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Consent searches: Evaluating the usefulness of a common and highly discretionary police practice

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Abstract
We analyze the consequences of using driver consent as a basis for initializing a traffic stop-and-search compared to those searches based on probable cause. We find that consent searches are less likely to result in contraband recovery than are probable cause searches. Moreover, police agencies with a relatively higher reliance on consent searches find similar amounts of contraband and make a similar number of arrests as agencies doing much less searching but with a greater reliance on probable cause. These patterns are amplified along racial lines, and there is no discernible relationship between the use of consent searches and crime. We also provide causal evidence that corroborate these observational findings by examining the consequences of a Texas Highway Patrol policy, which suddenly increased the consent search rate in two South Texas counties. We show the contraband recovery rate discontinuously decreases when the consent search rate discontinuously increases.

INTRODUCTION

A surge of activism in response to highly publicized police abuses has generated widespread calls for policing reform in the United States. Proposals range from circumspect adjustments to police training to far-reaching reforms that would abolish the police altogether. As diverse as these options may seem, they are motivated by a common belief, supported by empirical and anecdotal evidence, that interactions between police officers and citizens are potentially hazardous and prone to violent escalations (Jacobs & O'Brien, 1998; Oberfield, 2012; Petrocelli et al., 2003). Of course, this is dangerous for officers and citizens alike. A successful reform agenda must therefore either change the protocols by which officers engage in contact with citizens to make it safer, or make that contact less frequent (or both). At the same time, Americans across the ideological and racial spectrum continue to be concerned about crime, confounding attempts at meaningful reform (Parket & Hurst, 2021).

We enter into this milieu by evaluating one area ripe for immediate reform: discretionary policing practices, specifically, the decision to stop and search citizens in the course of traffic or pedestrian stops. Police work includes a wide range of activities. Certain types of policing may be important either as preventative measures, or as appropriate societal reactions to crime. But other activities may be tangential to crime control, counter-productive, or otherwise irrelevant. In the debate about how to generate better policing outcomes, distinguishing one type of police activity from the other is paramount. To that end, we investigate the use of consent searches relative to their constitutionally constrained counterpart, probable cause searches. Consent searches, we discover, are a particularly stark example of a widespread policing activity that is not associated with increased public safety, even as the extant literature documents their deep societal cost, especially for communities of color.

Consent searches take place when an officer makes a traffic or pedestrian stop and, lacking evidence that would generate probable cause, simply asks for the civilian's permission to search their person or vehicle. In theory, the motorist has a constitutional right to privacy and can refuse the search, but an officer has no obligation to share this information and the power dynamics of a stop are such that citizens may find it difficult to exercise their rights. For these reasons, advocates are critical of consent searches, believing they serve as a de facto work around to the 4th Amendment (Burke, 2016). Yet, it was precisely this aspect that saw them rise to prominence during the tough-on-crime era of the 1990s, when they came to be regarded as an essential tool in pro-active policing that sought to maximize police–citizen encounters.

Existing scholarship finds consent searches are more likely than other types of searches to be implemented in a racially disparate way (Shoub, 2021). Moreover, by leveraging changes in police protocol that created dramatic reductions in the use of consent searches in a few North Carolina municipalities, researchers have shown they are less likely to recover contraband than other types of searches and that their frequent use does not appear to depress violent crime (Baumgartner et al., 2018; Epp & Erhardt, 2021). Studies on terry-stops in New York City (another type of high-discretion police search) have found similar results (Mummolo, 2018; Rosenfeld & Fornango, 2017).

We build on this research with the largest study of consent searches data availability currently allows, drawing from 900,662 observations of police searches conducted by 25 agencies in five states. We find: (1) consent searches are common. In fact, for most of the agencies in our sample they are the most frequently conducted search; (2) compared to probable cause searches, they are on average 30% less likely to successfully locate contraband; (3) Police agencies that rely heavily on consent searches do more searching overall than agencies that use proportionally fewer consent searches, but, crucially do not find more contraband or make more arrests; (4) There is no relationship between the use of consent searches and violent crime rates; and (5) Black civilians are more likely to be subject to a consent search, and searches of Black civilians are less efficient than are searches of their white counterparts.

While this analysis is broad, it does not allow us to rule out endogenous driver behavior. For example, drivers might conceal contraband in response to police tactics that emphasize consent searches. We therefore complement the descriptive findings with a design-based approach leveraging a Texas policy change that suddenly increased highway patrol traffic stops in addition to reliance on consent searches in two South Texas counties: Hidalgo and Starr. Using a regression discontinuity-in-time design in addition to daily stop data from the Texas highway patrol, we offer plausibly causal evidence that heightened consent search reliance corresponds to a sudden decrease in the contraband recovery rate without commensurate crime reductions.

During the 1990s, police agencies across the country developed new habits around a policing style that sought to manage crime through proactive attempts to locate criminals through stops and searches. Chief among them was the widespread use of consent searches, which gave officers an avenue to investigate motorists based on only vague suspicions of wrongdoings. That these suspicions often turn out to be incorrect when compared to probable cause searches is not surprising. What we document is that these searches are so inefficient that their contribution to public safety (insofar as we can measure the concept) appears to be negligible. Yet consent searches continue to be a common element of police work, heavily engaged in by agencies across the country. If the goal is to reduce the potential for harmful contact between officers and citizens without undermining efforts at crime abatement, then scaling back or eliminating consent searches appears to be low-hanging fruit for workable police reform.

BACKGROUND

Consent searches in modern policing

Violent crime rates rose steadily from the 1960s to the early 1990s (Enns, 2016). In response, and with a public that was generally supportive of harsher penalties for crime, politicians from the left and right remade the criminal justice system (Murakawa, 2014). Police work was retooled around *broken windows theories of crime*, where everyday interactions with officers became an opportunity to identify and intervene in drug related and potentially violent activity (Epp et al., 2014).

The logic underlying policing strategies that spring from broken windows is that law enforcement can deter crime through a visible and active presence; and can preempt crime by intervening in low level, minor infractions before they escalate to more serious offenses (Corman & Mocan, 2005; Michener, 2013). Proactive practices like hot-spot policing rely on data to direct law enforcement activities, where Feeley and Simon (1992) characterize the contemporary approach to policing, and criminal justice writ large, when they write: "It pursues systemic rationality and efficiency. It seeks to sort and classify, to separate the less from the more dangerous, and to deploy control strategies rationally. The tools for this enterprise are "indicators," prediction tables, population projections, and the like," and they go on to note the human implications, writing, "in these methods, individualized diagnosis and response is displaced by aggregate classification systems for purposes of surveillance, confinement, and control," (p. 452). Scholars elsewhere write that the wide-spread adoption of these kinds of practices, "represent a general shift from a culture of investigating crime to investigating individuals who are believed prone to commit crime" (Rios et al., 2020, p. 58).

Consent searches are a key example of the kind of strategy that develops from broken windows. Law enforcement leverage seemingly innocuous infractions, like the proto-typical driving with a broken tail light or in a car with tinted windows, to then escalate the stop to a search in order to recover contraband. Moreover, the development of consent searches can be characterized as a constitutional accommodation of preemptive policing as a strategy. As part of the shift in policing and in pursuit of the war on drugs, police departments, together with the US Drug Enforcement Agency, developed criminal profiles and used them to inform decisions about who should be investigated for potential criminal wrongdoing. Yet, to adequately investigate an individual for criminal behavior officers need legal authority to conduct a search. Traditionally, police searches must be conducted on the basis of probable cause—a phrase that comes directly from the 4th Amendment, and constrains police interactions with civilians by requiring some threshold of evidence for intervention. Starting with *Terry v. Ohio* (1968), the Supreme Court hollowed out that constitutional constraint to accommodate the frequent police-initiated contacts called for by a broken windows style of policing. With the 1968 decision, the Court lowered the threshold of evidence necessary to search individuals from probable cause to reasonable suspicion (Baumgartner et al., 2018). Further, with *Schneckloth v. Bustamonte* (1973), officers may conduct searches without probable cause and without making individuals aware of their rights if they otherwise obtain consent. Finally, in *Whren v. United States* (1996) the Court upheld targeting specific types of drivers for traffic stops, reasoning that the selective enforcement of traffic laws is an inevitable part of police work (Alexander, 2012).

Together, these rulings make it possible for law enforcement to stop and search virtually any car or pedestrian so long as they obtain consent. During the

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1990s, police training evolved to take advantage of this legal leeway, emphasizing that a successful patrol was an active one during which an officer would make many stops and then strategically manipulate drivers into acquiescing to a search (Remsberg, 1995). A number of studies have examined the officer characteristics associated with a higher propensity to conduct discretionary searches, finding that white, male officers conduct higher rates of searches than their female or Black counterparts (Baumgartner et al., 2021; Close & Mason, 2007; Hong, 2017; Shoub et al., 2021). Officers with fewer years of service are also more likely to conduct such searches (Baumgartner et al., 2021). Few studies have sought to examine how department- or municipal-level characteristics affect these patterns, however. Rather, this new policing strategy is understood to have spread widely to departments throughout the United States.[1] Consent searches, and high-contact policing strategies more generally, are known to be concentrated in minority neighborhoods. We also know that highway patrol units tend to conduct fewer searches of any kind and instead concentrate predominately on writing speeding tickets (Baumgartner et al., 2018). Routine interactions with civilians and the relative ease with which consent searches could be conducted thus formed a cornerstone of the contemporary provision of public safety.

The costs and benefits of preemptive policing practices

Little is known about the efficacy of consent searches, particularly relative to probable cause searches, with respect to the violent crime they are designed to thwart. In 2012, officers from Fayetteville, North Carolina dramatically scaled back their use of consent searches without this leading to a corresponding increase in crime (Epp & Erhardt, 2021). Researchers have also given some effort to evaluating the consequences of related practices like stop, question and frisk (SQF), and other kinds of order maintenance strategies for public safety, but findings are decidedly mixed. While early studies suggested that an active police presence was associated with declining rates of robbery, more recent work suggests this relationship is attenuated by contextual factors (Cohen et al., 2003; Kane & Cronin, 2013; Sampson & Cohen, 1988).

New York City, faced with rising violent crime in the 1970s and 1980s, embraced broken windows policing strategies, and has thus been the site of a good deal of scholarly attention. While some research links broken windows policing to declining violent crime in the 1990s (e.g., Kelling & Sousa, 2001), other scholarship finds that such strategies modestly impacted non-violent and

[1]In the Appendix, we take a preliminary look at how patterns in the use of consent searches by police department correlate with municipal factors having to do with population, race, and poverty. No clear patterns emerge, which is consistent with consent searches being a routine part of policing that are used by all types of police agencies.

property crime, but had little impact on violent crime (Corman & Mocan, 2005; Kane, 2006; Rosenfeld et al., 2007). Scholars likewise highlight that economic conditions rose and violent crime diminished not only in New York, but in cities across the country (Eck & Maguire, 2000; Harcourt & Ludwig, 2006). Focusing in on SQF, the pedestrian iteration of consent searches, yields equally confused results, where some have linked SQF to declining crime (Rosenfeld & Fornango, 2017; Weisburd et al., 2016; Wooditch & Weisburd, 2016), and others find no such relationship (Ferrandino, 2018).

Moreover, experimental evidence suggests that, to the extent broken windows policing effectively deters crime, this is achieved through problem-oriented strategies specific to the needs of a given neighborhood (Braga et al., 1999; Braga & Bond, 2008). Hot spot policing strategies, where law enforcement are deployed to high crime areas (again in New York City), were associated with declining crime, but researchers attribute this to the effectiveness of probable cause searches and note the ineffectiveness of SQF at recovering contraband (Rosenfeld & Fornango, 2017; Shoub, 2021).

Researchers also highlight that consent searches are deployed in a racially discriminatory manner (Baumgartner et al., 2018; Epp et al., 2014; Rosenfeld & Fornango, 2017). The data-driven and preemptive turn in modern policing means communities are saturated with police presence, but not all communities and individuals are equally likely to be subjected to consent searches (Fagan & Geller, 2015). Instead, communities and individuals that fit profiles perceptibly indicative of a higher propensity to commit crime are disproportionately subject to surveillance, and such indicators are bound up with race, ethnicity and class (Alexander, 2011; Gelman et al., 2007; Stoudt et al., 2011; Tonry, 2011; Travis et al., 2014). Consequently, researchers have found that Black Americans are 2.7 times more likely to be subjected to an investigatory stop than are their white counterparts, and individuals driving low-value vehicles (the most obvious marker of class) are 70% more likely to be subjected to such a stop than drivers of high value vehicles (Epp et al., 2014).

Yet, even as Latinx and Black Americans are subject to invasive searches at higher rates than are their white counterparts, such searches are more effective at recovering contraband among white civilians (Baumgartner et al., 2018; Rosenfeld & Fornango, 2017). Recent findings using Texas Highway Patrol data show that state troopers could search racial groups at the same rate and increase contraband recovery, suggesting that the higher rates of search experienced by Black and Hispanic motorists are not being driven by any efficiency-maximization strategy (Feigenberg & Miller, 2021). The race of the officer matters here too. White officers search Black civilians at higher rates, and to recover less contraband per search than do their Black counterparts (Baumgartner et al., 2021; Close & Mason, 2007; Hong, 2017). Generally, conducting more discretionary searches is associated with lower rates of contraband recovery (Abrams et al., 2022).

Even as the public safety benefits of intrusive policing strategies are unclear, the costs of these practices are many. Preemptive policing heightens the risk of contact with the criminal legal system for individuals living in communities inundated with law enforcement, in turn increasing the risk that such encounters will turn violent, that individuals involved will be arrested even if not convicted, and that such contact will mark individuals for further criminal legal involvement (Murakawa & Beckett, 2010; Starr, 2014). These negative consequences spill over to impact the children, family and surrounding community of those targeted for contact (Burch, 2013; Lee et al., 2015; Walker, 2020; White, 2018).

Excessive and disproportionate policing practices likewise incur a democratic cost. Such practices actively constitute civic belonging and govern access to substantive citizenship (Loader, 2006). Repeated, involuntary and seemingly unfounded interactions with officers, however innocuous or congenial the encounter, erodes the legitimacy of police, which scholars broadly accept as a key problem facing law enforcement (Epp et al., 2014; Justice & Meares, 2014; Meares et al., 2015). Excessive policing undermines civic trust and political efficacy, leading individuals to approach other kinds of institutions with skepticism and to withdraw from political life (Burch, 2011; Lerman & Weaver, 2014a; Lerman & Weaver, 2014b; Weaver et al., 2020).

