Deputize and Deport: The Effect of Immigration Enforcement on Policing

Marcel Roman*

January 8, 2021

Abstract

Does expanding the role of the police to include immigration enforcement reduce their effectiveness? I use a regression discontinuity design and daily data on over 17 million traffic stops to evaluate the effect of a policy directive increasing Texas Department of Public Safety (DPS) Highway Patrol presence near the border in the predominantly Latinx counties of Hidalgo and Starr for the purposes of anti-human and drug trafficking. I find the directive substantially increased the number of traffic stops in its area of operations, increased the rate of unnecessary stops, and decreased the stop-and-search hit rate for recovering relevant contraband (e.g. drugs, weapons). Moreover, the directive appears to have had limited effectiveness in detecting human smuggling, finding undocumented immigrants, and reducing crime. These effects are not driven by an influx of inexperienced troopers, but rather lower thresholds for initializing a traffic stop motivated by the directive, consistent with qualitative accounts of profiling and unwarranted policing in the South Texas region.

*University of California-Los Angeles. I thank Matt Barreto, Efrén Pérez, and Emily Weisburst for their helpful comments and insight.

Introduction

What are the consequences of deputizing local police to enforce Federal immigration priorities? The question is relevant as local police have become increasingly involved in immigration enforcement since the passage of the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) in 1996. §287(g) and Secure Communities relegated Federal immigration enforcement authority to local law enforcement and increased cooperation on identifying and detaining undocumented immigrants under the guise of crime control.[1] States have also expanded the role of local law enforcement in immigration outside the scope of Federal mandates with "show-me-your-papers" laws such as Arizona's SB 1070 (2010), Alabama's HB 56 (2011), and Texas' SB 4 (2017). Likewise, a number of local police agencies have intervened independently. Some agencies, like the New Orleans police department, outlined rules against asking about immigration status and stopping individuals solely on the basis of perceived immigration status (NOPD, 2016). Others, like Arizona's Department of Public Safety Border Strike Force Bureau, are actively involved in enforcing immigration laws on the US-Mexico border (Ruelas and Pohl, 2018).

The underclass thesis of bureaucracy suggests street-level bureaucrats systematically discriminate against low-income and non-Anglo white communities (Sjoberg, Brymer, and Farris, 1966; Lineberry, 1975; Lipsky, 2010).[2] Concomitantly, legal scholars have raised concerns immigration policing may result in ethno-racial profiling, unnecessary stops and searches, and reduce police effectiveness by distracting officers from prioritizing high-level criminal activity (Arnold, 2007; Gill et al., 2009; Haas, 2011; Johnson, 2015). These concerns are likely informed by significant evidence policing practices are often unwarranted and racially discriminatory without the imposition of immigration policing

[1]§287(g) allows local law enforcement agencies to establish agreements with the Department of Homeland Security to enforce Federal immigration law. These include street-level enforcement actions (discontinued since 2012) and interrogations at jails to identify undocumented immigrants for referral to ICE (American Immigration Council, 2012). Secure Communities increased data-sharing between ICE and local jails to identify and hold undocumented immigrants on detainer (American Immigration Council, 2011). The Obama administration reduced the scope of immigration detainers in 2015 with the Priority Enforcement Program (PEP). However, Secure Communities was reinstated by President Trump's Executive Order 13768 in 2017.

[2]I specify Anglo whites since, for example, there are perceptibly and/or racially white Latinxs. This distinction has important consequences on life chances (López et al., 2018).

directives (Gelman, Fagan, and Kiss, 2007; Goel, Rao, and Shroff, 2016; Knox, Lowe, and Mummolo, 2019; Pierson et al., 2020; Feigenberg and Miller, 2020). On the other hand, street-level bureaucrats may engage in bureaucratic incorporation of immigrants and their co-ethnics independent of political considerations or the predispositions of elected leaders (Marrow, 2011; Williamson, 2018). Immigrant incorporation is particularly relevant for street-level police, who depend on community cooperation for identifying criminal activity, may want to avoid complaints from the community, and may seek efficiency in reducing crime (Lewis and Ramakrishnan, 2007). However, immigrant incorporation may be limited when immigration policing measures are politicized by electoral or departmental superiors, incentivizing street-level bureaucrats to focus on regulatory (e.g. traffic stops) over service-oriented interactions with immigrants and their co-ethnics, with potentially disparate consequences (Marrow, 2009).

In light of limited evidence evaluating the consequences of expanding the role of local police to include immigration or border enforcement on police behavior,[3] I adjudicate between the two aforementioned perspectives by assessing the consequences of a Texas Department of Public Safety (DPS) directive, *Operation Strong Safety*, jointly initialized by Governor Rick Perry and DPS director Steve McCraw on June 23, 2014, at the height of the Central American migrant crisis. Operation Strong Safety (OSS) increased DPS highway patrol trooper presence in the predominantly Latinx counties of Starr and Hidalgo for the stated purpose of preventing human smuggling and drug trafficking along the border.[4] Journalistic accounts suggest an additional implicit purpose of OSS was to prevent undocumented migrant crossings (Bosque, 2018).[5]

[3]However, there is some qualitative evidence suggesting immigration deputization mandates may motivate ethno-racial profiling (Armenta, 2017a). Moreover, a quantitative exception is Ciancio (2017), who evaluates the effect of the Priority Enforcement Program, which reduced the number of detainers sent to ICE, on crime clearance rates.

[4]I use "Latinx" instead of "Latino" to explicitly include gender non-binary and transgender people in the Latinx category. However, many Latinxs, including the predominantly Mexican-American communities in South Texas, may not personally characterize themselves as "Latinx (Noe Bustamante, Mora, and Lopez, 2020)."

[5]For instance, Terry Burke, the executive director of the ACLU, indicated "from the time Operation Strong Safety was announced last summer, the ACLU believed part of the objective was to enforce immigration law (Schladen, 2015)." Indeed, DPS troopers are now integrated with Patrol (CBP) officers in "Cortina units," where DPS troopers ride with CBP officers in the same vehicle, throughout South Texas (Benning, 2016). Moreover, the DPS now shares names of drivers who have received citations with CBP to identify potential undocumented immigrants (Associated Press, 2018).

Using data on 17 million independent traffic stops (2009-2016) and a regression discontinuity design, I estimate OSS discontinuously increased border stops in Hidalgo/Starr county by over 250%, increased the rate of warning stops (as opposed to citations) by 31 percentage points (relative to a 48 percentage point baseline mean prior to OSS), and decreased the contraband hit rate for weapons and drugs by 8 percentage points (relative to a 7 percentage point baseline mean prior to OSS). Moreover, I show OSS made an insignificant impact on identifying undocumented immigrants and human smugglers. Consistent with the underclass thesis, concerns posed by legal scholars, and qualitative accounts of over-policing in South Texas, these findings suggest OSS increased the rate of unnecessary stops and/or searches and decreased the efficiency of policing operations. These estimates are also not driven by an influx of inexperienced troopers in Hidalgo/Starr, but rather lower thresholds for initiating contact with drivers among troopers motivated by the directive.

This paper makes a number of contributions. In light of neglect concerning the coercive apparatus of the state in the field of American politics, a nascent literature in political science is assessing the consequences of policing and coercive institutions within what Soss and Weaver (2017) call "race-class subjugated communities." Much of this research focuses on the consequences of policing or coercive institutions on political behavior, particularly political participation (Weaver and Lerman, 2010; Burch, 2013; Walker, 2014; Gerber et al., 2017; Owens and Walker, 2018; Laniyonu, 2019; White, 2019a; White, 2019b; Williamson, Trump, and Einstein, 2018; Walker, 2020; Komisarchik, Sen, and Velez, 2020), and government trust (Lerman and Weaver, 2014; Peyton, Sierra-Arévalo, and Rand, 2019). However, with few exceptions (Mummolo, 2018; Hausman and Kronick, 2019), there is limited research evaluating the effect of policies that reshape bureaucratic incentives and encourage warrantless policing practices, much less at the intersection of immigration. Analyzing policy and street-level police behavior is necessary to understand the prospects for reform, shortcomings of reform, and policies that may actually make policing worse. Moreover, if political science is about analyzing what determines who gets what, when, and how, it is necessary to evaluate policies that may produce differential distributions of coercion, especially if they disproportionately implicate race-class subjugated communities. This paper makes a contribution by being the first to causally identify the effect of expanding the role of the police to include immigration and/or border enforcement on a variety of relevant policing outcomes that account for warrantless and inefficient policing.

Moreover, this paper highlights the limits of the immigrant incorporation thesis, which posits bureaucratic institutions have a number of social, economic, and political incentives to serve immigrants effectively without alienating them or their co-ethnics (Lewis and Ramakrishnan, 2007; Marrow, 2011; Williamson, 2018). I show empirical evidence consistent with Marrow (2009), who theorizes the politicization of the role of street-level bureaucrats (in this case, DPS troopers) with concomitant directives from high-level departmental and political principals (in this case, Governor Rick Perry and DPS Director McCraw) can exacerbate the exclusion of immigrants by emphasizing regulatory aspects of bureaucratic goals (e.g. traffic stops) as opposed to service-oriented goals.

