001$), equivalent to 0.72 and 1 standard deviation of the respective pre-OSS outcomes (Figure 5, Panels A-B). OSS did not change the consent search rate or hit rate for non-Latinos (Figure 5, Panels C-D). 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.

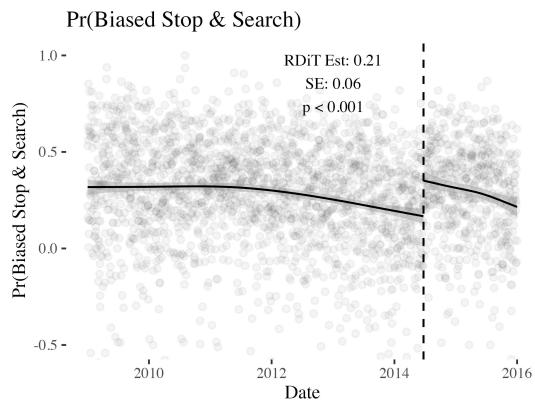
Finally, we use the approach proposed by (19) and (20) to assess whether OSS increased biased stop-and-searches against Latinos. RDiT estimates suggest that OSS discontinuously increased the rate of biased stop-and-searches by 21 pp. ($p < 0.001$), nearly 88% of the pre-OSS rate of biased stop-and-searches (Figure 6).

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

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 OSS did not reduce crime. First, there is no, or at least no sustained, increase in raw citations or contraband



1169 Fig. 5. OSS discontinuously increased the consent search rate and decreased the hit rate. Panels A-B show the consent search rate for Latinos and whites over time,
1170 Panels C-D show the hit rate for Latinos and whites over time. Dashed vertical line marks OSS onset. Loess lines are fit on each side of the OSS discontinuity.



1188 Fig. 6. OSS discontinuously increased discriminatory stop-and-searches
1189 against Latinos (relative to whites). Dashed vertical line denotes OSS onset.
1190 Loess lines fit on each side of the OSS discontinuity.

1192 recovered post-OSS (SI Fig. S18-19), implying OSS did
1193 not lead to a raw increase in the identification of citation-
1194 worthy activity or contraband despite a reduction in the
1195 citation or hit rate. Second, we use two independent
1196 research designs to show OSS did not reduce crime in the
1197 OSS area of operations (see SI Section 31 for details).

1199 Was OSS Effective at Identifying Undocumented Immigrants?. One
1200 of the implicit and explicit purposes of OSS was to prevent
1201 human smuggling and undocumented migration during
1202 the 2014 child migrant crisis (18). In SI Section 32,
1203 we demonstrate that OSS was ineffective at identifying
1204 undocumented migration or human smuggling. Thus, OSS
1205 increased racially disparate, inefficient, policing without
1206 meeting policy goals.

1208 Robustness Checks. Our RDIT estimates are similar if we
1209 use alternative polynomial (quadratic, cubic), kernel
1210 (triangular, Epanechnikov), and bandwidth (dividing

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optimal bandwidth by 2) specifications (SI Fig. S24), meaning our results are not driven by model specification choices. Our estimates are larger than the vast majority of fake pre-OSS temporal placebo discontinuities (SI Fig. S25), suggesting 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) (62), suggesting our results are not driven by anticipatory effects (SI Fig. S26).

We rule out if 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. Nevertheless, if Latinos were endogenously responding to OSS immediately post-OSS, Texas Latinos would be involved in less traffic crashes since they would drive more carefully to evade the DPS. We implement a falsification test and do not find OSS discontinuously reduced the number of Latino or non-Latino crashes throughout Texas or Hidalgo/Starr counties, suggesting our results are not explained by driver behavior in response to OSS (SI Fig. 27-28).

We rule out if our results are driven by inexperienced officers who typically patrol other counties being assigned to police Hidalgo and Starr counties post-OSS, leading to inefficient, racially disparate policing (see SI Fig. 29 for data on the increase in number of officers patrolling Hidalgo and Starr after OSS). To do this, we subset our stop data to officers who policed Hidalgo and Starr in the 60 days pre-OSS and re-evaluate the RDIT OSS effect on our outcomes. Our results do not change (SI Fig. 30), suggesting an influx of inexperienced officers into Hidalgo and Starr does not explain our results, but rather, the mandate to engage in immigration and border enforcement.

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Discussion

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

In our first study, we evaluate whether heightened federal immigration enforcement—a higher probability of deportation for noncitizens who have been arrested by local police—shifts police stop and arrest behavior. We find no evidence of such a shift. This null result is consistent across three empirical tests: of the effect of Secure Communities on traffic stops, the effect of sanctuary policies on traffic stops, and the effect of sanctuary policies on arrests of noncitizens. And the same null result holds across a wide variety of difference-in-differences and event study specifications, in addition to different political 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 numbers of Latino stops jumped, citation and hit rates suddenly fell, suggesting that the new stops were less effective than those that came before. Using an approach with (we think) reasonable assumptions, we conclude that biased searches of Latino drivers increased.

Why did immigration enforcement drive biased policing in one context but not another? 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.

Police officers' attention to organizational incentives is consistent with our precise null finding that sanctuary policies, which reduce deportations by a third (15), 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 correlated. Figure 7, Panel B, shows that correlation. Even though arrest rates are highly associated with deportation rates, rising deportation rates do not lead police officers to make more arrests, absent an organizational incentive to do so. Deportations depend on arrests, not vice versa.

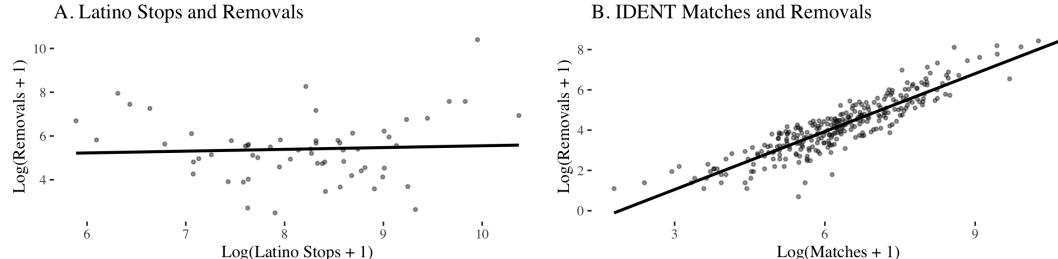


Fig. 7. Association between Latino stops, IDENT matches, and removals.

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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 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 (21–23). 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.^{†††}.

Our analysis has shortcomings. Given limitations in the accessibility of traffic stop data, our results do not generalize to the entire United States, nor do they capture police operations covering 100% of the Latino population. But our analyses do include contexts with a large Latino population (e.g. Los Angeles county). In addition, it is unclear why out-of-sample geographic contexts or police departments would be motivated differently in response to the policies we evaluate than the contexts/departments in our sample. 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 (SI Fig. S6 and S7).

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

^{†††}Examples include the Maricopa County's Sheriffs Office (discussed above) in addition to Operation Strong Safety.

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 7, Panel A). That descriptive fact should not be surprising—even though many deportations begin with convictions for traffic offenses—simply because deportations are so rare relative to traffic stops and to arrests. In 2014 and 2015, across our sample of the largest ten percent of counties by Latino population, about six percent of arrests triggered a match in ICE's database, and 11 percent of those matches resulted in deportations, meaning that under one percent of arrests resulted in deportations. Because our dataset does not connect traffic

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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 (4-8).

Data, Materials, and Software Availability. If accepted, all raw data and code will be made publically available on an OSF repository to fully replicate all analyses reported here and in the supplementary materials.