EXPECTATIONS

To examine the way consent searches are used, we introduce the following hypotheses:

> **H1.** Consent searches are less effective at finding contraband than are probable cause searches.

A justification for consent searches is that they are easy for officers to deploy, allowing a flexible avenue to investigate potential wrongdoings even when there is not clear evidence of criminal activity. It is therefore fair to say that consent searches are less likely to recover contraband than are probable cause searches by design. This hypothesis should not be surprising, and should we find evidence to support it, that evidence would indicate that consent searches are being used as intended.

Related to this hypothesis, H1a is that consent searches will be especially inefficient when performed on Black motorists. Previous research suggests that Black civilians are subject to discretionary searches at higher rates than are white ones. We also know that discretionary searches of Black civilians less frequently yields contraband than do searches of white civilians, suggesting that the factors officers use to determine reasonable suspicion are not very good at identifying criminal behavior, and are racially biased (Baumgartner et al., 2018; Rosenfeld & Fornango, 2017).

> **H1a.** Relative to probable cause searches, consent searches will be less effective at recovering contraband from Black civilians than from white civilians.

The use of consent searches developed from the logic that frequent police contact with civilians can ameliorate violent crime, thus enhancing public safety. Following this logic, our second hypothesis is that departments that rely more heavily on consent searches will also conduct more total discretionary searches. That is, we expect a hypothetical department where officers make only consent searches would be conducting substantially more searches than a department where officers use only probable cause. Opportunities to conduct consent searches are much more plentiful and departments that embrace a high-contact policing strategy will naturally be conducting more total searches. Our second hypothesis is as follows:

> **H2.** Police departments with a greater reliance on consent searches relative to probable cause will conduct more total discretionary searches.

H1 and H2 describe the basic mechanics of consent searches, which are thought to be part of a blunt, high-contact form of policing. But if H2 is true, it may be that, even as they are relatively inefficient at identifying contraband, departments that heavily rely on consent searches are able to arrest more criminals and confiscate more contraband through sheer force of numbers. Insofar as searching is usually a prerequisite for finding contraband and making arrests it stands to reason that doing more of one would lead to more of the other.

However, we think this is unlikely because prior research on discretionary police searches suggests that they are highly inefficient at locating contraband. This means that officers from a department relying heavily on a high-contact, consent search policing strategy could easily end up making more searches, but still finding less or similar amounts of contraband than a department that relies more on probable cause searches. If and how often this happens is an empirical question, which we are well-suited to answer given the cross-departmental breadth of our data. Our remaining hypotheses explore this question, positing that on balance relying heavily on consent searches will not lead to more contraband, more arrests, or less crime:

> **H3.** Police departments with a greater reliance on consent searches relative to probable cause will not make more arrests.

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H4. Police departments with a greater reliance on consent searches relative to probable cause will not find more contraband.

H5. A greater reliance on consent searches relative to probable cause will not be associated with less crime.

The substantive significance of these hypotheses is contingent on the answer to H2. If a higher reliance on consent searches is predictive of greater contact with civilians, then we would want to know if this contact is bearing fruit given that the social costs of invasive policing practices are well documented, and increasingly, so are the political costs. These costs are not trivial, particularly given that they are disproportionately born by race-class subjugated communities. As there is strong evidence that routine and unnecessary stops and searches have negative externalities for the people and neighborhoods subject to them, understanding their contributions to public safety is important so that political and community leaders can make informed decisions.

STUDY 1: CROSS-AGENCY OBSERVATIONAL APPROACH

Data and design

Study 1 draws on Stanford Open Policing Project data (SOPP).[2] SOPP aims to assemble traffic stop data from every police agency with public records. Altogether, over 200 million records are available from dozens of agencies. However, many of these records are not suitable for our study, as we require knowing, (1) whether a search took place pursuant to a stop, (2) what type of search it was, and (3) whether contraband was recovered. These parameters are met for 25 agencies, 18 in NC.[3] Outside NC, we have data for the police agencies serving San Diego and Oakland in California, Austin and San Antonio in Texas, and the State Highway Patrols for Texas, Wisconsin, and Colorado. Clearly, this is not a random sample of police agencies, but it does include agencies from different regions, left- and right-leaning states, and demographically diverse municipalities. Moreover, it is comprehensive in that we make use of all

[2]See: https://openpolicing.stanford.edu/data/.

[3]Data are available for every police department in NC but we limit our study to cities with at least 50,000 residents as of the 2010 Census. Police agencies for many small municipalities do not conduct enough searches to make reliable statistical estimates on a monthly basis. NC is a model of transparency when it comes to reporting police data as the state legislature has mandated since 2002 that virtually every traffic stop is documented and reported to the state Department of Justice. These data are being collected on a continuous basis, forming a nearly comprehensive record of traffic stops in the state over the last 20 years. They have been used in dozens of peer-reviewed publications on policing.

TABLE 1 Stop information for all agencies included in the analysis.

Department	State	Years	Total searches	Total consent	% consent
Asheville	NC	2002–2015	6039	4349	0.72
Austin	TX	2006–2016	16,433	3113	0.19
Burlington	NC	2002–2015	3607	2057	0.57
Cary	NC	2002–2015	2257	1383	0.61
Chapel Hill	NC	2002–2015	1470	697	0.47
Charlotte-Mecklenburg	NC	2002–2015	121,596	89,760	0.74
Colorado State Patrol	CO	2010–2017	8289	3285	0.40
Concord	NC	2002–2015	3453	2660	0.77
Durham	NC	2002–2015	26,620	18,388	0.69
Fayetteville	NC	2002–2015	36,824	21,134	0.57
Gastonia	NC	2002–2015	3632	2599	0.72
Greensboro	NC	2002–2015	41,506	31,682	0.76
Greenville	NC	2002–2012	2343	1995	0.85
High Point	NC	2002–2015	7941	5035	0.63
Jacksonville	NC	2002–2015	5085	3546	0.70
NC State Patrol	NC	2000–2015	13,385	6730	0.50
Oakland	CA	2016–2017	5443	336	0.06
Raleigh	NC	2002–2015	36,598	20,490	0.56
Rocky Mount	NC	2002–2015	3112	2089	0.67
San Antonio	TX	2012–2018	4933	3766	0.76
Sandiego	CA	2014–2017	5125	2813	0.55
Texas State Patrol	TX	2006–2009	176,041	128,290	0.73
Wilmington	NC	2002–2015	4904	3677	0.75
Winston-Salem	NC	2002–2015	9216	4752	0.52
Wisconsin State Patrol	WI	2010–2016	9545	2635	0.28

of the publicly available traffic stop data that is relevant to our research question.

Table 1 provides descriptive information for all agencies under study, including the time period where we have data, the total number of discretionary stop-and-searches, and the percent of searches that were consent. Discretionary searches are those carried out either with the driver's consent or with probable cause. They are discretionary in that the decision to initiate a search stems from an officer's judgment during a stop. This makes them interesting as a window into police decision-making. Non-discretionary searches are those called for by the police protocols governing a particular situation. For example, when

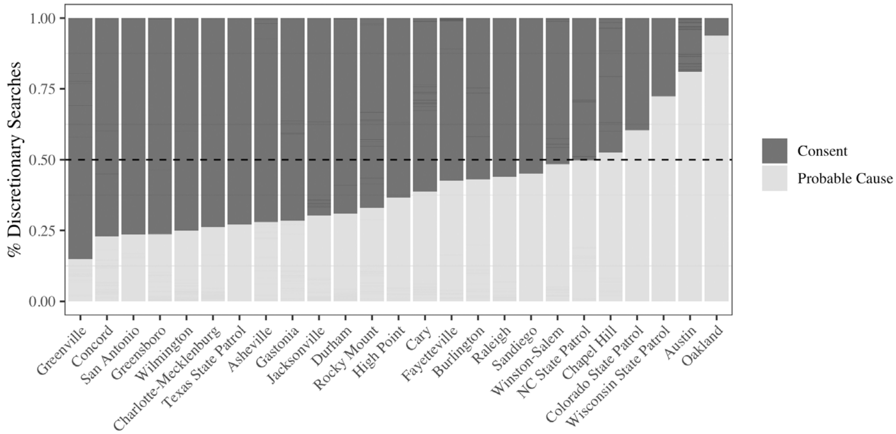

FIGURE 1 Reliance on consent searches relative to probable cause searches across departments.

arresting a drunk driver, officers are meant to conduct a "protective frisk" to make sure the person being taken into custody is not carrying weapons. Likewise, officers sometimes have to exercise search warrants.

Figure 1 displays the percentage of total discretionary searches that are either consent or probable cause for each agency. This makes clear that in the effort to locate contraband, consent searches are a major element of modern policing. For every agency except Oakland and Austin, consent searches make up at least 25% of discretionary searches. For 20/25 agencies, consent searches are more than half of the total.

To evaluate the hypothesis that consent searches will be less likely to recover contraband than probable cause searches (H1), we employ separate logistic regressions predicting the likelihood of finding contraband during a discretionary search for each of the 25 departments. We also provide a random effects meta-analytic estimate that offers a single, weighted average of the estimates for all departments.[4] Because our data are observational, we control for other relevant factors that may be related either to the likelihood of recovering contraband or to the decision to conduct a search. Our ability to control for these factors is constrained by the data that each department makes available, however, and varies by department. For every department, we control for the age, gender, and race of the driver stopped. For departments and state patrols in California, North Carolina, Texas, and Wisconsin, we control for the day of the week a stop was conducted. In California and North Carolina, we are able to control for the various reasons officers stopped a driver. Reasons include

[4]Implemented via the meta package in R using the Hartung–Knapp method.

suspected parole violations, intoxicated driving, and speeding. The Wisconsin State Highway Patrol publicly reports the age and make of the vehicles they stop. We include both of these variables as controls in our analysis of this department. To test H1a, we re-estimate these models separately for white and Black motorists using logistic regressions for each police department. Tests evaluating H1 and H1a are performed on stop-level data.

To test whether departments that rely heavily on consent searches make more searches overall (H2), we collapse the dataset to the agency-month level, summing the total number of monthly discretionary searches to use as the dependent variable in a pooled Poisson regression model with fixed effects for agency-years. We use to same approach to answer H3, H4, and H5, estimating separate Poisson models that use the raw monthly count of arrests, contraband hits, and instances of violent crime as dependent variables.

RESULTS

H1 posits greater reliance on consent relative to probable cause searches will be associated with a lower contraband recovery rate. We evaluate this hypothesis for the 25 agencies we collected data on. To render the logistic regression estimates legible, we derive the difference in the predicted probability of contraband recovery via consent searches minus the predicted probability of contraband recovery via probable cause searches.

Figure 2 shows that the difference in likelihood of recovering contraband between probable cause and consent searches across all departments is stark, even when controlling for other relevant factors. In all but three departments, officers are more likely to find contraband during a probable cause than a consent search. This result is statistically significant for these 22 departments. The magnitude of the likelihood varies by department. For most, officers are between 20 and 40 percentage points (pp) more likely to find contraband in a probable cause search than in a consent search. That likelihood goes up to 50 pp for the Colorado State Patrol. The meta-analytic estimate suggests, on average, consent searches are 27 pp less likely to recover contraband than probable cause searches. There are three departments that depart from this pattern. There is no statistically meaningful difference between reliance on probable cause and consent searches in San Diego and Austin. Only in Gastonia did reliance on consent searches increase the likelihood of recovering contraband.

In sum, in terms of contraband recovery, across 22 jurisdictions that vary by geography, political affiliation, and demographic diversity, consent searches are worse at recovering contraband. This finding holds even after adjusting for several potentially relevant factors, and it holds true in departments for which we employed a full set of controls, and for departments for which we were only able to control for factors such as driver race, gender, and age.

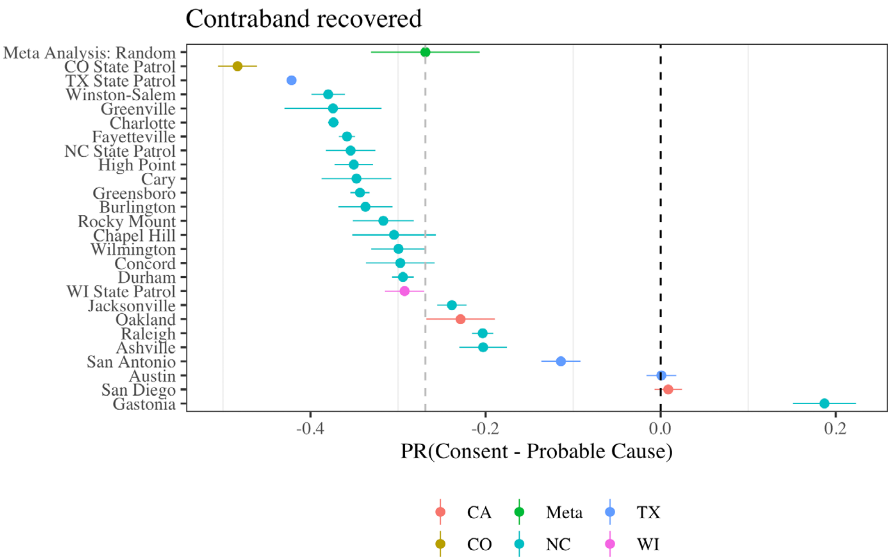

FIGURE 2 The difference in the probability of finding contraband between probable cause and consent searches across agencies in the United States. [Color figure can be viewed at wileyonlinelibrary.com]

H1a concerns the disparate impact of policing on civilians of color, and especially, Black civilians. We examine the efficiency of consent searches when the civilian stopped is white and when the civilian stopped is Black. Figure 3 displays the effectiveness of consent searches relative to probable cause searches in yielding contraband, revealing that consent searches are especially inefficient when the civilian being searched is Black relative to when the civilian is white. The meta-analytic estimates suggest the effect of relying on consent searches relative to probable cause searches is twice the size when Black civilians are being searched than when white civilians are being searched. That is, probable cause searches are 17 pp more likely to recover contraband among Black civilians than are consent searches, while they are only 8 pp more likely to do so when whites are being searched.