The empirical findings in this paper also contribute to a growing literature underscoring the power of directives from high-level principals on normatively positive or negative police behavior (Mummolo, 2018; Hausman and Kronick, 2019). A large swath of prior work assessing the causes of warrantless or discriminatory policing has emphasized the individual characteristics of officers (e.g. "bad-apples," implicit bias, authoritarian personality, bigotry, conservative attitudes, sensitivity trainings, etc.) (Balch, 1972; Twersky-Glasner, 2005; Skolnick, 2010; Bonanno, 2015; Morin et al., 2017). Yet, existing solutions to resolving individual-level predispositions may be ineffective or without significant supporting evidence (Christie, Petrie, and Timmins, 1996; Paluck and Green, 2009). But, directives initialized by high-level principals appear to have substantial influence on police behavior (Mummolo, 2018), even for the worse (Hausman and Kronick, 2019). This paper provides more evidence that high-level bureaucratic directives can reorient the goals of police departments and lead to deleterious outcomes.

More broadly, this paper also contributes to a multidisciplinary literature assessing the effects of immigration policing on a wide array of outcomes, including, demographic changes (Dee and Murphy, 2020), employment (Pham and Van, 2010; Bohn and Santillano, 2017;

East et al., 2018), sector-specific shocks (Kostandini, Mykerezi, and Escalante, 2014), homeownership (Rugh and Hall, 2016), social service participation (Alsan and Yang, 2018), prenatal care usage (Rhodes et al., 2015), crime (Nowrasteh, 2018), and political participation (White, 2016; Walker, Roman, and Barreto, 2019; Kuhn, 2020; Walker, Roman, and Barreto, 2020). Prior research also demonstrates immigration policing reduces cooperation with law enforcement (Vidales, Day, and Powe, 2009; Theodore and Habans, 2016; Jacome, 2019). However, it is unclear whether the mechanism is due to changes in police behavior or the perception of threat. This paper demonstrates immigrant communities may have good reason to fear expanding the role of police in immigration enforcement, since it motivates over-policing in predominantly low-income and immigrant communities.

How the police in/exclude immigrants and their co-ethnics

A growing segment of research demonstrates bureaucratic institutions and street-level bureaucrats have strong incentives to serve immigrant communities without alienating them despite the political constraints or controversies engendered by immigration issues. Since bureaucrats interface with immigrants as clients more often than elected officials and have a high level of discretion in the provision of resources (Lipsky, 2010; Maynard-Moody and Portillo, 2010), bureaucratic institutions are often "first-movers" in attempting to provide equitable services to immigrants and their co-ethnics (Winders, 2012). Williamson (2018) suggests bureaucratic incorporation of immigrants, even in conservative contexts, occurs because service-providers perceive immigrants as vital to economic development and departmental goals would be undermined if immigrants were excluded as clients.

Police departments may also be incentivized to cooperate and work with immigrant communities (Lewis and Ramakrishnan, 2007). Working with Federal immigration authorities or profiling on the basis of race, ethnicity, or immigration status may reduce crime reporting among immigrants and their co-ethnics, frustrating departmental goals (Vidales, Day, and Powe, 2009; Theodore and Habans, 2016; Jacome, 2019).[6] Although the Texas DPS highway patrol is a state agency, their policing actions are embedded in urban areas and communities near the South Texas border (Bosque, 2018). DPS troopers cooperate with local law enforcement agencies in identifying human smuggling (CBP, 2020). They also have a tip line for civilians to report suspicious activities (DPS, 2015).

Additionally, police officers may not want to face complaints related to warrantless policing or profiling on the basis of race and/or immigration status. Texas DPS officers are subject to complaints (McGaughy, 2015), and only troopers in good standing (e.g. no complaints, disciplinary actions) are eligible for promotion (Luh, 2019). Incentives for avoiding complaints may have also increased since the murder of Sandra Bland after she was stopped and detained by a DPS officer, precipitating increased scrutiny on DPS traffic stop disparities (Ura, 2016).[7]

Moreover, police may want to reduce their rates of warrantless stops against immigrants and their co-ethnics in order to focus their efforts on identifying and preventing high-level criminal activity (e.g. recovering drug and weapon contraband). Indeed, the DPS emphasized the main goal of Operation Strong Safety was to stem cartel activity, drug trafficking, and human smuggling. Funding for the operation is also principally evaluated by the Texas State Legislature on the basis of meeting this goal (Schwartz, 2016b). Lewis and Ramakrishnan (2007) highlight the aforementioned incentives and demonstrate incorporative behavior on part of police departments is motivated less by politics, and more by the size of immigrant communities. Their findings underscore the rational basis police departments have to generate goodwill with immigrants as they comprise a larger proportion of the community they serve.

[6]The Texas DPS admits preventing mistrust related to profiling is important, indicating, "Trust and mutual respect breaks down if the community we serve believes that law enforcement is impartially and unfairly targeting people or subjecting certain groups to increased scrutiny. Racial and ethnic profiling, real or imagined, quickly causes this breakdown of public trust (Department of Public Safety, 2000)." Moreover, when asked about the risk of racial profiling during Operation Strong Safety to identify and refer undocumented immigrants to Federal authorities, DPS spokesman Tom Vinger indicated "Racial profiling is illegal and prohibited by DPS policy (Aguilar, 2014)."

[7]There is evidence suggesting the DPS is highly responsive to scrutiny. After the release of dashcam video recording Bland's traffic stop on July 28, 2015, there was a precipitous drop in the number of stops in both Starr and Hidalgo counties (Figure 1). However, there was no commensurate decrease in the *rate* of warrantless stops, measured by warning stop rates and hit rates for stop-and-searches.

However, the prospects for immigrant incorporation may break down in certain contexts. Prior theory and evidence suggests bureaucratic directives from high-level superiors can dramatically change the behavior and incentive structure of street-level bureaucrats (Mummolo, 2018; Hausman and Kronick, 2019). Principals can leverage new rules, funding, and sanctions to align the behavior of street-level bureaucrats with their preferences (McCubbins, Noll, and Weingast, 1987; Carpenter, 1996; Huber and Shipan, 2002). Operation Strong Safety may have reshaped DPS trooper incentives along a number of dimensions that undercut relatively optimal forms of discretion that otherwise reduce the rate of warrantless policing in the South Texas border.[8]

The directive was an unequivocal mandate from both the top departmental and political principals (i.e. DPS Director McCraw and Governor Perry), with the support of the Republican Texas State Legislature. Support from multiple principals may reduce shirking since there are limited avenues for complying with alternative preferences from other principals (Miller, 2005). Operation Strong Safety was also implemented in a politicized context. Rick Perry developed the directive in response to the Central American migrant crisis. There are accounts suggesting Perry was using the crisis to delegitimize the border enforcement efforts of the Obama administration to shore up support from an anti-immigrant GOP base in the run-up to the 2016 Republican presidential primary (Altman and Miller, 2014; Benen, 2014; Root, 2014). Consistent with Marrow (2009), the politicization of immigration issues may motivate superiors to frustrate the bureaucratic incorporation of immigrants via restrictive directives and heightened oversight on police behavior.

Operation Strong Safety was well-funded, which may encouraged warrantless policing among DPS troopers. The governor and Texas House Speaker authorized the redirection of money, equipment, and troopers toward the border on short notice (Aguilar, 2014). The Texas State Legislature later approved $800 million for the operation in 2015 (Aguilar,

[8]This is not to say trooper discretion was optimal prior to Operation Strong Safety. Just that it *relatively* better. The mean warning/hit rate in Hidalgo/Starr was still higher/lower than the rest of Texas (48 percentage points relative to 24 percentage points for warnings, 7 percentage points relative to 20 percentage points for hit rates) (Tables 1 and 2). However, it is important to note that the hit rate was improving over time (Figure 2).

2015). Prior research suggests budgetary signaling encourages adherence to the preferences of superiors among street-level bureaucrats since it presupposes extensive oversight on part of political principals (Carpenter, 1996). Additionally, DPS troopers had strong positive incentives to adhere to mandates to engage in border enforcement. Operation Strong Safety generated new recruits, jobs, promotions, and access to equipment (Burnett, 2015; Department of Public Safety, 2015a). Participation in Operation Strong Safety helped justify the need for more funding and resources to the agency (Aguilar, 2015). Moreover, an influx of troopers in the border region may have perceptibly reduced the cost of engaging in warrantless policing, since other officers could compensate for an increase in the rate of unnecessary stops.