Consistent with our hypotheses, consent searches are much less likely to locate contraband than searches made with probable cause and this is especially true for Black motorists. Yet, consent searches were never advertised as efficient. Their purported benefits are in their flexibility, allowing officers to stop and investigate more drivers than if probable cause was required in each case. Thus, H2 posits that a greater reliance on consent searches will be associated with more searches overall.

Results are displayed in Table 2. Recall, these analyses are conducted at the department-month level using Poisson models. Column one provides support

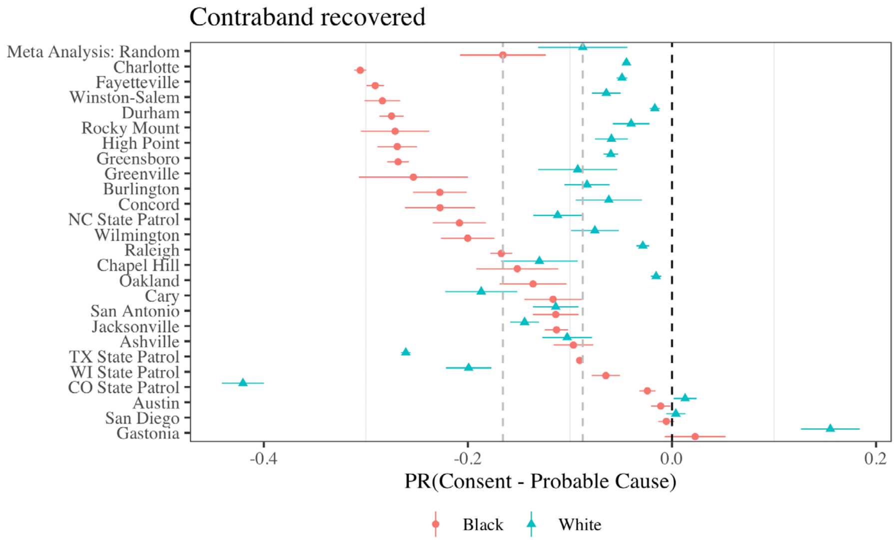

FIGURE 3 The difference in the probability of finding contraband between consent and probable searches across cities in the United States, by race of civilian searched. [Color figure can be viewed at wileyonlinelibrary.com]

TABLE 2 The impact of reliance on consent searches on policing outcomes.

	Searches (H2)	Arrests (H3)	Contraband (H4)	Violent crime (H5)
Percent consent	15.876	−2.153	−2.725	6.697
	(8.100)	(1.818)	(2.627)	(4.920)
Agency-year fixed effects	Yes	Yes	Yes	Yes
Observations	2815	2815	2815	2815
R^2	0.001	0.001	0.0004	0.001

$^*p < 0.05$;

for H2: for every 1 percentage point increase in the proportion of discretionary searches that are based on driver consent, the average police department conducts almost 16 more searches per month (although, this relationship only approaches significance with a *p*-value of 0.05009). In substantive terms this effect is large. The Oakland Police Department, which uses consent searches in only 5% of discretionary searches, is predicted to carry out 1248 fewer searches per month than the Greenville, NC Police Department, which uses consent searches in 83% of discretionary searches.

As expected, consent searches are less likely to result in contraband, are racially disparate, and are part of a high-contact policing strategy. What, if any, are their benefits to public safety? Do departments that make proportionally more consent searches also make more arrests, find more contraband, or have lower levels of violent crime? Answers to these questions are found in columns two to four of Table 2. In each case, the coefficient for consent searches is not significant, and thus the answer appears to be *no*. Departments that are more reliant on consent searches conduct more searches, but do not make more arrests, do not find more contraband, and do not experience less crime than departments relying more heavily on probable cause to conduct searches.

Searching is a prerequisite for finding contraband (and often for making an arrest for a non-driving criminal violation) so by searching more departments might naturally be expected to be doing more of these things as well. That this is not the case highlights the downsides of a high-contact policing strategy based on only vague suspicions of wrongdoing, at least insofar as it comes at the expense of legally constrained, evidenced-based police work. However, these analyses are descriptive. Police departments may not easily be able to substitute searches based on consent for fewer searches based on probable cause. With these concerns in mind, we turn to our second study.

STUDY 2: OPERATION STRONG SAFETY

Study 1's advantage is that it allows us to assess consent search patterns across several agencies. The downside is that the estimates we present may be subject to bias via endogenous driver behavior. For example, consent searches may be justified based on an officer's expertise to sense driver wrongdoing. Yet, drivers typically subject to consent searches may adjust driving habits by diligently hiding or driving without contraband. Another endogenous behavior is that drivers with less to hide may be inclined to say yes to consent searches. These endogenous behaviors may generate a difference in contraband recovery between consent and probable cause searches that is not the result of inherent inefficiencies in consent searches, but instead reflects the deterrent effect of consent searches and related preemptive practices in addition to the propensity for non-offending drivers to say yes to consent searches.

We address this possibility through a design-based approach in Study 2. Our approach leverages daily-level traffic stop data from the Texas Department of Public Safety (DPS) Highway Patrol to evaluate the plausibly exogenous effect of *Operation Strong Safety* (OSS) on the contraband recovery rate. OSS was a policy that suddenly increased the consent search rate in two predominantly Mexican-American border counties, allowing us to estimate the immediate effect of shifting toward using consent searches at a point in time where drivers may have had limited ability to adjust to the shift in police tactics and where the distribution of drivers who say yes to consent searches is likely constant. Study 2 complements Study 1 by providing internal validity on the consequences of privileging consent searches in policing tactics, while Study 1 provides external validity by testing our hypotheses in multiple departments.

Context

OSS was implemented by the Texas Governor and DPS on June 23rd, 2014. OSS redirected highway patrol resources to Hidalgo and Starr county from the rest of Texas for the stated goal of combatting human smuggling and drug trafficking along the border during the 2014 Central American child migrant crisis (DPS, 2015a).[5] OSS specifically increased patrol activity near Highway 83, which cuts through several border towns in Hidalgo and Starr (DPS, 2015b). Importantly, the policy was announced only 2 days before implementation, minimizing anticipatory effects on the part of drivers and the highway patrol (Aguilar, 2014). Additionally, there was extremely limited news coverage of the operation's development before June 2014, further suggesting drivers did not anticipate the policy. Using LexisNexis, we gather data on the count of articles related to "operation strong safety" and "rick perry," a proponent of OSS, at the monthly level during 2014. There were only 2 news articles related to OSS while there were 300 articles related to Rick Perry on May 2014 (Figure C24). Additionally, there was no detectable Google Search activity related to "operation strong safety" in the Harlington–Welasco–Brownsville–McAllen metropolitan area, which contains the area of operations for OSS, further suggesting limited anticipatory effects (Figure C25). Although the DPS rejects the notion OSS was focused on stopping unauthorized immigration, journalistic accounts suggest DPS officers were also directed to engage in indiscriminate traffic stops to identify and detain undocumented migrants (Bosque, 2018).

Government officials raised concerns OSS was increasing the number and rate of unnecessary traffic stops throughout Hidalgo and Starr (Aguilar, 2014). Charis Kubrin, a UC-Irvine criminologist who analyzed DPS data, suggested the high numbers of traffic stops suggests the DPS might be profiling Mexican-Americans in border communities, noting, "I see a parallel in the New York stop-and-frisk policy, which was ruled unconstitutional and a complete failure" (Schladen, 2016). Indeed, OSS led to a dramatic increase in traffic stops throughout Hidalgo/Starr. Post-OSS, the number of daily stops increased by 343, 135% of the pre-treatment mean (254), without a commensurate shift throughout the rest of Texas (Figure C3). This is equivalent to an increase from three to seven stops *per day* per 10,000 Hidalgo/Starr residents. The number of

[5]DPS was not ready to fully implement the policy. To meet operational demand in Hidalgo/Starr, the DPS reduced patrols and troopers in other parts of Texas (Nelsen, 2016).

DPS troopers present in Hidalgo/Starr on a given day increased from 42 to 158 post-OSS (Figure C4). Consistent with journalistic accounts, there is evidence the precipitous increase in stops was unwarranted. The warning rate discontinuously increased post-OSS, suggesting traffic stops imposed by OSS were based on weak legal justifications (Aguilar, 2014) (Figure C5).

OSS also encouraged a shift toward using consent searches. The number of daily searches discontinuously increased by eight in Hidalgo/Starr post-OSS, 160% of the pre-OSS daily search average (five) (Figure C7). At the same time, OSS imposed policing tactics that *privileged the use of consent searches throughout Hidalgo and Starr*. Figure 4, Panel A displays the monthly consent search rate between January 2009 and December 2016.[6] Pre-OSS, the average consent search rate hovers around 50%. Immediately after, the consent search rate discontinuously increases to 80%. Figure 4, Panel B displays the monthly weapon, drug, and money contraband recovery rate for Hidalgo and Starr. Consistent with Study 1, as DPS suddenly shifts to a strategy increasingly reliant on consent searches, the contraband recovery rate discontinuously decreases from 40 to 15 percentage points. Descriptively, this offers support for our main hypothesis. We now turn to an evaluation of OSS using a regression discontinuity-in-time (RDiT) approach and daily DPS stop data.

Data and design

To test our hypotheses, we use SOPP data on traffic stop-and-searches from the Texas DPS highway patrol in Hidalgo and Starr counties from January 1, 2009 to December 31, 2016 ($N = 16{,}203$). We hone in on Hidalgo and Starr given these two counties were the OSS area of operations (Figure C1). We assess the effect of OSS on two outcomes. The first indicates whether a stop-and-search was reported as a consent search. The second indicates whether a stop-and-search led to contraband recovery. The consent search outcome helps establish that OSS led to a greater reliance on consent searches. The contraband recovery outcome helps us test H1. Unfortunately, we cannot test H1a with the DPS Hidalgo and Starr stop-and-search data given that 93% of stop-and-searches throughout Hidalgo and Starr were of Latinos/Hispanics, and there is limited data to make a daily-level comparison with whites on policing outcomes.

To assess the effect of OSS on the probability a stop-and-search is a consent search and produces contraband, we use an RDiT approach which derives the discontinuous effect of OSS on consent and contraband recovery rates on the day OSS was implemented:

[6]Search data are missing for 2017.

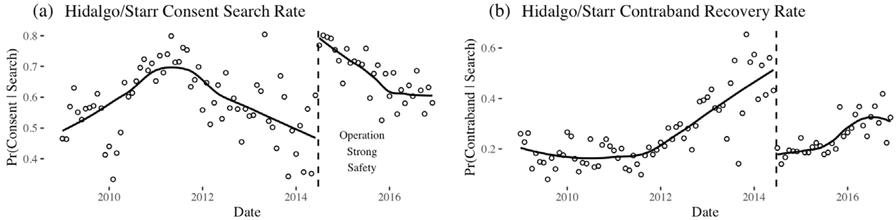

FIGURE 4 Consent search and contraband recovery rate (y-axis, Panels A and B) over time (x-axis, both panels) in Hidalgo and Starr counties. Each dot is a monthly average of the rate outcomes. The dashed vertical line is the moment OSS is implemented (2014-06-23). The solid line is a loess fit on each side of the time OSS is implemented.

$$Y_i = \alpha + \tau OSS_i + f_j(d_i) + \varepsilon_i. \tag{1}$$

For Equation (1), Y_i is an indicator of whether stop-and-search i was either a consent search or resulted in contraband recovery. α is the intercept. OSS_i is an indicator if stop i occurs post-OSS (June 23rd, 2014). $f_j(d_i)$ are functions modeling the running variable, days from OSS implementation (d_i), at different polynomial degrees, j. j is from degree = 0–3. For brevity, we only present findings in the main text where degree, j, is equal to 1. ε_i are heteroskedastic robust errors. Our expectation is that OSS will increase the consent search rate (τ = positive) while simultaneously decreasing the contraband recovery rate (τ = negative). We display two sets of RDiT estimates in the main text. The first uses all stop-and-search data, adjusting for year, month, and day-of-week fixed effects to account for outcome seasonality in addition to a lagged dependent variable (Hausman & Rapson, 2018). The second set of estimates uses data from narrow bandwidths before and after OSS implementation (10–100 days) (Imbens & Lemieux, 2008). Our approach is similar to Mummolo (2018), who leverages daily New York stop-and-frisk variation to assess the effects of shifts in police tactics.

We choose to use a regression discontinuity-in-time relative to other approaches (e.g. a difference-in-differences approach assessing the differential effect of OSS on Hidalgo and Starr relative to other Texas counties) for several reasons. First, the design is less susceptible to long-term unobservable differential time trends that may correspond with the timing of OSS and the outcomes of interest in Hidalgo and Starr counties. Although secular time trends may affect the outcomes of interest independent of OSS, these trends are likely constant at the discontinuity characterizing the immediate day after OSS implementation versus the immediate day. Indeed, we conduct an event study assessing the differential effect of OSS on consent search and contraband recovery rates in Hidalgo and Starr counties relative to other Texas counties using a county/

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year panel.[7] We find evidence of parallel pre-trend violations (Figures C22 and C23), suggesting secular differential time trends may bias estimates derived from an alternative design. Second, other approaches assessing longer-term effects may be susceptible to bias in the form of endogenous driver behavior to hide contraband in response to increased policing. The regression discontinuity-in-time design circumvents this problem given the immediate and relatively unanticipated nature of the policy.

RESULTS

Figure 5 characterizes the effect of OSS on consent searches (Panel A) and contraband recovery (Panel B) throughout Hidalgo/Starr. OSS suddenly increases the consent search rate by 19 pp in Hidalgo/Starr ($p < 0.001$), 30% of the pre-treatment mean, 63%. Findings using the full dataset are corroborated using data at narrow temporal bandwidths less likely to be driven by seasonal trends and model-dependent assumptions (see Panel A). Commensurately, OSS suddenly decreased the contraband recovery rate throughout Hidalgo and Starr by 10 percentage points ($p < 0.001$), 50% of the pre-treatment mean, 19%. These results demonstrate sudden shifts in policing tactics that privilege consent searches decrease police efficiency via the recovery of contraband. Likewise, OSS did not substantially increase the raw amount of contraband recovery. OSS only increased raw contraband recovery by one per day in Hidalgo/Starr. Yet, the surge in DPS officers in Hidalgo/Starr came at the expense of patrols elsewhere, leading to a daily decrease of eight raw contrabands throughout the rest of Texas, a net decline in raw contraband recovery of seven (Table C2).