Metrics for evaluating the effectiveness of Operation Strong Safety were also distorted, which may have provided license for warrantless policing. The DPS was not clear about what constituted a successful operation and often shifted metrics for success. During State Congressional hearings, the DPS would describe border operations as successful for its ability to intercept more drugs and people in one instance, less drugs and people in another instance, or its ability to provide an anecdotal sense of safety among locals in a different instance (Dexheimer and Schwartz, 2018). There is also suggestive evidence the DPS characterized Customs and Border Patrol (CBP) metrics on drug seizures and apprehensions of persons as their own, with no data on the cooperative contribution of the DPS to the CBP statistics or the individual contribution of the DPS border enforcement effort (Washington and Saacks, 2015).[9] Vagueness on success metrics may have provided sufficient cover and reduced accountability for troopers to engage in problematic policing practices (Wilson, 1978).

Although there is prior research suggesting street-level police may be resistant to bureaucratic directives and supervision from superiors, these studies primarily focus on reformist policies that reduce the scope of policing behavior. Mechanisms that forestall reform, such as the lack of congruence between the preferences of superiors and the

[9]For instance, when State Representative Cesar Blanco of El Paso asked for DPS-specific performance metrics during Operation Strong Safety, the agency responded instead with drug seizure information that combined efforts from state, local, and Federal agencies (Dexheimer and Schwartz, 2018).

predispositions of street-level bureaucrats (Brehm and Gates, 1999), are mechanisms that may motivate warrantless policing under a directive that expands the role of the police to engage in border and immigration enforcement. Officer predispositions concerning notions of immigrant illegality and criminality at the border may be primed as a result of immigration policing directives, generating a behavioral basis for using lower thresholds to stop and/or search civilians (Armenta, 2017b). Therefore, consistent with the principles of the underclass hypothesis of bureaucratic behavior and the concerns of legal scholars who posit immigration policing may motivate discriminatory behavior, immigration policing directives may reinforce societal hierarchies that exist independent of street-level interactions with civilians (Lineberry, 1975; Arnold, 2007; Gill et al., 2009; Lipsky, 2010; Haas, 2011; Johnson, 2015).

In sum, although there are incentives for the bureaucratic incorporation of immigrant communities among street-level police bureaucrats, more specifically, Texas highway patrol troopers, well-funded and unequivocal bureaucratic directives mandating immigration policing in a politicized context can undercut the prospects of inclusion and encourage warrantless policing practices.

Safety for whom?

Operation Strong Safety (OSS) is a directive jointly implemented by the Texas Governor, Lieutenant Governor, and Speaker of the House on June 23rd, 2014 mandating the Texas Department of Public Safety (DPS) "combat the criminal elements exploiting the Texas-Mexico border" during the 2014 Central American migrant crisis (Department of Public Safety, 2015b). The directive redirected DPS highway patrol resources to Hidalgo and Starr county for the stated goal of reducing human smuggling, drug trafficking, and Mexican drug cartel crime along the border. The DPS indicated OSS was meant to supplement CBP enforcement efforts given potential resource overstretch during the migrant crisis (Department of Public Safety, 2015a). The DPS highway patrol primarily drove within and near border towns (e.g. McAllen, Pharr, Mission) and Highway 83, which

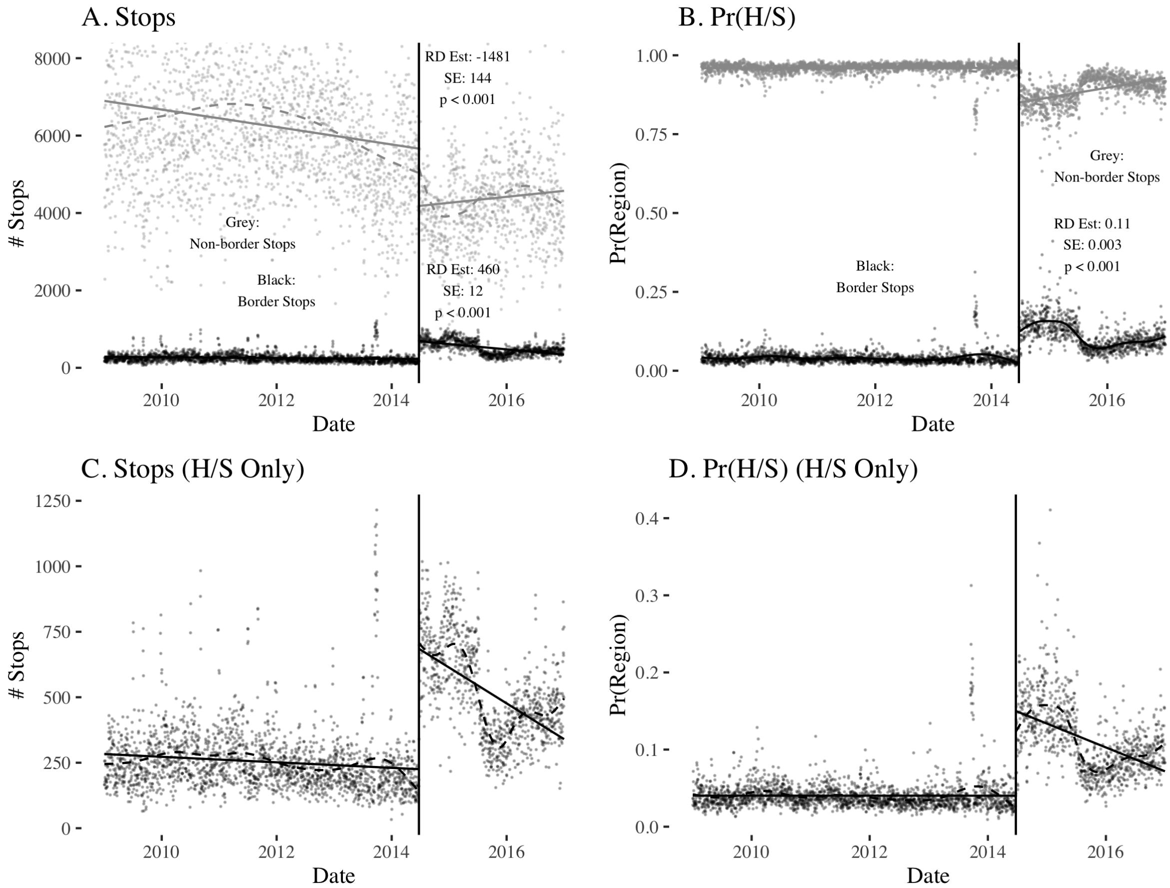

Figure 1: Traffic stops (y-axis) over time (day, x-axis). Panels A and B disaggregate traffic stop data by border region (i.e. Hidalgo/Starr (H/S) or not). Panels C and D only show data from the Hidalgo/Starr region. The y-axis for Panels A and C characterizes the raw number of stops. The y-axis for Panels B and D characterizes the probability a stop is from a specific region. The vertical line characterizes the implementation of Operation Strong Safety (on 2014-06-23). Solid lines characterize linear models fit on each side of the discontinuity. Dashed lines characterize loess models fit on each side of the discontinuity.

cut through a number of communities near the border.[10] Importantly, the policy was announced only two days prior to its implementation, minimizing the risk of anticipatory effects on part of the police or the driving population (Aguilar, 2014).[11]

According to Figure 1, OSS shifted DPS regional priorities. OSS discontinuously increased the number of stops in the Hidalgo/Starr region by 460 relative to a baseline of roughly 250, a 180% increase in the raw number of stops (Panels A and C). Conversely, OSS discontinuously decreased the number of traffic stops by roughly 1500 throughout the rest of Texas from an immediate prior baseline of 6000. Likewise, the proportion of stops that occur in Hidalgo and Starr increase by 11 percentage points relative to a baseline of roughly 4 percentage points (Panels B and D).

OSS involved the redeployment of DPS troopers from throughout the state of Texas to Hidalgo and Starr. The number of individual officers who initialized a stop in Hidalgo/Starr on a given day discontinuously increased from a baseline of roughly 50 to just over 150, 3x the baseline number of troopers (Appendix Section E.2, Figure E.4, Panel A). The cumulative number of officers who initialized a stop in Hidalgo or Starr increases from 800 to over 2000 near the time OSS is implemented (Appendix Section E.2, Figure E.4, Panel B). The Texas Highway Patrol is broken up into 6 operational regions, 19 districts, and 157 sergeant areas (Feigenberg and Miller, 2020).[12] RD estimates using trooper/day-level data demonstrate OSS significantly increased the probability of stops in Hidalgo/Starr while decreasing the probability of stops occurring in all operational regions (including parts of Region 3 excluding Hidalgo/Starr, which is otherwise inclusive of Hidalgo/Starr, Appendix Section E.1, Tables E.1 and E.2). Interestingly, it appears the relative decrease in stop rates outside Hidalgo/Starr are equally distributed across regions instead of deriving from a particular area of Texas.