To further assess our hypotheses around the public safety benefits of consent searches, we assess whether OSS reduced crime in Hidalgo/Starr using FBI Uniform Crime Report data[8] on total, violent, and property crime rates each year between 2000 and 2017 in Hidalgo/Starr and the rest of Texas.[9] Descriptively, total and property crime rates appear to be decreasing in Hidalgo/Starr *prior to OSS* (Figure C16A,B), suggesting OSS did not uniquely precipitate crime reductions. Additionally, although OSS appears to have forestalled an increasing trend in violent crime rates, violent crime rates were also generally decreasing prior to OSS and it appears that Hidalgo/Starr's violent crime rates did not decrease as precipitously as they did in the rest of Texas between 2013 and 2014 (Figure C16C), the moment of OSS's implementation.

[7]There are 240 out of 254 Texas counties that have stop-and-search data for each year between 2009 and 2016 (including Hidalgo and Starr). These counties constitute our sample.

[8]UCR Data are often incomplete at the departmental-level, but Texas state law requires all departments report crime.

[9]Rates are number of crimes divided by the county population for a given year multiplied by 10,000.

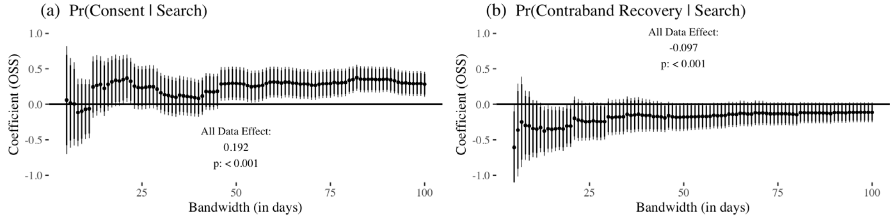

FIGURE 5 Effect of OSS on consent search (Panel A) and contraband recovery rate (Panel B) throughout Hidalgo and Starr using stop-and-search data at small bandwidths near the discontinuity. Annotations denote discontinuous effect of OSS using the full stop-and-search data adjusting for year, month, and day-of-week fixed effects in addition to a lagged dependent variable. The running variable (days to OSS) polynomial degree for all estimates is equal to 1. 95% CIs displayed using robust SEs.

We formally test the effect of OSS on the crime rate categories in Hidalgo/Starr relative to a re-weighted set of other Texas counties using a generalized synthetic control approach developed by Xu (2017) to ensure parallel pre-treatment outcome trends. The OSS average treatment effect over the treated is statistically null for total, violent, and property crime rates. However, the sign of the effects are negative, the size of the effects are relatively large, and the effects are noisy ($\beta = -78$, $p = 0.14$; $\beta = -74$, $p = 0.13$; and $\beta = -4$, $p = 0.24$ for total, property, and violent crimes respectively, see Table C5). The event study estimates provide more statistical context for understanding these negative, large, yet statistically insignificant effects. Consistent with the descriptive patterns, total and property crime rates appear to be on a decreasing trend in the pre-treatment period starting in 2012 (Figure C17A,B). Thus, the negative post-OSS trend may be unrelated to OSS. Moreover, the effect of OSS on violent crime rates only appears in 2016 and 2017, 2 years after OSS's implementation (Figure C17C). Thus, either OSS had a long-term negative effect on violent crime or long-term unobserved differential trends are driving the long-term effect. Given OSS amassed significant police resources within a short period of time (Figures C3 and C4), it is unclear why there are not large short-term effects post-OSS in 2014 and 2015. Therefore, it may not be sensible to attribute these long term negative trends to OSS, especially since policing intensity decreased substantially in July 2015 after the DPS became embroiled in scandal associated with Sandra Bland's murder (Figures C3 and C4). Overall, based on the FBI UCR data, we conclude the evidence on the effect of OSS on crime is mixed/ambiguous and suggests negative crime shifts are either driven by preexisting trends or long-term effects susceptible to unobserved differential time trends unrelated to OSS. We use an additional dataset to test the effect of OSS on crime. We collected official monthly index crime statistics from the McAllen Police Department between 2011 and 2020 (Figure C26A). McAllen is the

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largest city in Hidalgo and Starr counties, the area of operations for Operation Strong Safety. McAllen also touches the US–Mexican border and Highway 83, where many state troopers patrolled during OSS, cutting through the city. We use a regression discontinuity-in-time design to assess to discontinuous effect of OSS implementation on the monthly number of index crimes in McAllen, TX, and find OSS had no effect on crime (Figure C26B). We also assess the effect of OSS on index crimes using a simple difference-in-means approach comparing the number of index crimes pre-OSS to the number of index crimes post-OSS (Figure C26C). The naive difference-in-means model shows that OSS had a negative effect on crime. But, after adjusting for a monthly trend, OSS has a statistically null effect on crime, further suggesting OSS either had an ambiguous effect on crime or no effect at all.

Robustness checks

The findings are robust. One may be concerned the contraband recovery decrease is due to a *bundled treatment*. That is, OSS did not simply increase the consent search rate, but shifted other policing activities, namely, the intensity of policing. These other shifts may have affected contraband recovery rates. While OSS is admittedly a bundled treatment, we contend it is still an externally valid case to assess tactical shifts toward consent search reliance. *Tactical policing shifts do not occur in a vacuum*. Departments often do not, or cannot, shift highly specific tactical dimensions, but rather employ a bundle of interrelated tactics (Baumgartner et al., 2018). For instance, increasing consent search reliance may be bound up with generally weaker evidentiary standards for initializing a stop, resulting in higher levels of stops and searches. Indeed, Study 1 demonstrates the consent search rate is positively associated with higher search levels (Table 2, Column 1), suggesting consent search shifts go hand-in-hand with shifts in other policing activity dimensions. Therefore, evaluating tactical shifts toward consent search reliance *requires* an evaluation of multifaceted policing shifts.

Moreover, we provide evidence the bundled treatment may not mean the consent search rate increase post-OSS is not an operative mechanism explaining the contraband rate decrease. If the consent search rate increase post-OSS is not an operative explanation for the contraband rate decrease, then either the daily consent search rate should not be associated with the daily contraband rate net of adjusting for the intensity of policing (i.e. number of stops, number of searches, and number of officers) in Hidalgo/Starr, or daily policing intensity metrics should be consistently associated with the contraband rate. We demonstrate stop and search intensity is not consistently associated with the contraband recovery rate at the daily-level in Hidalgo/Starr between 2009 and 2016, pre-OSS, and post-OSS with the exception of stops pre-OSS (Table C7, Columns 1–3). Moreover, the number of daily-level officers is also not consistently associated with contraband recovery rates in Hidalgo/Starr pre-OSS and post-OSS (Table C7, Columns 4–6). In fact, inconsistent with bundled treatment concerns, the daily officer count is positively associated with contraband recovery in the full sample. Most importantly, the consent search rate is associated with contraband recovery rates regardless of adjusting for policing intensity. These findings suggest OSS, as a bundled treatment that not only shifted the consent search rate but the intensity of policing, decreased the contraband recovery rate in large part due to the increase in the consent search rate and not policing intensity.

We further rule out endogenous driver behavior by demonstrating OSS did not suddenly shift traffic crashes. If our effects are driven by drivers still able to become more cautious in hiding contraband despite OSS's unanticipated implementation, then we might expect drivers to also drive more carefully and avoid traffic crashes in the short-run in response to OSS. We estimate the effect of OSS on the daily number of traffic crashes using data from the Texas Transportation Department in Hidalgo and Starr during 2014. The majority of RDiT specifications demonstrate OSS had a statistically null effect on crashes (Table C8, Figure C20), increasing confidence our estimates are due to an inefficient shift toward consent searches instead of driver behavior.

We further rule out endogenous driver behavior by demonstrating most drivers likely did not perceive OSS near the moment the policy was implemented outside of encountering more police for a prolonged period of time. First, Google search intensity for "operation strong safety" was undetectable in the Harlington–Welasco–Brownsville–McAllen metropolitan area, which includes the area of operations for OSS (Figure C25). Therefore, it is unlikely the driving public in South Texas perceived the implementation of the policy. Second, LexisNexis newswire/press release/newspaper data suggests there were only 15 articles related to "operation strong safety" written on July 2014, the month the policy was implemented. Conversely, there were nearly 1100 articles related to "rick perry" on July 2014, the main proponent of OSS. The ratio of articles related to "operation strong safety" vis-a-vis "rick perry" is 0.02 (out of 1), suggesting the media space was oversaturated with information unrelated to OSS and that the driving mass public may have not perceived the implementation of the policy (Figure C24).

We provide additional evidence our findings are not driven by drivers who say "yes" to consent searches because they have less to hide. This problem only exists if a significant proportion of drivers say "no" to consent searches. *But nearly all drivers say "yes" to a consent search if asked.* Experimental evidence approximating a consent search scenario demonstrates 97% of individuals say "yes" to these intrusive requests. The experimental evidence was corroborated by LAPD traffic stop data in 2006, demonstrating 99.98% of individuals say yes to consent searches during vehicular stops (Sommers & Bohns, 2019). Our own

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evidence from LAPD stop data between January 2020 and October 2022. shows 96% of drivers asked for consent to search say "yes" (Figure C21A). Although the contraband recovery rate is 10 percentage points higher for searches of drivers who said "no" instead of "yes" to consent searches, this difference has a negligible impact on the overall contraband recovery rate of consent searches given refusals are so rare (Figure C21B). Although data are not available on consent search refusals for the Texas highway patrol, the coercive nature of consent searches suggests consent search refusals are rare and do not drive our findings.

Finally, OSS does not have a corresponding sudden effect on consent search or contraband recovery rates in Texas counties outside Hidalgo and Starr (Tables C3 and C4, Figures C9 and C10), suggesting our findings are not driven by secular trends in criminality or driving behavior across Texas. Related to the bundled treatment problem, we rule out whether our findings are driven by an influx of inexperienced officers who typically patrol outside Hidalgo and Starr post-OSS instead of tactical policing shifts. We subset our stop-and-search data to officers who initiated 90% of their stops inside Hidalgo/Starr pre-OSS. Even for officers experienced in policing Hidalgo/Starr, OSS discontinuously increases the consent search rate while depressing the contraband recovery rate (Table C6). Our conclusions do not change using different model specifications, running variable degrees, and bandwidths (Tables C3 and C4, Figures C9 and C10). Our results are the same when we use the Calonico et al. (2015) optimal bandwidth selection approach (Figure C8). We demonstrate the findings are not due to statistical chance by showing the OSS effect is often larger than placebo effects based on pre-treatment discontinuities (Figures C11–C14). We also use a "donut-hole" approach to rule out anticipatory effects by re-estimating the OSS effect excluding observations near the discontinuity most likely subject to anticipatory effects. Our conclusions do not change (Figure C15).

CONCLUSION

This article presents the results of the broadest study yet conducted on the efficiency of consent searches at locating contraband and the consequences of reliance on consent searches for public safety. Looking across 25 police agencies, we find that probable cause searches are more efficient at recovering contraband, that heavy reliance on consent searches amounts to more searches (i.e., more police–citizen contacts), but not to more contraband recovery overall, more arrests, or less crime. The findings derived from the observational analysis are remarkable insofar as they consistently hold across nearly all agencies under study. The size of the effect is likewise quite large, where probable cause searches increase the likelihood of recovering contraband by between 25% and 50%. Given this huge efficiency gap, the other findings make sense as a police department would have to conduct vastly more consent searches to find more contraband than a department relying predominately on probable cause to investigate drivers.

However, these findings are only correlational—we cannot say that reliance on consent searches causes a decrease in the efficiency of law enforcement activities. To address this shortcoming, we leverage a policy change in Texas in 2014, which temporarily flooded two counties along the Mexican border with highway patrol in order to deter human smuggling at the height of the child migrant crisis. This sudden and dramatic policy change affords us the opportunity to take a design-based approach to our questions of interest. The results of a regression discontinuity analysis affirm the findings from the observational data: the policy change led to an increase in officer activity and reliance on consent searches, a decrease in the overall rate in contraband recovery, limited impact on raw contraband recovered and limited impact on crime. Next steps in this research agenda should look to further understand the use of consent searches at the agency and officer levels. Potentially, certain types of police agencies or officers may be less likely to engage with this element of policing and understanding why could provide useful information on proliferation and consequences of consent searches.

Together, the observational and design-based approaches offer strong support for the claims that a reliance on consent searches often does not contribute to public safety, unnecessarily puts civilians and officers at risk, and has the downstream effect of further marginalizing already marginalized people. This paper complements a handful of studies evaluating the responsiveness of consent searches to efforts to reform their application. Mummolo (2018) finds that a requirement by the NYPD'd Chief of Patrol to not only document why a civilian was stopped but also produce a copy of the report for supervisors at the end of every shift lead officers to engage in more conservative practices, due to heightened scrutiny. Similarly, the Fayetteville, North Carolina Police Department adopted the policy of requiring drivers to sign a consent form prior to the execution of a consent search likewise lead to an immediate and dramatic decline in their usage, with commensurate rise in crime (Epp & Erhardt, 2021).

These studies demonstrate that practices related to the deployment of consent searches are easy to change even at the departmental level. Moreover, further research finds that declining use of consent searches was associated with declining assaults on officers (Boehme, 2023). Thus, relying on consent searches less holds the promise of improving the experiences of civilians (especially civilians of color), saving departments time and resources, and improving officer safety, all without sacrificing public safety. It is also the case that this is the kind of reform that should reduce contact between citizens and law enforcement overall—a primary objective of those calling for progressive police reforms. Finally, at the center of our inquiry are concerns over the impact of policing on

American democracy. Democracy requires that law enforcement act in ways that uphold civil and human rights. Constitutional constraints embedded in the Fourth Amendment and imposed on probable cause searches are designed to ensure this balance, and to protect citizens from unnecessary intrusion from the enforcement arm of the state. Consent searches upset that balance with limited practical yield, and reforming their use is both practical and high-impact.

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None.

DATA AVAILABILITY STATEMENT

Data necessary to replicate the results of this article are available upon request from the corresponding author.

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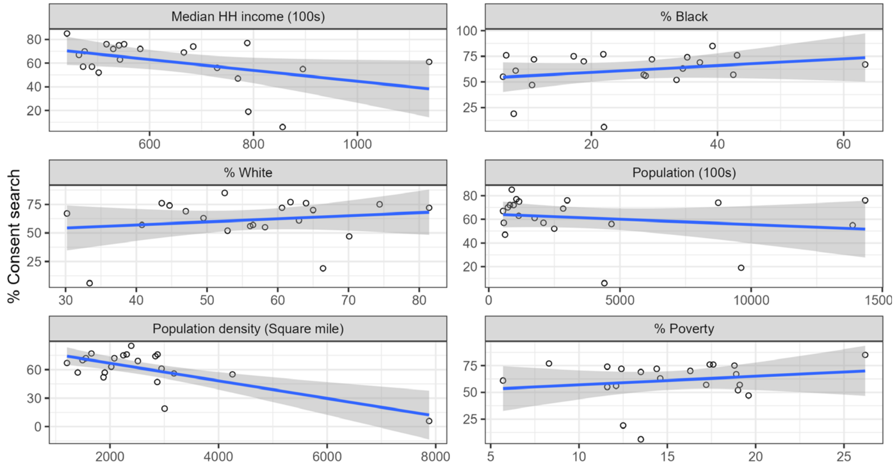
APPENDIX A

CONSENT SEARCHES AND MUNICIPAL CHARACTERISTICS



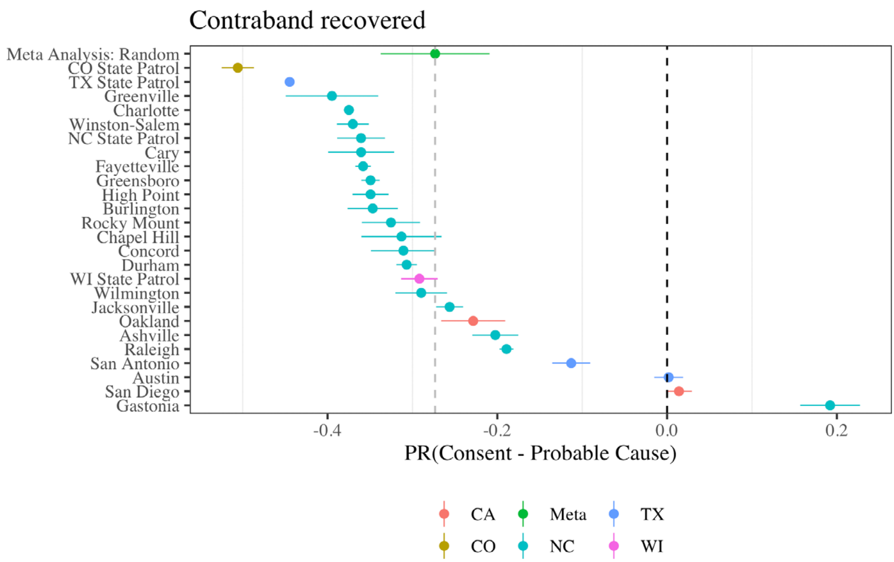
The figure shows correlations between a department's use of consent searches (see Figure 1) and municipal-level characteristics available from the US Census. For example, pairing the Austin Police Department with data on the city of Austin, TX. State patrols are therefore not included in this analysis. None of the correlations are statistically meaningful. We believe there are two reasons for this: First, consent searches are commonly used across all types of police departments and policing strategies are idiosyncratic. Officers from the Oakland Police Department rarely use consent searches but this likely has less to do with attributes of Oakland and more to do with changes in policing strategy brought about by a confluence of local factors that are hard measure. Second, it is possible that meaningful patterns would emerge at the neighborhood level. Plausibly, impoverished or minority neighborhoods within these cities see disproportionately more consent searches but we lack the stop-level geographic data to investigate.

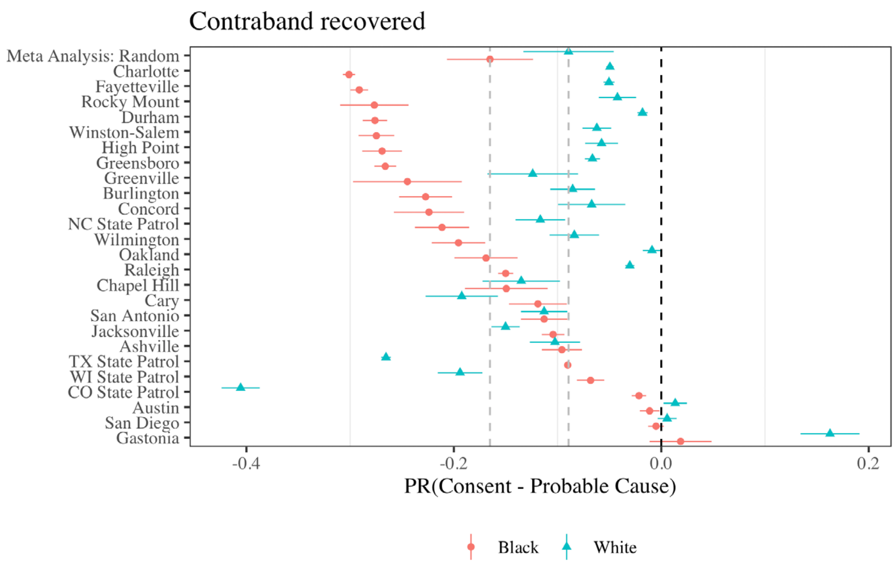
APPENDIX B

CONSENT SEARCHES AND CONTRABAND RECOVERY

Some might wonder about the relationship between reliance on consent searches and contraband recovery, in the absence of other covariates that officers might use to determine whether or not to search and individual, such as age, gender and race of driver. The figure displays the relationship between reliance on consent searches and contraband recovery among all civilians stopped.

The figure displays the same relationship, but among subsets of white and Black drivers stopped. The patterns observed and reported on in the main manuscript remain unchanged.





APPENDIX C

OSS ANALYSIS

OSS area of operations

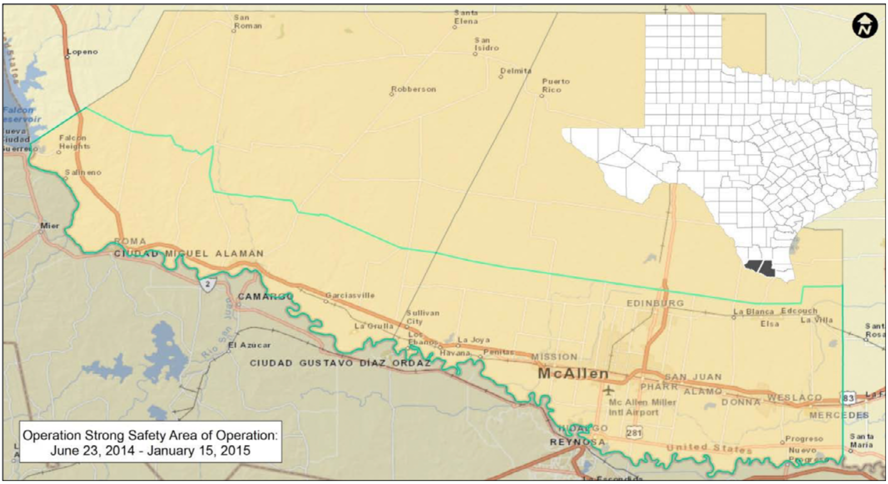


FIGURE C1 DPS operation strong safety area of operations. *Source*: Department of Public Safety. [Color figure can be viewed at wileyonlinelibrary.com]

Spending over time

See Figure C2.

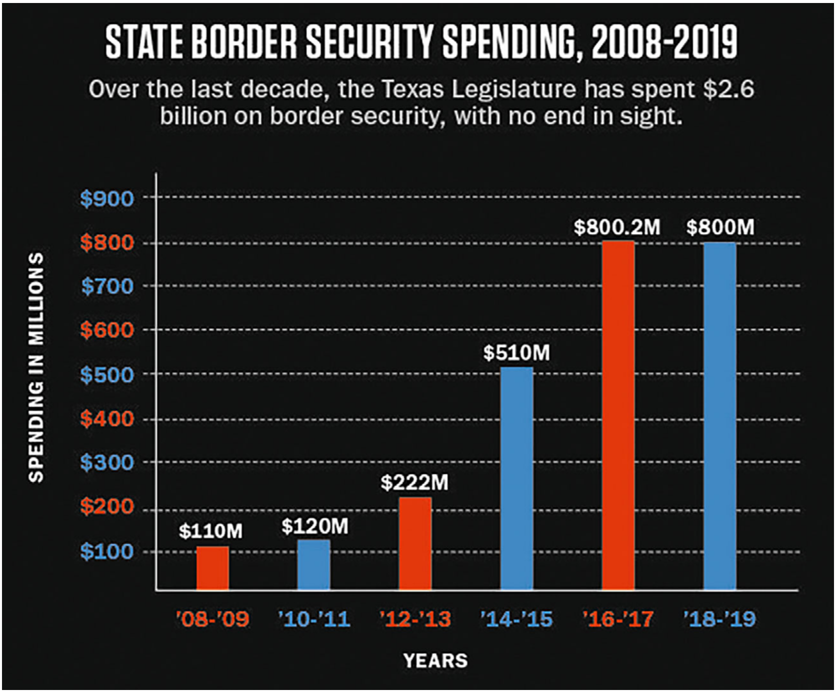


FIGURE C2 Texas State Legislature spending (in millions, y-axis) on border security over time (x-axis) (2008–2019) *Source*: Texas Observer. [Color figure can be viewed at wileyonlinelibrary.com]

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Stops over time

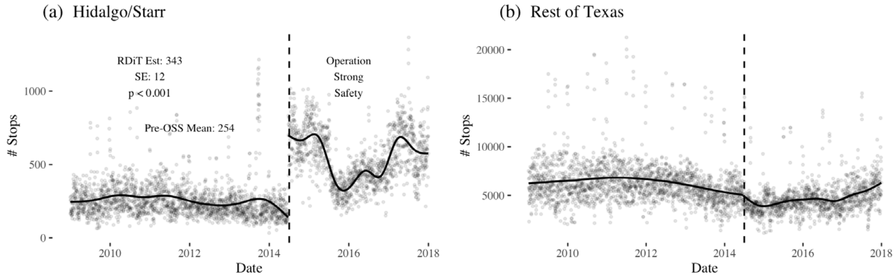

FIGURE C3 The count of traffic stops (y-axis) over time (x-axis) in Hidalgo/Starr (Panel A) and the rest of Texas (Panel B). Solid black lines are loess fits on each side of the moment operation strong safety (OSS) was implemented. Dashed vertical line is the day OSS was implemented. Annotations denote pre-OSS mean in Hidalgo/Starr county along with a regression discontinuity-in-time estimate characterizing the discontinuous effect of OSS on the count of traffic stops in Hidalgo/Starr (polynomial = 1, uniform kernel).

Officers over time

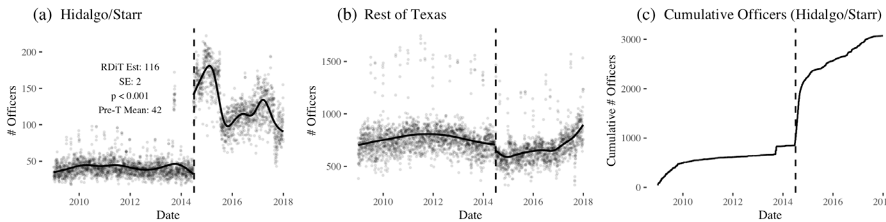

FIGURE C4 The count of officers (y-axis) over time (x-axis) in Hidalgo/Starr (Panel A) and the rest of Texas (Panel B). Solid black lines are loess fits on each side of the moment operation strong safety (OSS) was implemented. Dashed vertical line is the day OSS was implemented. Annotations denote pre-OSS mean in Hidalgo/Starr county along with a regression discontinuity-in-time estimate characterizing the discontinuous effect of OSS on the number of officers in Hidalgo/Starr (polynomial = 1, uniform kernel). Panel C displays the cumulative number of officers operating in Hidalgo/Starr counties over time.

Warning rate over time

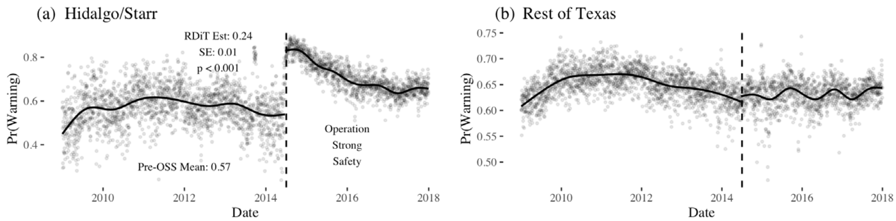

FIGURE C5 Warning rate (y-axis) over time (x-axis) in Hidalgo/Starr (Panel A) and the rest of Texas (Panel B). Solid black lines are loess fits on each side of the moment operation strong safety (OSS) was implemented. Dashed vertical line is the day OSS was implemented. Annotations denote pre-OSS mean in Hidalgo/Starr county along with a regression discontinuity-in-time estimate characterizing the discontinuous effect of OSS on the traffic stop warning rate in Hidalgo/Starr (polynomial = 1, uniform kernel).

License violations over time

See Figure C6.