To implement OSS, significant financial resources were amassed for the directive. Governor Perry and the Texas State Legislature immediately authorized the redirection of

[10]See Appendix Section B, Figure B.1 for a photo of the OSS Area of Operations.

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

[12]See Appendix Section C, Figure C.2 for a map of Texas Highway Patrol regions and districts.

appropriated funds toward DPS border security efforts. Between 2012/2013 and 2014/2015, spending on border security increased from \$222 million to \$510 million. The increase in border security spending is sustained to this day, with \$800 million earmarked annually for border security (Appendix Section D, Figure D.3) (Aguilar, 2015).

Operation Strong Safety faced criticism on multiple dimensions from politicians and civil rights organizations. There is evidence to suggest it was ineffective in meeting its stated goals of preventing human smuggling and drug trafficking. On human smuggling, Governor Perry claimed OSS sharply reduced border apprehensions. However, he used CBP data to make the claim, which does not measure the DPS contribution to the effort (Dexheimer and Schwartz, 2018). There is reason to be skeptical DPS significantly contributed to the CBP apprehension numbers in 2014 given CBP admitted they had limited operational integration with DPS at the time OSS was first implemented (Schwartz, 2016a). Moreover, since DPS highway patrol cannot directly engage in immigration enforcement along the border or the Rio Grande, they policed border towns and Highway 83, which may have reduced rates of contact with potential undocumented immigrants or human smugglers (Rosenthal, 2015). The reduction in border apprehensions after June 2014 may have also been motivated by two confounding factors: an increase in CBP patrols mandated by DHS Secretary Jeh Johnson at the start of May (Johnson, 2014), and a mass immigration crackdown on Central American migrants by Mexican authorities (WOLA, 2015).[13]

With respect to drug trafficking, the DPS also used CBP metrics to evaluate the effectiveness of their efforts despite limited initial coordination. The American-Statesman, an Austin newspaper, acquired disaggregated data and found the DPS contributed less than 10% of the yield in drug interdiction efforts during OSS, with the rest of the seizures coming from other law enforcement agencies (Dexheimer and Schwartz, 2018). Moreover, the DPS has been prone to using alternative metrics for assessing the success

[13]It is possible the increase in CBP patrols on May 12 may confound the main estimates evaluating the effect of OSS since CBP patrols may change the composition of drivers. I estimated a series of placebo tests assessing the effect of CBP patrols on the main outcomes of interest (warning and hit rates) and find no discontinuous effects of CBP patrols on hit rates and a negative discontinuous effect for warning rates (which is in the opposite expected direction of the effect for OSS), suggesting the CBP patrols do not influence the main results. These results also underscore the limited confounding influence of the Central American migrant crisis, which was in full swing by March 2014.

of drug interdiction efforts and has inflated estimates regarding the value of drugs seized (Dexheimer and Schwartz, 2018; Schwartz and Collier, 2018). The limited impact of OSS on drug seizures is consistent with critics who argued a policing surge is an ineffective response to a child migrant crisis, which may have limited correlation with drug cartel activities (Aguilar, 2014).

Additionally, concerns have been raised that Operation Strong Safety is a trojan horse for encouraging immigration enforcement among DPS troopers along the border. Officially, the DPS rejects the notion OSS is an immigration enforcement operation explicitly focused on stopping the flow of unauthorized immigration. However, DPS still provides its troopers leeway to refer individuals they suspect of being undocumented to Customs and Border Patrol (Aguilar, 2014).[14] Terri Burke, the Texas Director of the ACLU, is skeptical of DPS indications they are not actively enforcing immigration law, indicating "just as they get the person pulled over, the Border Patrol shows up. (DPS Troopers) are a stalking horse for the border patrol." Burke goes on to mention the ACLU believed part of the objective for OSS was to enforce immigration law (Aguilar, 2014).

Democratic Texas State Representative Trey Fisher of San Antonio was concerned about the high rate of warning stops (as opposed to stops that lead to citations) after OSS. Fisher indicated the high rate of warnings suggests a large number of stops are not "being made for law enforcement reasons (Aguilar, 2014)." Another State Representative, Ryan Guillen of Starr County, raised concerns about the frequent unnecessary and minor traffic stops in the region his constituents faced. He also expressed the purpose of many stops in Hidalgo/Starr after OSS may have little to do with traffic violations, indicating "you stop them, you stop and see if there's something else, or you stop and see if they take off. That's probably what they're after."

Qualitative accounts of DPS behavior reinforce the sentiment expressed by these politicians. In one incident, DPS and CBP officers stopped and questioned a woman at a baseball game about her immigration status (Bosque, 2018). In another incident, a

[14]For instance, DPS spokeperson Tom Vinger indicated "When our state law enforcement officers make contact with someone (during a lawful encounter) who is admittedly or suspected to be in the country illegally, that individual is immediately referred to the appropriate Federal authorities."

Starr County police officer identified undocumented immigrants on the police dispatch, asking for CBP assistance. DPS troopers showed up instead to chase the unauthorized immigrants. Later, the Texas DPS officer asked the Starr County officer why he "didn't want to come out and play (e.g. to assist in detaining the migrants)." The Starr County officer indicated he was "not authorized to enforce immigration law." The DPS officer responds by saying "Hey partner, I'm just following orders (Bosque, 2018)." Given the qualitative accounts DPS officers are focused on apprehending unauthorized migrants, it is necessary to evaluate if OSS meaningfully increased the detection of human smuggling or immigration violations among highway patrol.

The operational emphasis on unauthorized immigrants and drug trafficking in the predominantly Latinx Hidalgo and Starr counties has raised concerns the directive is resulting in pre-textual and potentially unnecessary stops. Although ethnicity cannot be a sole basis for a traffic stop, it can be a basis among other considerations that meet relatively weak standards for reasonable suspicion (Johnson, 2009). Therefore, it is necessary to evaluate if Operation Strong Safety, a policy which has been characterized as a parallel to New York's stop-and-frisk (Schladen, 2016), has lowered the threshold for initializing a stop and/or search on part of highway patrol in the Hidalgo/Starr region.

Design

Data

To evaluate the effect of Operation Strong Safety, I use data on 17 million individual traffic stops recorded by the Texas Department of Public Safety Highway Patrol between January 1, 2009 and December 31, 2016 (2920 days). The Stanford Open Policing Project (SOPP) collected these data and are publicly available on their website.[15] The data contain a number of features useful for testing the hypotheses. The location of the stops is recorded by county, allowing for a disaggregated analysis between the OSS area of

[15] See: `https://openpolicing.stanford.edu/data/`. The SOPP data are especially useful since they attempt to correct deliberate miscoding of Latinxs by the DPS using ethnicity probability estimates conditional on last names from 2000 Census data (KXAN, 2015; Luh, 2019).

operations (Hidalgo and Starr county) and the rest of Texas. The data are high frequency in that they are recorded at the date-level, allowing for a research design that can assess the discontinuous and immediate effect of OSS on policing outcomes, which may be less perturbed by omitted temporal dynamics or endogenous driver behavior. Additionally, the data include information on traffic and criminal violations identified by the officer, which can help rule out if OSS is effectively reducing human smuggling and unauthorized immigration. In total, there are 16,589,141 stops outside of Hidalgo/Starr, 981,046 stops inside Hidalgo/Starr, 303,406 stop-and-searches outside of Hidalgo/Starr, and 16,191 stop-and-searches inside Hidalgo/Starr.[16]

Moreover, the data include a variety of indicators that help measure potentially unnecessary, warrantless stops, and serve as the main dependent variables of interest. The first outcome is an indicator for whether a stop resulted in a warning (as opposed to a citation). Assuming the characteristics of the unobserved driver population is smooth near the moment OSS was implemented, a precipitous increase in warning stops may be evidence of an increase in lower legal thresholds for initializing traffic stops that do not warrant the provision of a citation (Schladen, 2016; Hausman and Kronick, 2019).[17]

The second outcome is whether a stop-and-search resulted in the recovery of drug and/or weapon contraband, otherwise known as the "hit rate." I focus on drug or weapon recovery since this is a principal goal of Operation Strong Safety. A reduction in the hit rate may suggest an increase in unwarranted stop-and-searches and a reduction in the efficiency of policing operations (Persico and Todd, 2006). I also use additional data on stop-and-search justifications to demonstrate DPS highway patrol is lowering the legal

[16]Violations are recorded post-stop and as a list. So the violation is not necessarily the reason for the stop, but any violations the officer decided to persecute or record while in the course of the contact with the driver. The violation list is recorded as a string in the SOPP data separated by "|." I identify immigration violations as any strings from the violation covariate including the following character patterns: "immigrat," "smuggling of persons." This string pattern identifies stops that include smuggling of persons violations and violations where the driver has been identified as having misdemeanor or felony Federal immigration warrants. I also use driver's license violations as a proxy measure for identifying potential undocumented immigrants. Since Texas does not offer undocumented immigrants driver's licenses and the absence of government identification is often used as a basis for suspecting illegality by police (Armenta, 2017a), we may expect a discontinuous increase in driver's license violations if the goal of OSS is to identify undocumented immigrants for deportation.

[17]It is important to note DPS officers are often evaluated on the basis of providing more citations relative to warnings, so estimates of the effects of OSS on warning rates may be downwardly biased (McCollum and Wright, 2020).

threshold for initializing stop-and-searches. Each stop-and-search in the data includes indicators for whether the stop-and-search is a consent search, probable cause search, or search incident to arrest. I evaluate the effect of OSS on the consent search rate. Consent searches are when an officer asks to search a vehicle, and the driver allows a search. Consent searches are likely not legally justifiable under reasonable suspicion or probable cause. Consent searches are known to be subject to abuse given most drivers typically say yes to the police even though they cannot legally search their car (Baumgartner, Epp, and Shoub, 2018).

Estimation Strategy

To estimate the effect of Operation Strong Safety on police behavior, I use a regression discontinuity-in-time approach:[18]

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

For Equation (1) Y_i is an indicator of whether stop (or search) i resulted in a warning (or contraband hit). α is the intercept. OSS_i is an indicator for whether stop i occurs after OSS is in effect (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 $1 \ldots 4$, that is, from degree $= 0 \ldots 3$. ε_i are heteroskedastic robust errors.[19] Since the estimation uses date-level data, it increases the plausibility of the continuity identifying assumption, that is, omitted covariates such as criminal activity and driver characteristics at the border are not discontinuously changing at the point OSS is implemented. If OSS increases/decreases warning/hit rates, consistent with expectations, then τ will be positive/negative.

[18]An alternative design might use a difference-in-differences framework where warning and hit rates are evaluated differentially between Hidalgo and Starr county versus other counties. I do not use this approach for two reasons. First, outcome trends are not parallel between border and non-border regions, suggesting the existence of time-varying confounders across treated and non-treated regions (Figure 2). Second, the possibility of bias from SUTVA violations given OSS resulted in a redistribution of policing effort (Figure 1). One might use synthetic control methodologies to compensate for the absence of an adequate counterfactual from the given data, but I find that re-weighting units to garner parallel pre-trends is impossible given the scale of the pre-trend violations (and it still does not resolve the SUTVA problem).

[19]When degree $= 0$, τ is a simple difference-in-means.

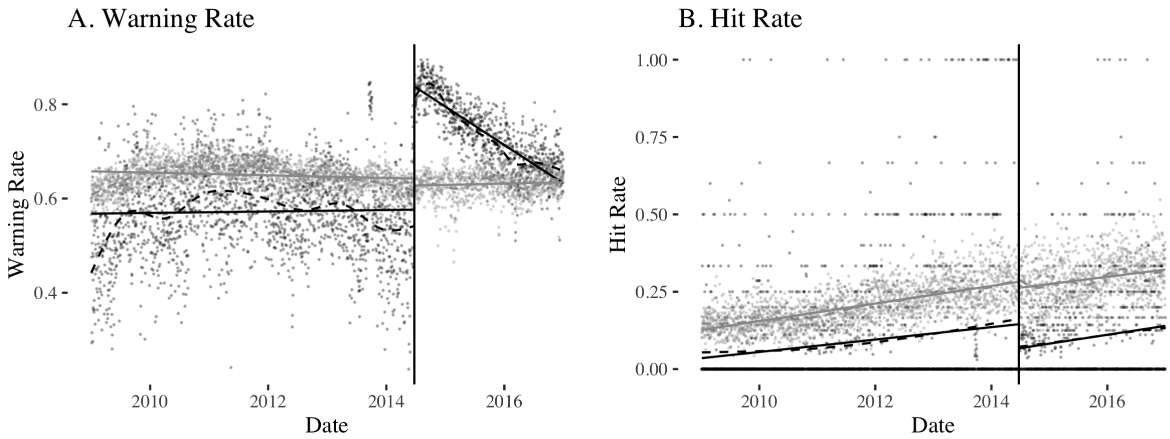

Figure 2: Warning rates (Panel A, y-axis) and hit rates (Panel B, y-axis) over time (x-axis). Solid and dashed lines characterize linear and loess models fit on each side of the discontinuity. Black lines denote stops in Hidalgo/Starr. Grey lines denote stops outside of Hidalgo/Starr. The vertical line denotes when Operation Strong Safety is implemented.

I display two sets of OSS effect estimates in the main text. The first set uses all stop (or search) data, with and without adjusting for year, month, and day-of-week fixed effects to account for outcome seasonality (Hausman and Rapson, 2018). The second set uses data from a series of narrow bandwidths before and after OSS is initialized (10-200 days) (Imbens and Lemieux, 2008).[20] I estimate Equation (1) using data from Hidalgo/Starr and data from the rest of Texas as both a geographic placebo check and to account for potential spillover effects (e.g. a reduction in search effort in non-treated areas) (Hausman and Rapson, 2018).[21]

Results

Figure 2 displays warning and hit rates over time (Panels A and B respectively). Both plots include loess and linear models fit to each side of the date Operation Strong Safety is implemented. Consistent with expectations, OSS appears to discontinuously increase the warning stop rate and decrease the stop-and-search hit rate in Hidalgo and Starr counties,

[20]The main results are unchanged conditional on using various permutations of the data-driven mean-squared error optimal bandwidth approach (Calonico et al., 2017) (Appendix Section I, Table I).

[21]I also assess heterogenous treatment effects of OSS conditional on the border region. Consistent with the main results, the heterogenous effect estimates suggest OSS discontinuously increases warning stops and decreases the hit rate conditional on the stops occurring in the Hidalgo/Starr region (Appendix Section H, Tables H.5 and H.6, Figures H.6 and H.7).

Table 1: Discontinuous Effect of Operation Strong Safety on Warning Stop Rates

	Warning Stop Rate (0 = Citation)							
Panel A: H/S	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.16***	0.24***	0.32***	0.30***	0.30***	0.31***	0.34***	0.30***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	981046	981046	981046	981046	981046	981046	981046	981046
Pre-OSS μ	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48
Panel B: Non-H/S	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	−0.02***	−0.02***	0.02***	−0.00**	0.00*	0.00	0.01***	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	16589141	16589141	16589141	16589141	16589141	16589141	16589141	16589141
Pre-OSS μ	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Panel A (H/S) characterizes RD estimates using stop data from area of operations for Operation Strong Safety, Hidalgo and Starr county. Panel B (non-H/S) characterizes RD estimates using stop data from all other counties of Texas. Models with controls adjust for year, month, and day-of-year fixed effects. Robust standard errors in parentheses.

but have no effect on the relevant outcomes throughout the rest of Texas.

Table 1 displays treatment effect estimates of OSS on the warning rate using Equation (1) across a variety of specifications with and without controls and using different degrees for the running variable. Panel A displays estimates using all stop data from Hidalgo/Starr and Panel B displays estimates using all stop data from the rest of Texas. OSS significantly increased the warning rate in Hidalgo/Starr, with effect estimates ranging from 16-34 percentage points relative to a pre-OSS mean baseline of 48 percentage points (between 30-70% of the pre-OSS mean). Conversely, outside Hidalgo/Starr, OSS had a limited and sign-indeterminate effect on warning rates, ranging from -2 to 2 percentage points.[22] These estimates are robust to using smaller bandwidths of data, demonstrating a clear increase in the warning rate as a result of OSS in Hidalgo/Starr, and small or null effects for the rest of Texas (Figure 6).

Table 2 displays treatment effect estimates of OSS on the hit rate across a variety of specifications using all stop data from Hidalgo/Starr and the rest of Texas. OSS appears to decrease the hit rate for weapons and drugs between 5-9 percentage points relative to a pre-OSS baseline of 7 percentage points in Hidalgo/Starr (equivalent to 70-130% of the

[22]The high levels of statistical significance for some estimates despite small substantive effects are due to the large number of stops occurring outside Starr/Hidalgo.