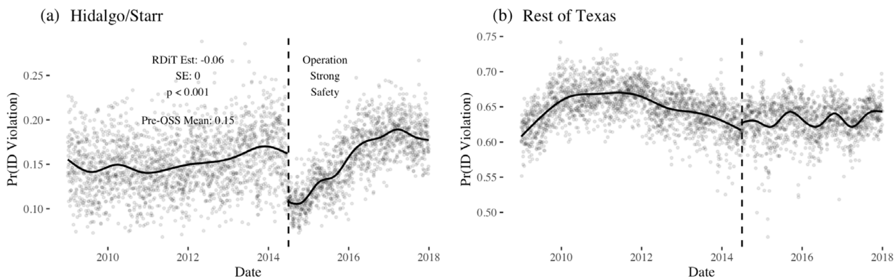

FIGURE C6 Driver's license violation rate (y-axis) over time (x-axis) in Hidalgo/Starr (Panel A) and the rest of Texas (Panel B). Solid black lines are loess fits on each side of the moment operation strong safety (OSS) was implemented. Dashed vertical line is the day OSS was implemented. Annotations denote pre-OSS mean in Hidalgo/Starr county along with a regression discontinuity-in-time estimate characterizing the discontinuous effect of OSS on the driver's license violation rate in Hidalgo/Starr (polynomial = 1, uniform kernel).

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Searches over time

FIGURE C7 Traffic searches (y-axis) over time (x-axis) in Hidalgo/Starr (Panel A) and the rest of Texas (Panel B). Solid black lines are loess fits on each side of the moment operation strong safety (OSS) was implemented. Dashed vertical line is the day OSS was implemented. Annotations denote pre-OSS mean in Hidalgo/Starr county along with a regression discontinuity-in-time estimate characterizing the discontinuous effect of OSS on the number of searches in Hidalgo/Starr (polynomial = 1, uniform kernel).

Optimal bandwidth estimates

Alternative specifications under CCT optimal bandwidth framework

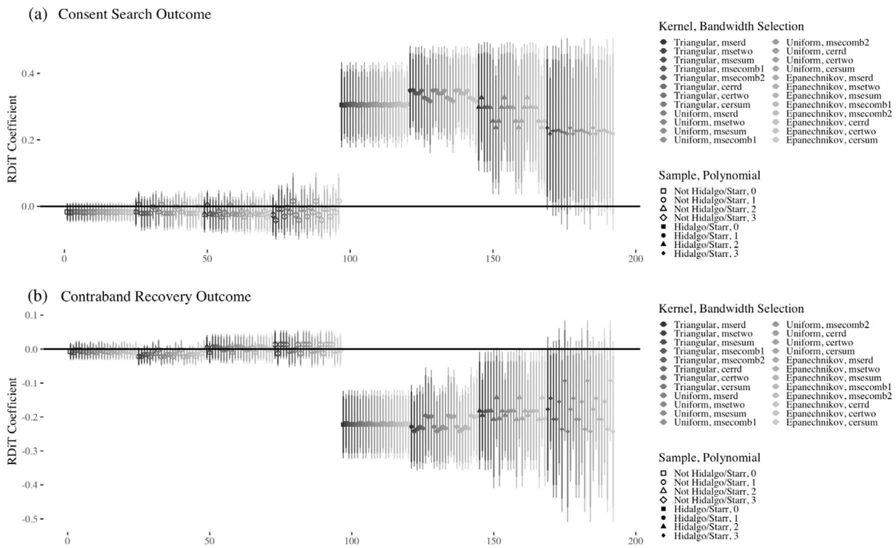

FIGURE C8 RDiT estimates characterizing effect of OSS on consent searches (Panel A) and contraband recovery rates (Panel B) throughout Hidalgo/Starr (coefficient estimates on the right) and the rest of Texas (coefficient estimates on the left). RDiT estimates displayed include permutations of sample (Hidalgo/Starr, rest of Texas), running variable polynomial (0, 1, 2, and 3), kernel (triangular, uniform, Epanechnikov), and bandwidth selection mechanism (mserd, msetwo, msesum, msecomb1, msecomb2, cerrd, certwo, cersum) implemented via rdrobust in R (see Calonico et al. (2015)). 95% CIs derived using default nearest neighbor ($n = 3$) robust SEs.

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Full sample

Search counts

TABLE C1 RDiT effect of OSS on searches by geographic region using full sample.

# searches								
Panel A: Non-HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	−51.03***	−42.35***	−14.29**	−21.67***	−10.51***	−11.24***	−7.60*	−9.58**
	(1.39)	(2.56)	(4.19)	(3.60)	(2.43)	(2.48)	(3.24)	(2.84)
N	2922	2922	2922	2922	2921	2921	2921	2921
R^2	0.24	0.29	0.33	0.33	0.70	0.70	0.70	0.70
Panel B: HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	2.00***	7.82***	13.57***	12.41***	5.38***	5.62***	7.47***	6.95***
	(0.18)	(0.39)	(0.60)	(0.53)	(0.50)	(0.50)	(0.69)	(0.61)
N	2922	2922	2922	2922	2921	2921	2921	2921
R^2	0.04	0.15	0.22	0.23	0.42	0.42	0.43	0.43
Controls	N	N	N	N	Y	Y	Y	Y
Polynomial	0	1	2	3	0	1	2	3

Note: Panel A characterizes the effect of OSS on the number of searches outside of Hidalgo and Starr. Panel B characterizes the same effect but within Hidalgo and Starr counties. Models 1–4 do not include control covariates. Models 5–8 adjust for day of week, month, and year fixed effects in addition to a lagged dependent variable. Models 1–4 and Models 5–8 use 0, 1st, 2nd and 3rd order polynomials for the running variable, respectively. HC2 robust SEs in parentheses.

***$p < 0.001$;

**$p < 0.01$;

*$p < 0.05$.

See Table C1.

Contraband recovery counts

TABLE C2 RDiT effect of OSS on contraband recovery count by geographic region using full sample.

# contraband recovered								
Panel A: Non-HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	−4.97***	−17.37***	−12.40***	−14.36***	−7.52***	−7.51***	−5.97***	−7.02***
	(0.63)	(1.23)	(2.11)	(1.81)	(1.38)	(1.41)	(1.83)	(1.61)
N	2922	2922	2922	2922	2921	2921	2921	2921
R^2	0.02	0.06	0.07	0.07	0.47	0.47	0.47	0.47
Panel B: HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	0.52***	0.72***	1.65***	1.42***	1.15***	1.19***	1.71***	1.52***
	(0.06)	(0.13)	(0.20)	(0.18)	(0.16)	(0.17)	(0.23)	(0.20)
N	2922	2922	2922	2922	2921	2921	2921	2921
R^2	0.00	0.01	0.02	0.02	0.05	0.05	0.05	0.05
Controls	N	N	N	N	Y	Y	Y	Y
Polynomial	0	1	2	3	0	1	2	3

Note: Panel A characterizes the effect of OSS on the number of hits outside of Hidalgo and Starr. Panel B characterizes the same effect but within Hidalgo and Starr counties. Models 1–4 do not include control covariates. Models 5–8 adjust for day of week, month, and year fixed effects in addition to a lagged dependent variable. Models 1–4 and Models 5–8 use 0, 1st, 2nd and 3rd order polynomials for the running variable, respectively. HC2 robust SEs in parentheses.

***$p < 0.001$;

**$p < 0.01$;

*$p < 0.05$.

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Consent searches

TABLE C3 RDiT effect of OSS on consent search rate by geographic region using full sample.

Pr(consent search)								
Panel A: Non-HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	−0.15***	−0.02***	0.02**	0.01*	−0.02**	−0.02**	−0.01	−0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	303,608	303,608	303,608	303,608	303,462	303,462	303,462	303,462
R^2	0.02	0.02	0.03	0.03	0.04	0.04	0.04	0.04
Panel B: HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	0.06***	0.08***	0.27***	0.23***	0.18***	0.19***	0.23***	0.22***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
N	16,199	16,199	16,199	16,199	16,195	16,195	16,195	16,195
R^2	0.00	0.01	0.02	0.02	0.05	0.05	0.05	0.05
Controls	N	N	N	N	Y	Y	Y	Y
Polynomial	0	1	2	3	0	1	2	3

Note: Panel A characterizes the effect of OSS on the probability a search is a consent search for counties outside of Hidalgo and Starr. Panel B characterizes the same effect but within Hidalgo and Starr counties. Models 1–4 do not include control covariates. Models 5–8 adjust for day of week, month, and year fixed effects in addition to a lagged dependent variable. Models 1–4 and Models 5–8 use 0, 1st, 2nd and 3rd order polynomials for the running variable, respectively. HC2 robust SEs in parentheses.

***$p < 0.001$;
**$p < 0.01$;
*$p < 0.05$.

Contraband recovery rates

TABLE C4 RDiT effect of OSS on contraband recovery rates by geographic region using full sample.

Pr(contraband recovery)								
Panel A: Non-HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	0.11***	−0.01**	−0.03***	−0.03***	−0.01	−0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	303,637	303,637	303,637	303,637	303,491	303,491	303,491	303,491
R^2	0.01	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Panel B: HS	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	0.02***	−0.07***	−0.07***	−0.07***	−0.10***	−0.10***	−0.10***	−0.10***
	(0.00)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
N	16,203	16,203	16,203	16,203	16,199	16,199	16,199	16,199
R^2	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Controls	N	N	N	N	Y	Y	Y	Y
Polynomial	0	1	2	3	0	1	2	3

Note: Panel A characterizes the effect of OSS on the probability a search leads to the recovery of contraband outside of Hidalgo and Starr. Panel B characterizes the same effect but within Hidalgo and Starr counties. Models 1–4 do not include control covariates. Models 5–8 adjust for day of week, month, and year fixed effects in addition to a lagged dependent variable. Models 1–4 and Models 5–8 use 0, 1st, 2nd and 3rd order polynomials for the running variable, respectively. HC2 robust SEs in parentheses.
***$p < 0.001$;
**$p < 0.01$;
*$p < 0.05$.

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Truncated sample

Iterated estimates, consent search rate

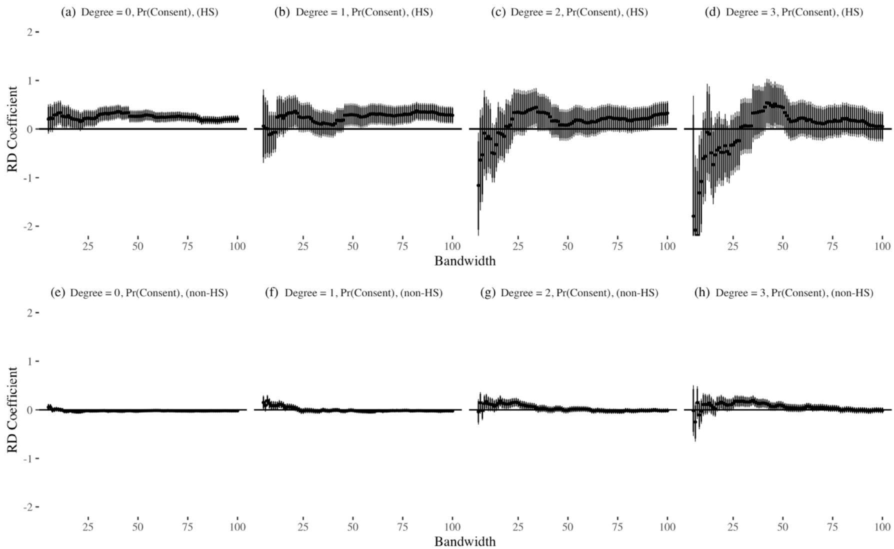

FIGURE C9 RDiT Effect of OSS on consent search rate using temporal bandwidths near the day OSS was implemented. Panels A–D denote estimates using data from Hidalgo and Starr county. Panels E–H denote estimates using data outside Hidalgo and Starr. 95% CIs displayed using robust standard errors.

Iterated estimates, contraband recovery rates

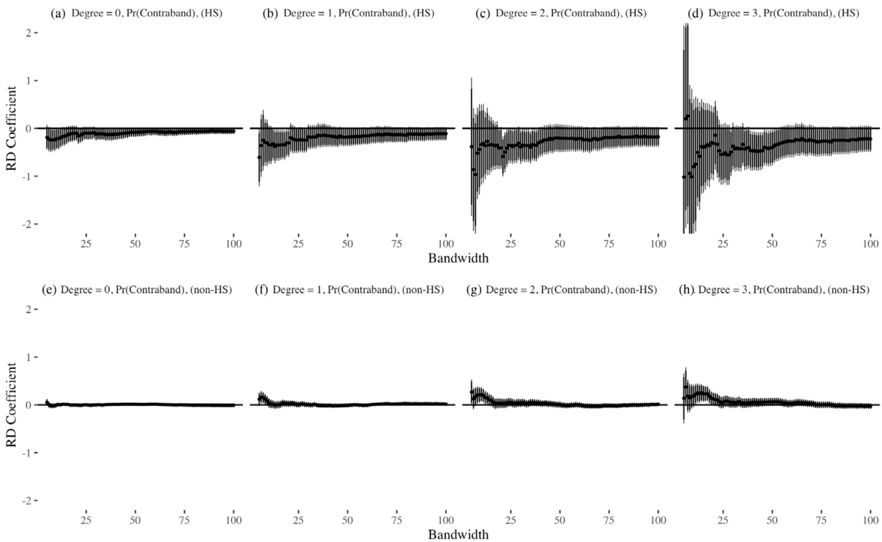

FIGURE C10 Effect of OSS on contraband recovery rates using temporal bandwidths near the day OSS was implemented. Panels A–D denote estimates using data from Hidalgo and Starr county. Panels E–H denote estimates using data outside Hidalgo and Starr. 95% CIs displayed using robust standard errors.

Temporal placebo tests

Full data

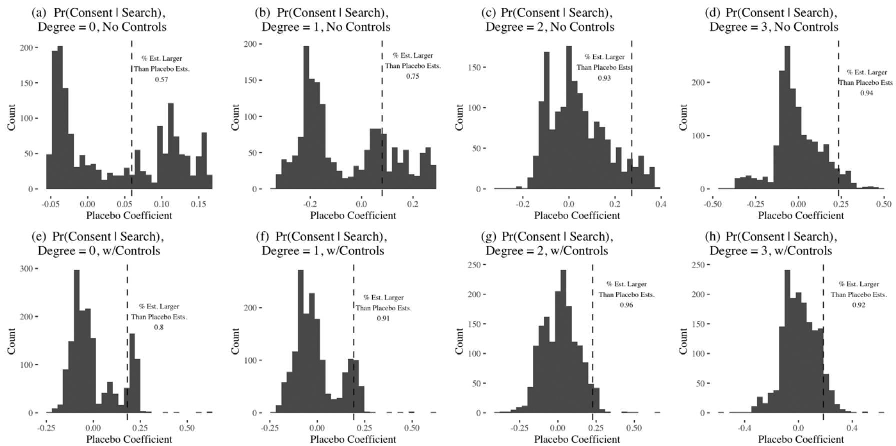

FIGURE C11 Comparing true effect of OSS on consent searches (dashed vertical line) with temporal placebo effects (x-axis) based on all potential discontinuities prior to OSS (discontinuities are at least 50 days prior to OSS or after the first day of the dataset, January 1, 2009) and using the full traffic stop-and-search data. Panels A–D do not include control covariates. Panels E–H include control covariates. Annotations denote the proportion of placebo estimates the true estimate is larger than.