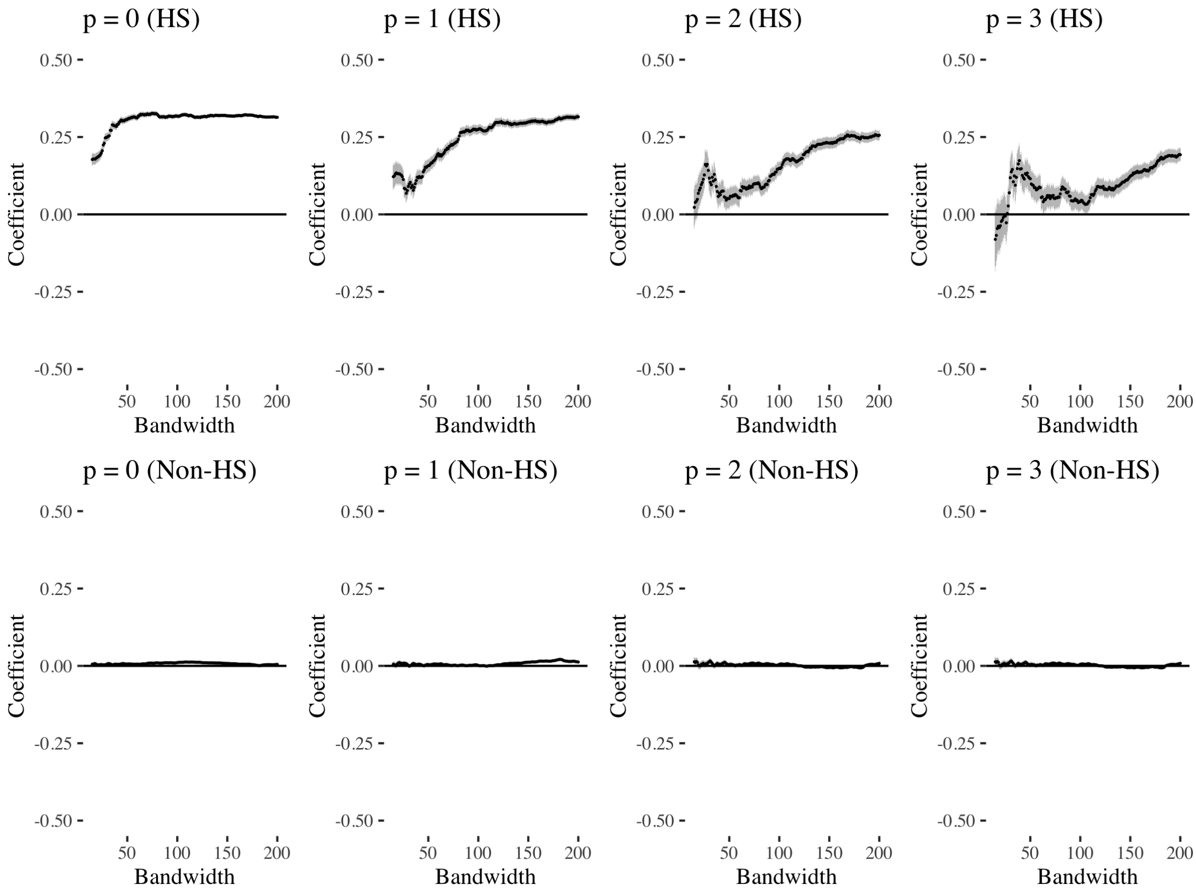

Figure 3: Effect of Operation Strong Safety (y-axis) on warning stop rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \dots 3$). Top 4 panels display effects using stop data from Hidalgo and Starr. Bottom 4 panels display effects using data from the rest of Texas.

pre-OSS mean).[23] Figure 4 displays the effect of OSS on the hit rate between 10-200 day bandwidths. Consistent with the estimates using all the data, the hit rate discontinuously decreases after OSS in Hidalgo/Starr, but there is no effect throughout the rest of Texas.

Reducing the threshold for stops

I will now describe a series of tests that suggest OSS reduced the threshold for initializing a stop. I first evaluate the effect of OSS on the rate of two different kinds of speeding stops. The first speeding stop is a "discretionary" speeding stop, that is, where the driver

[23]With the exception of Model 1 on Panel A, which uses a difference-in-means approach without accounting for year, month, and day-of-week fixed effects. This is because the hit rate is improving over time outside of the influence of OSS on both sides of the discontinuity (Figure 2, Panel B). The inclusion of temporal fixed effects in Model 5 on Panel A adjusts for the global upward trend and produces a negative coefficient similar to the rest of the estimates.

Table 2: Discontinuous Effect of Operation Strong Safety on Hit Rates

	Hit Rate							
Panel A: H/S	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.02^{***}	-0.05^{***}	-0.07^{***}	-0.05^{*}	-0.08^{***}	-0.08^{***}	-0.09^{***}	-0.07^{*}
	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
N	16191	16191	16191	16191	16191	16191	16191	16191
Pre-OSS μ	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Panel B: Non-H/S	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.10^{***}	-0.02^{***}	-0.03^{***}	0.00	-0.01	-0.01	-0.00	0.02^{*}
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	303406	303406	303406	303406	303406	303406	303406	303406
Pre-OSS μ	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Panel A (border) characterizes RD estimates using stop-and-search data from area of operations for Operation Strong Safety, Hidalgo and Starr county. Panel B (non-border) characterizes RD estimates using stop-and-search data from all other counties of Texas. Models with controls adjust for year, month, and day-of-year fixed effects. Robust standard errors in parentheses.

was above the speed limit but less than 10% of the limit. In this case, the driver cannot be ticketed, but can only receive a warning. The second speeding stop is a "ticketable" speeding stop. In this case, the driver drove over 10% of the speed limit and should receive a ticket. Indeed, discretionary speeding stops receive a warning (as opposed to a citation) 90% of the time. Conversely, ticketable speeding stops receive a warning around 3% of the time (Appendix Section O, Table O.12). If the threshold for stops is lower, then we should observe a discontinuous increase in discretionary speed stops in Hidalgo/Starr and a discontinuous decrease in ticketable speed stops in Hidalgo/Starr. Table O.13 indicates OSS increased the rate of discretionary speed stops between 3-8 percentage points in Hidalgo/Starr with no comparable increase in the rest of Texas. Conversely, Table O.14 demonstrates OSS decreased the rate of ticketable speed stops between 8-18 percentage points with no comparable effect in the rest of Texas.

I then demonstrate the threshold for initializing a stop-and-search has decreased after Operation Strong Safety. Table 3 demonstrates OSS discontinuously increased the search rate between 0.2-1.2 percentage points. These coefficients may seem small, but they are equivalent to 10-63% of the pre-OSS mean. Conversely, consistent with the decrease in stop effort outside of Hidalgo/Starr displayed on Figure 1, there is a decrease in the search

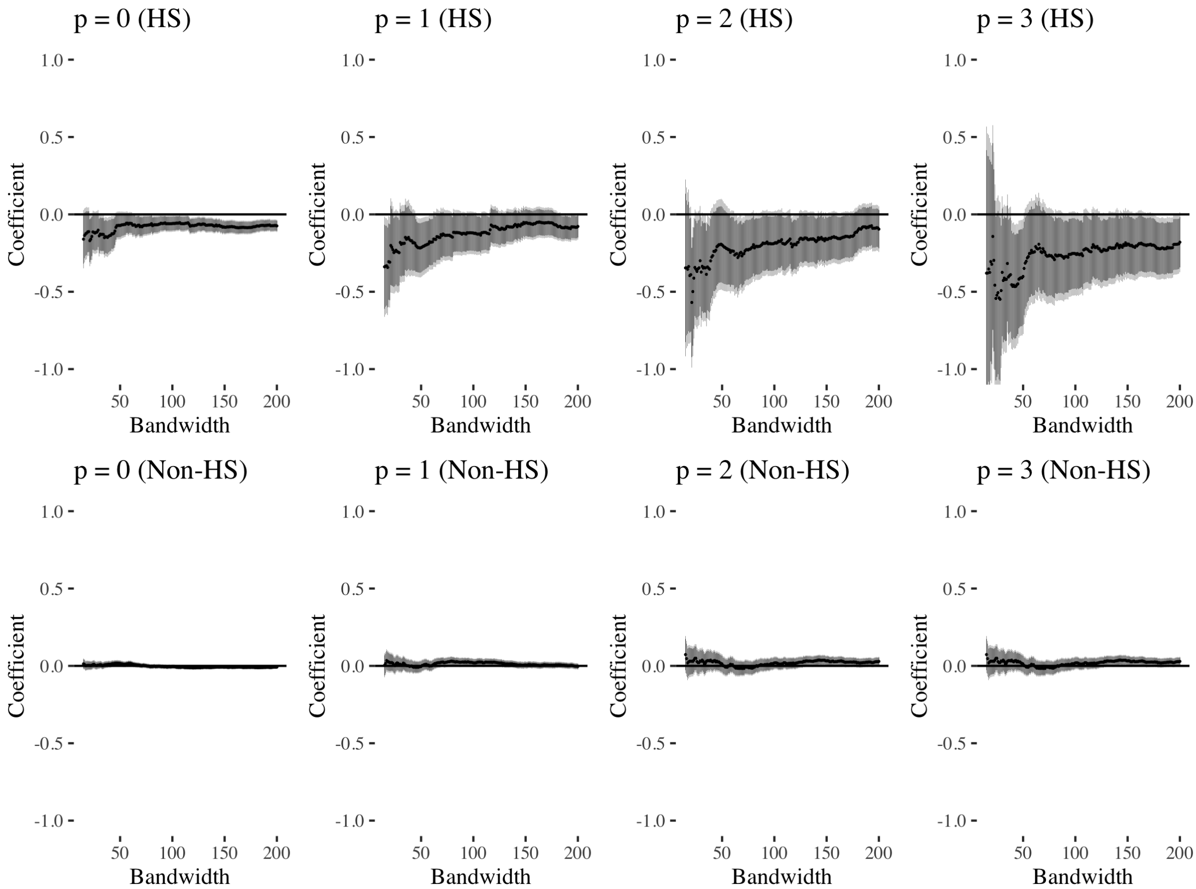

Figure 4: Effect of Operation Strong Safety (y-axis) on contraband hit rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$). Top 4 panels display effects using stop-and-search data from Hidalgo and Starr. Bottom 4 panels display effects using data from the rest of Texas.

rate between 0.1-0.4 percentage points relative to a 1.9 percentage point pre-OSS mean baseline. However, the search rate findings are somewhat indeterminate using data near the discontinuity after specifying a quadratic or cubic fit for the running variable (Figure 6).

Moreover, Figure 6 demonstrates the hit *count* over time relative to the search count over time in Hidalgo/Starr. There is a clear increase in the number of searches, from nearly 0 to 15, but a limited increase in the number of hits (around 1) after OSS is implemented. I aggregate the hit and search count data from Hidalgo/Starr to the daily level and assess the discontinuous effects of OSS on hit and search counts (Appendix Section F, Table F.3). I find OSS increased the number of hits by 0.25-0.68, but increased the number of searches by 7-13. These findings suggest the threshold for initializing a search are lower

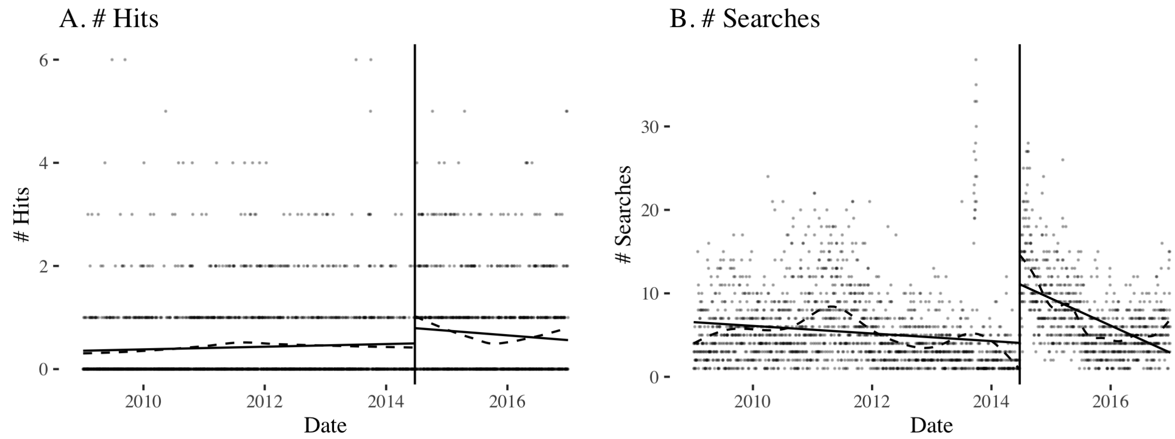

Figure 5: The number of searches (Panel A, y-axis) and hits (Panel B, y-axis) over time (x-axis) in the Hidalgo/Starr border region. The solid vertical line denotes the initialization of Operation Strong Safety. The solid and dashed black lines denote linear and loess models fit on each side of the discontinuity respectively.

Table 3: Discontinuous Effect of Operation Strong Safety on Search Rates

	Search Rate							
Panel A: HS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.002^{***}	0.002^{***}	0.013^{***}	0.013^{***}	0.008^{***}	0.008^{***}	0.012^{***}	0.013^{***}
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	981046	981046	981046	981046	981046	981046	981046	981046
Pre-OSS μ	0.0193	0.0193	0.0193	0.0193	0.0193	0.0193	0.0193	0.0193
Panel B: Non-HS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.003^{***}	-0.004^{***}	-0.001^{***}	-0.001^{***}	-0.001^{***}	-0.002^{***}	-0.001^{***}	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	16589141	16589141	16589141	16589141	16589141	16589141	16589141	16589141
Pre-OSS μ	0.0191	0.0191	0.0191	0.0191	0.0191	0.0191	0.0191	0.0191
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Panel A (border) characterizes RD estimates using stop data from area of operations for Operation Strong Safety, Hidalgo and Starr county. Panel B (non-border) characterizes RD estimates using stop data from all other counties of Texas. Models with controls adjust for year, month, and day-of-year fixed effects. Robust standard errors in parentheses.

among DPS troopers after OSS (Mummolo, 2018).

I then demonstrate that the rate of consent searches discontinuously increases after OSS, as opposed to probable cause or searches incident to arrest, which indicate a higher legal standard has been met for justifying a search. Table 4 indicates, among stop-and-searches, the rate of consent searches increased between 6-25 percentage points in Hidalgo/Starr relative to a 64 percentage point baseline, equivalent to 9-39% of the pre-OSS mean (Panel A). Outside of Hidalgo/Starr (Panel B), the effect of OSS is indeterminate and null with

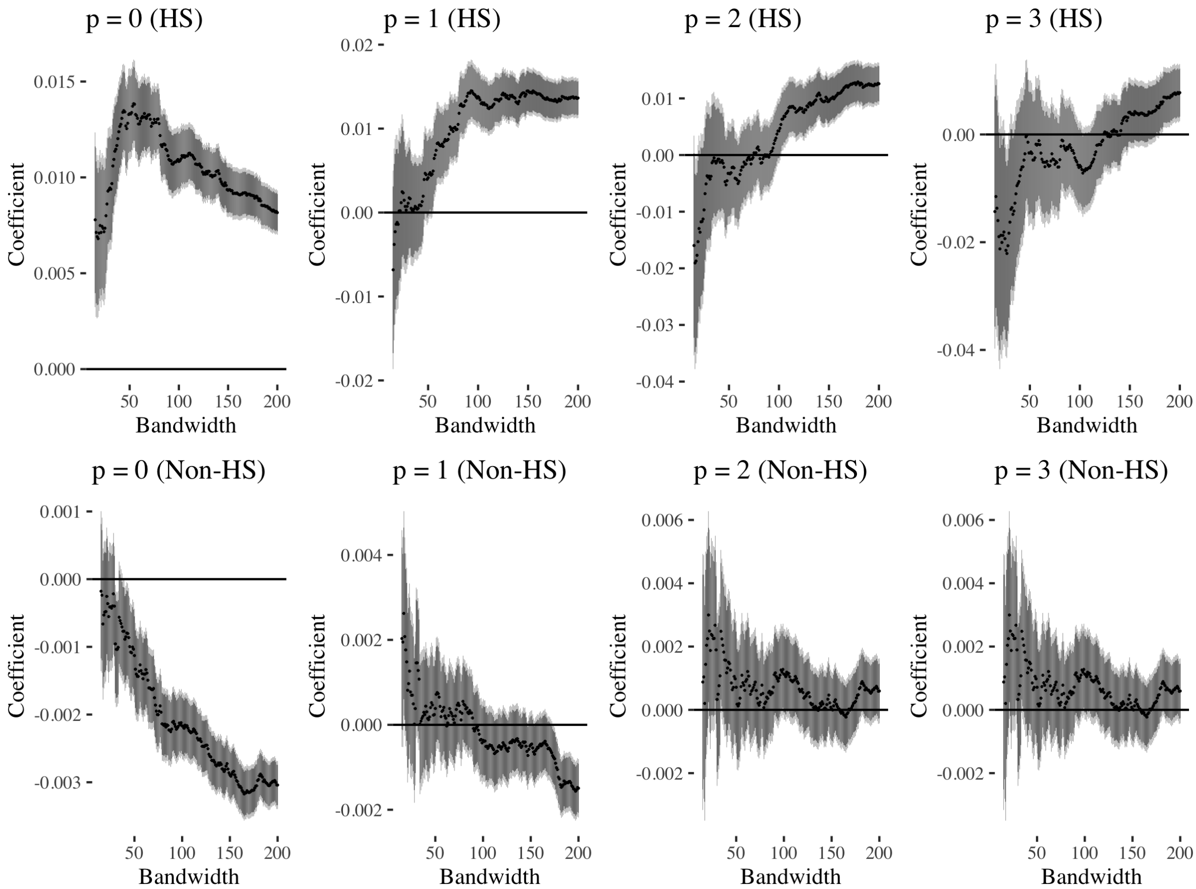

Figure 6: Effect of Operation Strong Safety (y-axis) on search rate using different bandwidths (x-axis, 10-100 days) and degrees (by panel e.g. $p = 0 \ldots 3$). Top 4 panels display effects using stop data from Hidalgo and Starr. Bottom 4 panels display effects using data from the rest of Texas.