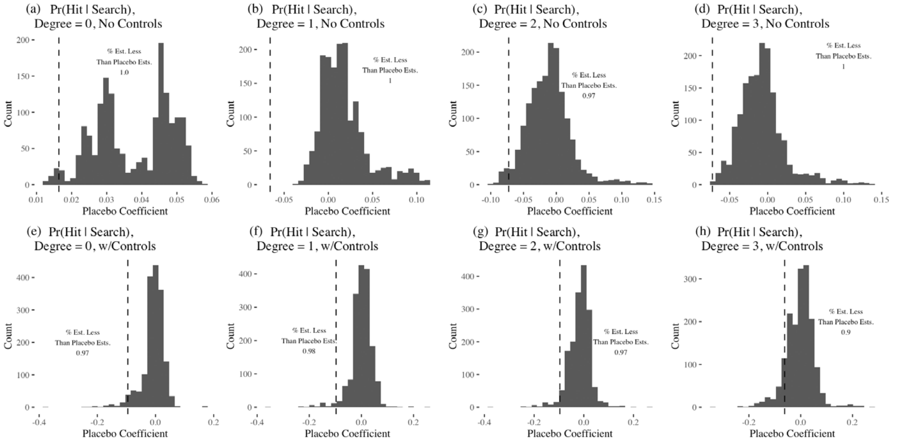

FIGURE C12 Comparing true effect of OSS on contraband recovery rates (dashed vertical line) with temporal placebo effects (x-axis) based on all potential discontinuities prior to OSS (discontinuities are at least 50 days prior to OSS or after the first day of the dataset, January 1, 2009) and using the full traffic stop-and-search data. Panels A–D do not include control covariates. Panels E–H include control covariates. Annotations denote the proportion of placebo estimates the true estimate is larger than.

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50 day bandwidth

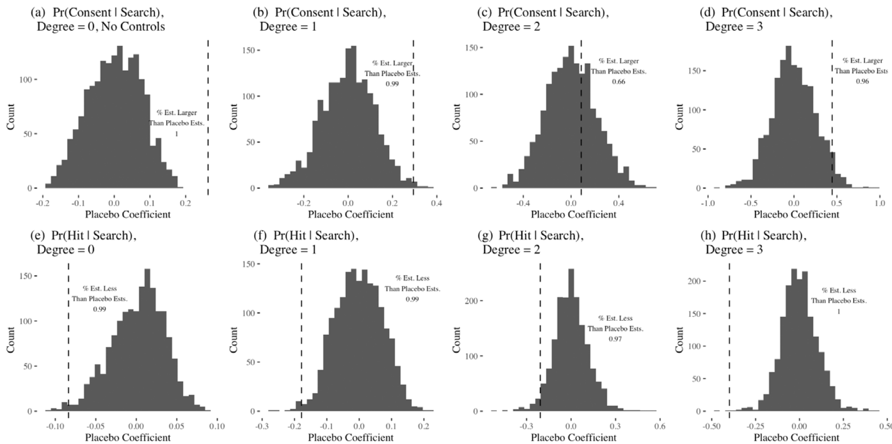

FIGURE C13 Comparing true effect of OSS on contraband recovery and consent search rates (dashed vertical line) with temporal placebo effects (x-axis) based on all potential discontinuities prior to operation strong safety (discontinuities are at least 50 days prior to OSS or after the first day of the dataset, January 1, 2009) and using the full traffic stop-and-search data (50-day bandwidth). Panels A–D do not include control covariates. Panels E–H include control covariates. Annotations denote the proportion of placebo estimates the true estimate is larger than.

25 day bandwidth

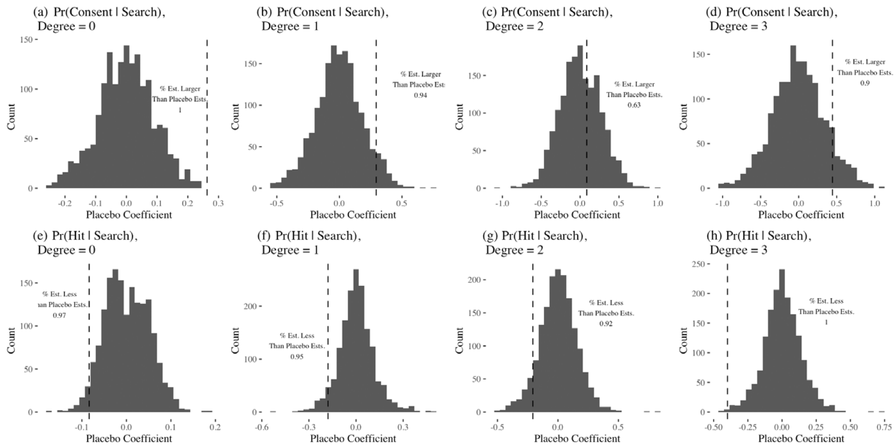

FIGURE C14 Comparing true effect of OSS on hit and consent search rates (dashed vertical line) with temporal placebo effects (x-axis) based on all potential discontinuities prior to operation strong safety (discontinuities are at least 50 days prior to OSS or after the first day of the dataset, January 1, 2009) and using the full traffic stop-and-search data (25-day bandwidth). Panels A–D do not include control covariates. Panels E–H include control covariates. Annotations denote the proportion of placebo estimates the true estimate is larger than.

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Donut RDiT estimates

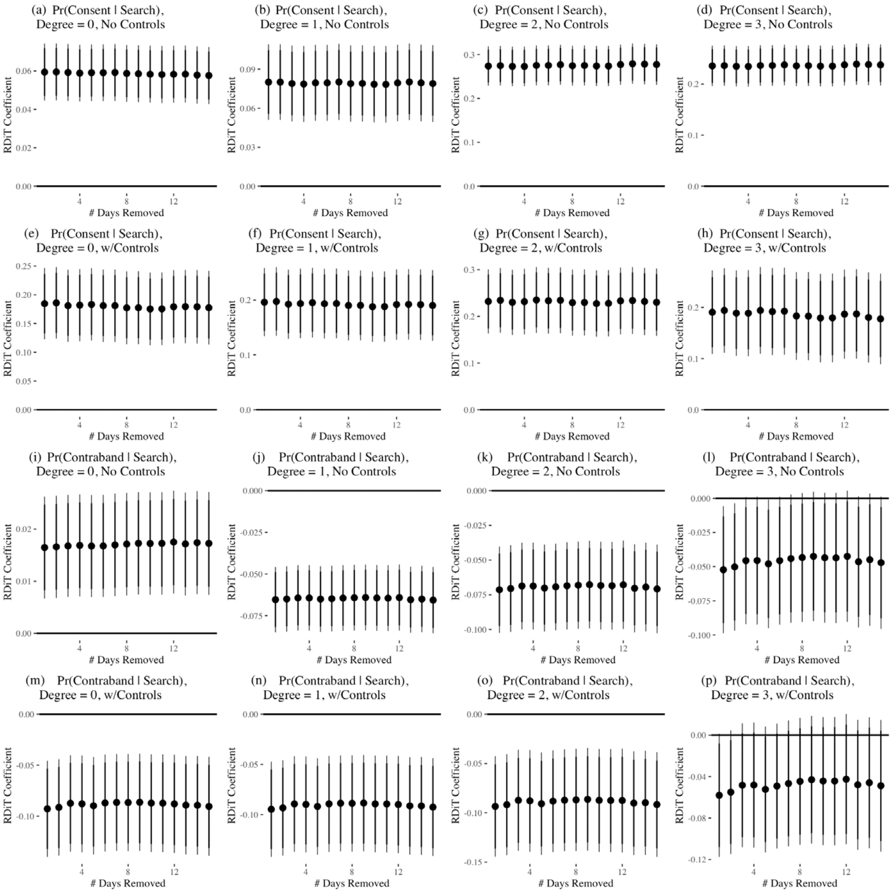

FIGURE C15 Donut hole RDiT estimates removing 1–15 days before and after OSS is implemented to rule out anticipatory effects. X-axis is the number of days removed, y-axis is the RDiT coefficient using the full stop-and-search data but removing days near the OSS discontinuity. Panels A–H are re-analyses using the consent search rate outcome. Panels I–P are re-analyses using the contraband recovery rate outcome. Panels A–D and I–L do not include control covariates. Panels E–H and M–P include control covariates.

Effects of OSS on crime

Descriptive statistics

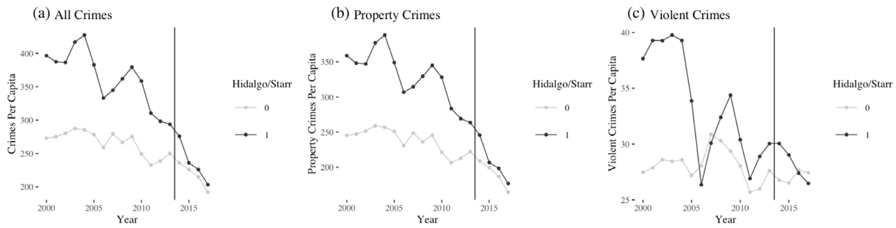

FIGURE C16 Average crime rates (y-axis) over time (x-axis, 2000–2017). Panels A–C display total, property, and violent crime rates over time (incidents divided by county population multiplied by 10,000 persons). Vertical line is the moment OSS is implemented. Line color denotes data from Hidalgo/Starr (black) and all other Texas counties (gray). Crime data are from the FBI Uniform Crime Report.

Synthetic control estimates

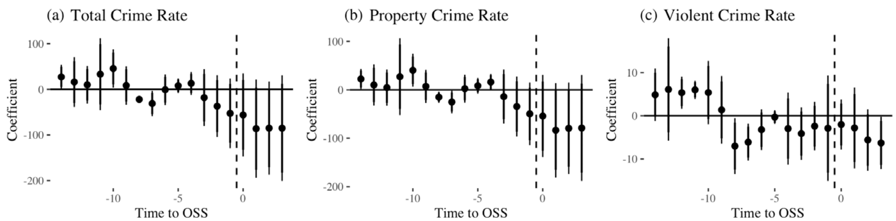

FIGURE C17 Event study estimates characterizing effect of operation strong safety on total (Panel A), violent (Panel B), and property (Panel C) crime rates in Hidalgo/Starr relative to a synthetic counterfactual.

TABLE C5 Effect of operation strong safety on crime rate (per 10,000 people).

	Total crime rate (1)	Property crime rate (2)	Violent crime rate (3)
OSS	−78.16	−74.01	−4.19
	(53.11)	(49.24)	(3.58)
N	4572	4572	4572

Note: Models 1, 2, and 3 characterizes ATT for OSS on all crimes, property crimes, and violent crimes per 10,000 residents in Hidalgo and Starr counties. SEs derived from parametric bootstrap procedure (1000 repetitions).

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Ruling out inexperienced officers alternative explanation

TABLE C6 RDiT effect of OSS on the consent search and contraband recovery rate among officers experienced in policing Hidalgo and Starr.

Pr(consent search)				
OSS	**0.07** (0.05)	**0.05** (0.05)	**0.22***** (0.06)	**0.23**** (0.07)
N	7632	7632	7632	7632
R^2	0.06	0.06	0.06	0.06
Pr(contraband recovery)				
OSS	−0.15*** (0.03)	−0.15*** (0.03)	−0.15*** (0.04)	−0.14** (0.05)
N	7632	7632	7632	7632
R^2	0.01	0.01	0.01	0.01
Controls	Y	Y	Y	Y
Polynomial	0	1	2	3

Note: 95% confidence intervals displayed using robust standard errors.
$^{***}p < 0.001$;
$^{**}p < 0.01$;
$^{*}p < 0.05$.

Accounting for bundled treatment

See Figure C18.

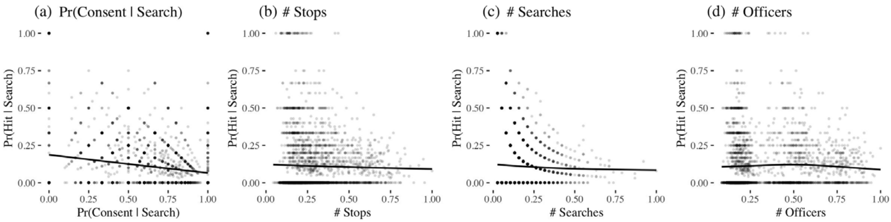

FIGURE C18 Association between dimensions of policing tactics (x-axis) and hit rates (y-axis) throughout Hidalgo and Starr counties. Panels A–D display the association between consent search rates, the number of stops, the number of searches, and the number of officers and hit rates. Fitted line is a loess model. Data are aggregated to the day-level.

TABLE C7 Association between dimensions of policing tactics and hit rates.

Contraband recovery rate						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Consent rate	−0.12***	−0.10***	−0.21***	−0.13***	−0.10***	−0.21***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
# Stops	0.02	−0.11**	−0.05			
	(0.02)	(0.04)	(0.04)			
# Searches	−0.03	0.03	0.02			
	(0.03)	(0.04)	(0.05)			
# Officers				0.04**	−0.02	−0.06
				(0.01)	(0.05)	(0.03)
Sample	Full	Pre-OSS	Post-OSS	Full	Pre-OSS	Post-OSS
R^2	0.04	0.03	0.10	0.04	0.03	0.10
N	2742	1833	909	2742	1833	909

Note: The outcome for each model is the contraband recovery rate. Consent Rate is the consent search rate conditional on a stop-and-search. # Stops is the number of stops per day. # Searches is the number of stop-and-searches per day. # Officers is the number of officers operating in Hidalgo and Starr per day. Data are aggregated to the daily-level and derived from the Hidalgo and Starr stop data. All covariates rescaled between 0 and 1. HC2 robust SEs in parentheses.

***$p < 0.001$;

**$p < 0.01$;

*$p < 0.05$.

Further demonstrating no endogenous driver behavior

Crashes over time

See Figure C19.

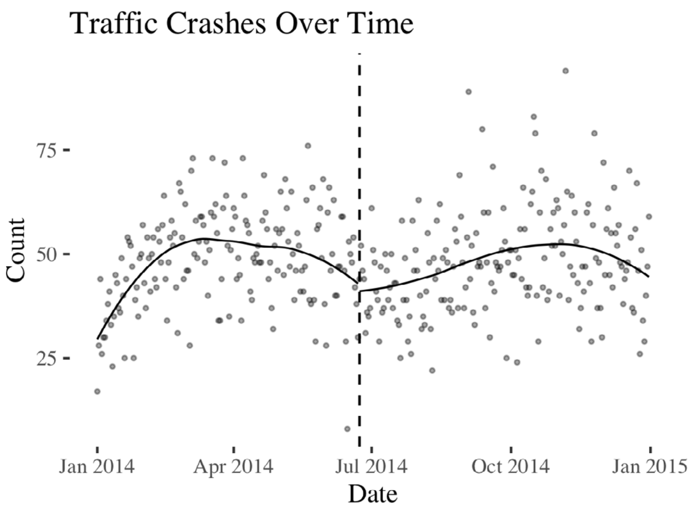

FIGURE C19 Daily number of traffic crashes (y-axis) over time (x-axis) in Hidalgo and Starr counties during 2014. Dark lines are fitted loess linens on each side of the moment OSS was implemented. Dashed vertical line is the moment OSS is implemented.

OSS traffic crashes analysis (full dataset)

TABLE C8 Effect of OSS on traffic crashes (using all data).

# traffic crashes								
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
OSS	−0.72	−9.53***	−3.26	−2.69	−5.24	−4.94	−0.93	−1.10
	(1.27)	(2.50)	(3.55)	(4.72)	(3.89)	(4.03)	(4.34)	(4.95)
Controls	N	N	N	N	Y	Y	Y	Y
Polynomial	0	1	2	3	0	1	2	3
N	365	365	365	365	364	364	364	364
R^2	0.00	0.05	0.15	0.16	0.42	0.43	0.45	0.45

Note: All regressions characterize the effect of OSS on the number of traffic crashes within Hidalgo and Starr counties. Models 1–4 do not include control covariates. Models 5–8 adjust for day of week, month, and year fixed effects. Models 1–4 and Models 5–8 use 0, 1st, 2nd and 3rd order polynomials for the running variable, respectively. HC2 robust SEs in parentheses.
***$p < 0.001$;
**$p < 0.01$;
*$p < 0.05$.

OSS traffic crashes analysis (close to discontinuity)

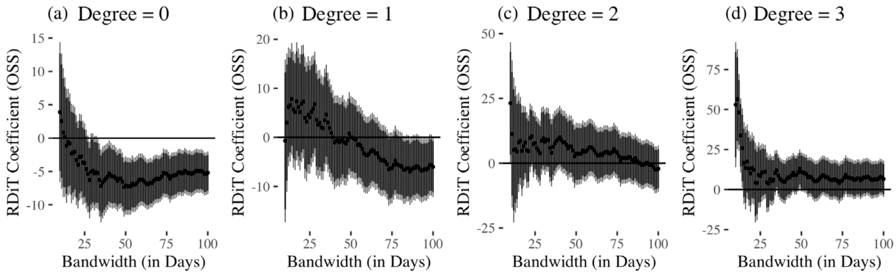

FIGURE C20 RDiT effect of OSS on number of traffic crashes (y-axis) by different bandwidths (x-axis). Panels A–D characterize estimates where the running variable degree (days to OSS) is to the 0th, 1st, 2nd, and 3rd degree. 95% CIs displayed derived from robust SEs.

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Demonstrating nearly all drivers say "yes" to consent searches

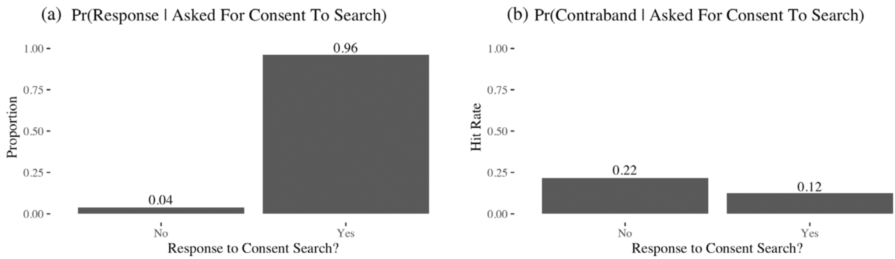

FIGURE C21 Proportion of drivers who say "no" or "yes" during consent searches (Panel A) and contraband recovery rate of drivers who say "no" or "yes" to consent searches (Panel B). Data are from LAPD traffic stops between January 2020 and October 2022.

Difference-in-differences replication

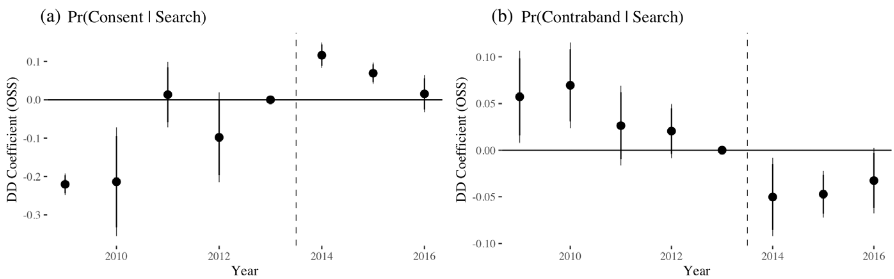

FIGURE C22 Event study estimates assessing the differential effect of operation strong safety on Hidalgo and Starr counties. Dashed vertical line indicates the onset of operation strong safety. 95% CIs displayed derived from HC2 robust SEs clustered by county.

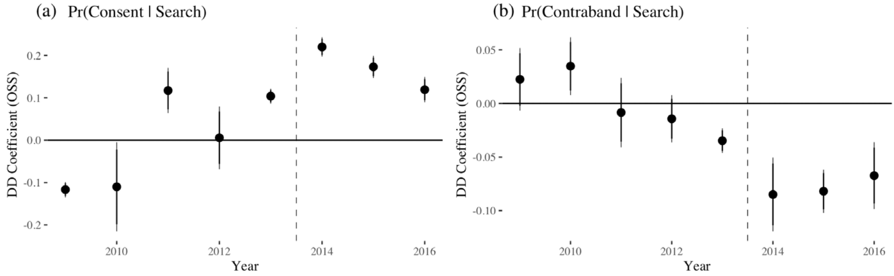

FIGURE C23 Event study estimates assessing the differential effect of operation strong safety on Hidalgo and Starr counties. Dashed vertical line indicates the onset of operation strong safety. Estimates derived from a generalized synthetic control approach developed by Xu (2017), which attempts to reweight non-Hidalgo/Starr counties to produce an appropriate counterfactual with parallel pre-trends. 95% CIs displayed derived from bootstrapped SEs clustered by county.

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Media coverage of OSS

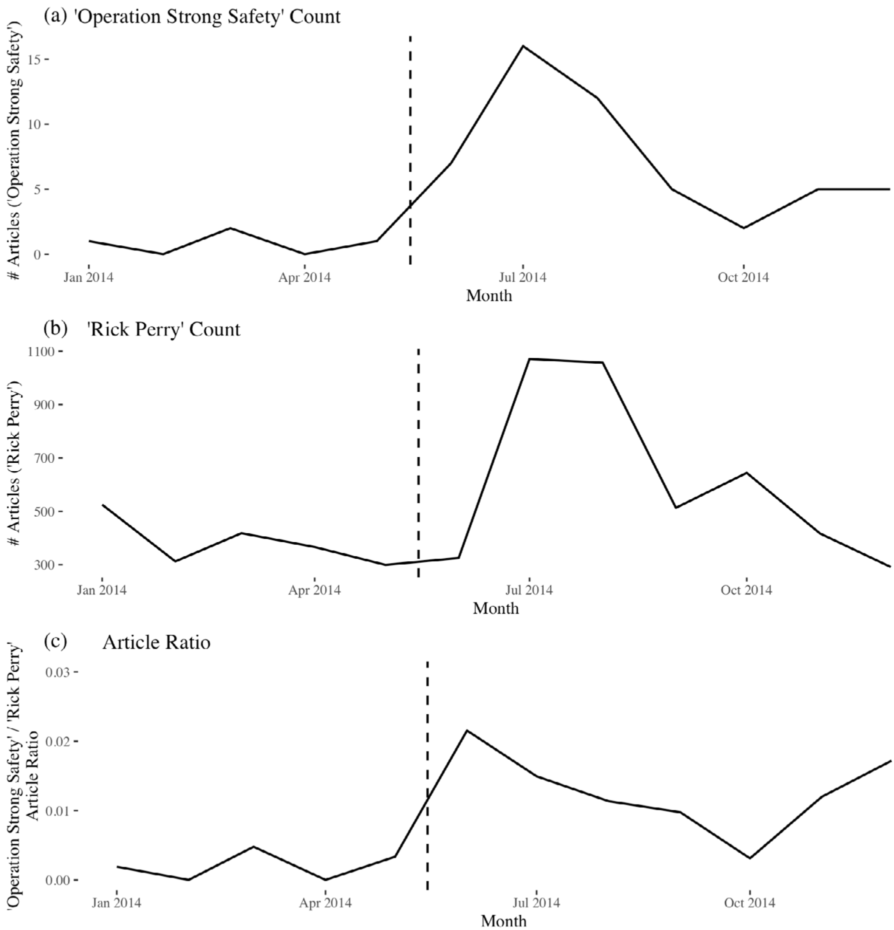

FIGURE C24 Media coverage of operation strong safety (y-axis) over time during months of 2014 (x-axis). Panel A characterizes the count of articles related to "operation strong safety." Panel B characterizes the count of articles related to "Rick Perry." Panel C characterizes the ratio of articles related to "operation strong safety" to articles related to "Rick Perry." Data are from the Nexis Uni search database. Data on count of articles are from newswires and press releases OR newspapers in the Nexis Uni database.

Google search intensity: Operation strong safety

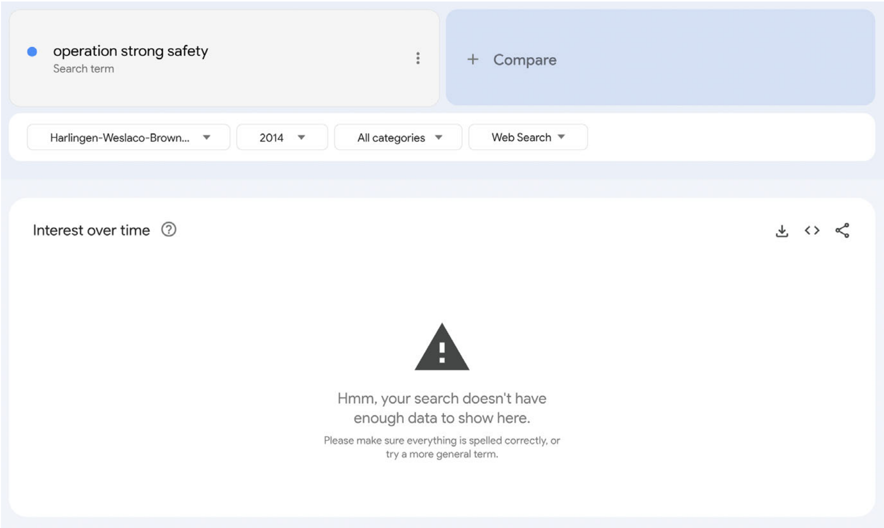

FIGURE C25 Google search intensity on "operation strong safety" in the Harlington–Weslaco–Brownsville–McAllen metropolitan area. [Color figure can be viewed at wileyonlinelibrary.com]

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McAllen crime analysis

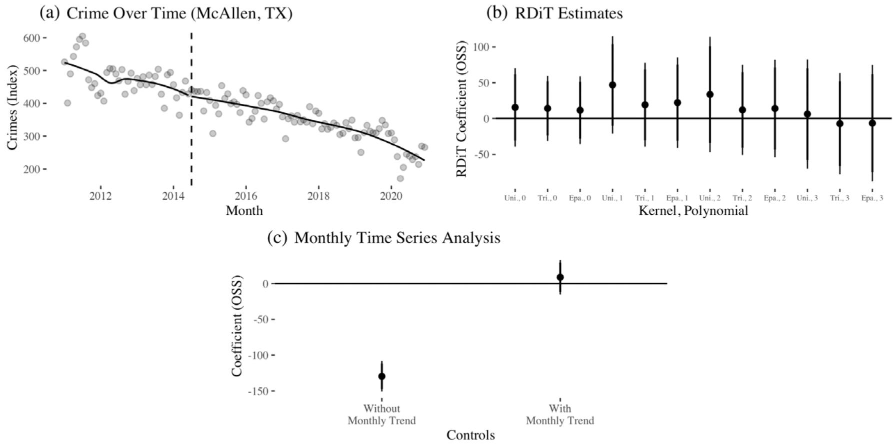

FIGURE C26 Assessing the effect of operation strong safety on crime in McAllen, Texas. Panel A characterizes the count of "index" crimes (y-axis) over time by month (x-axis) in McAllen, Texas between 2011 and 2020 using official McAllen Police Department statistics (*Source*: https://www.mcallen.net/departments/pd/records-bureau/crime-reports). Index crimes are: murder, forcible rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, arson, and human trafficking. The dashed vertical line denotes the onset of operation strong safety. Loess lines are fit on each side of the moment OSS was implemented. Panel B characterizes regression discontinuity-in-time estimates of the immediate, discontinuous effect of OSS on index crimes (y-axis) across kernel and polynomial specifications using the optimal bandwidth selection procedure by Calonico et al. (2015). Panel C characterizes a simple difference-in-means analysis assessing the effect of operation strong safety (y-axis) on index crimes without and with adjusting for monthly time trends (x-axis).