Table 4: Discontinuous Effect of Operation Strong Safety on Consent Search Rates

	Consent Search Rate (Probable Cause = 0)							
Panel A: HS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.06^{***}	0.08^{***}	0.27^{***}	0.20^{***}	0.20^{***}	0.21^{***}	0.25^{***}	0.19^{***}
	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
N	16191	16191	16191	16191	16191	16191	16191	16191
Pre-OSS μ	0.64	0.64	0.64	0.64	0.64	0.64	0.64	0.64
Panel B: Non-HS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.15^{***}	-0.02^{***}	0.02^{**}	-0.02^{*}	-0.01	-0.01	0.00	-0.02
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	303406	303406	303406	303406	303406	303406	303406	303406
Pre-OSS μ	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Panel A (border) characterizes RD estimates using stop-and-search data from area of operations for Operation Strong Safety, Hidalgo and Starr county. Panel B (non-border) characterizes RD estimates using stop-and-search data from all other counties of Texas. Models with controls adjust for year, month, and day-of-year fixed effects. Robust standard errors in parentheses.

the inclusion of temporal fixed effects.

What about undocumented immigration?

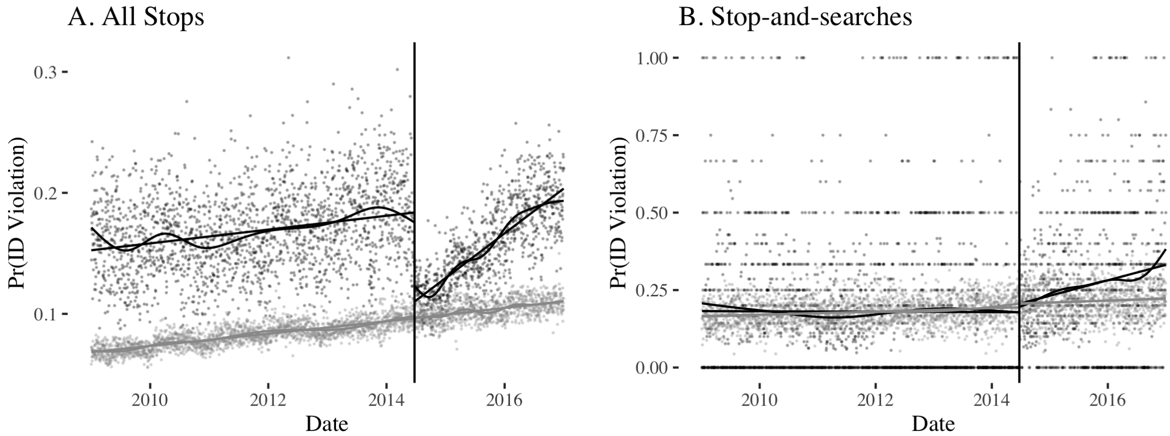

Figure 7: Driver's license violations (y-axis) over time (day, x-axis) by border (black dots, lines) and non-border (grey dots, lines) region.

The stop data include violation measures that can help identify immigration-related violations, namely, whether the driver has been found to have Federal immigration warrants or was involved in human smuggling. The data suggest OSS, and the DPS writ large, rarely identify and prosecute immigration violations. In the Hidalgo/Starr region, the proportion of all stops involving immigration violations is 0.00011 before Operation Strong Safety. After, it is 0.00013. This is a minuscule 0.00002 increase and is the difference between 52 (out of 507999) and 65 (out of 473047) stops. Likewise, for stop-and-searches in Hidalgo/Starr, the proportion of stops involving immigration violations is 0.0009 and 0.001 before and after OSS respectively. Again, the difference is very small, between 9 (out of 9805) and 9 (out of 6368) stop-and-searches.

I also evaluate whether OSS increased the identification of undocumented immigrants by DPS troopers by using a proxy measure for undocumented status, the rate of stops resulting in driver's license violations. Given Texas does not provide driver's licenses to undocumented immigrants and the absence of ID is often understood as suspected illegality by police officers, this outcome may provide insight into whether the DPS is efficiently identifying undocumented immigrants as a result of OSS. Figure 7 demonstrates

OSS discontinuously decreased the rate of ID violation stops and did not change the rate of stop-and-searches that had ID violations. In tandem, these results suggest if the DPS was attempting to enforce Federal immigration law, they are not doing it effectively, and may be unnecessarily stopping drivers in the meantime.

Robustness Checks

I assess the sensitivity of the main estimates to increase confidence in the results. I adjust for day-to-day weather and the prior days hit rate to account for additional sources of bias and autoregressive processes (Hausman and Kronick, 2019). I derive results similar to the main estimates (Appendix Section L, Table L.8 and Figures L.17 and L.18. Appendix Section M, Table M.10, Figures M.20, and M.21). Although there is likely limited risk of anticipatory effects given the short timeframe between the announcement of the policy and its implementation, I evaluate if outcomes are trending in a direction consistent with the expectations of the theory (Hausman and Kronick, 2019). Warning rates appear to be upwardly increasing, raising concerns over anticipatory effects. Conversely, hit rates trend in the opposite direction of expectations (Appendix Section K.1). I estimate a "donut" RD, removing observations 1-50 days from the OSS cutpoint. For both warning and hit rate outcomes, the estimates are similar to the main results, suggesting limited concerns regarding anticipatory effects (Appendix Sections K.2 and K.3). I also generate a series of placebo coefficients estimating the discontinuous effect of days prior to OSS while censoring all post-OSS data. Across a variety of specifications, the main estimates are usually statistically larger than the distribution of placebo coefficients (Appendix Section J).

I rule out whether OSS generated spillover effects by shifting drug trafficking to surrounding counties. If this is true, the hit rate for drug trafficking may have increased in counties neighboring Starr and Hidalgo, which did not experience a surge in DPS highway patrol officers. I estimate the effect of OSS in "support" counties, that is, counties that the DPS officially indicated they were drawing on trooper resources from near the border. I also estimate the effect of OSS in all counties that have entry points between the U.S. and

Mexico, which may be more prone to drug trafficking. Using all the stop-and-search data and data near the OSS cutpoint, the hit rate does not appear to change discontinuously after OSS in counties that are likely to experience spillover effects (Appendix Section G, Table G.4 and Figure G.5).

I rule out whether OSS increased unwarranted stops (or searches) because of an influx of officers inexperienced with operating in the border region. To do this, I aggregate the data to the officer-level and derive the mean proportion of daily stops that occur in Hidalgo/Starr prior to OSS for each officer. I exclude all officers with 0 stop experience in Hidalgo/Starr prior to OSS and assess the effects of OSS on DPS troopers who have spent 50% or 90% of their average daily stops in Hidalgo/Starr prior to OSS. If the experience mechanism is true, then we should expect attenuated or null effects among the most experienced DPS troopers with respect to warning and hit rates.[24] I still find OSS discontinuously increased/decreased warning/hit rates among border veterans. Although the coefficients for warning rates are smaller than the main estimates on Table 1, they are larger for hit rates relative to the main estimates on Table 2 (Appendix Section P, Tables P.15 and P.16). I also evaluate whether OSS decreased crime rates in Hidalgo/Starr. Using county-level UCR data on property and violent crime and a generalized synthetic control method developed by Xu (2017), I do not find evidence OSS decreased crime in the region (Appendix Section N, Table N.2).

Discussion and Conclusion

Consistent with the underclass theory of bureaucracy, the concerns of legal scholars, and qualitative accounts of over-policing, Operation Strong Safety increased the rate of warrantless and inefficient policing in the predominantly Latinx Hidalgo and Starr counties. Prior research identifies multiple incentives on part of street-level bureaucrats to incorporate immigrant communities. For local police, unnecessary contact that may result in alienation on part of the community may be antithetical to efficiency, effectiveness, and

[24] I cannot estimate the effects of OSS for officers with limited or 0 experience in Hidalgo/Starr prior to OSS given these officers do not have statistical support prior to the implementation of OSS.

community trust. However, consistent with prior research on the power of unequivocal top-down directives, the findings outlined in this paper suggest these incentives are insufficient to encourage immigrant incorporation among local police in the presence of politicized, unequivocal, and well-funded mandates to engage in immigration enforcement. In sum, this paper demonstrates immigrant incorporation is not preordained, but subject to the political contexts bureaucracies are situated in.

Additionally, these findings provide credible evidence of the effect of immigration policing on police behavior. Previous studies on the effects of immigration policing, although principled, may still suffer from intervening factors or unobserved time-varying covariates. This study, with a design evaluating immediate effects using high frequency data in a context where the policy was unequivocal, circumvents issues with prior research designs.

Future research should assess the conditions by which immigration policing directives affect policing practices. Evaluating the effect of Operation Strong Safety is ideal since it is an unequivocal mandate. Immigrant incorporation on part of street-level bureaucrats is increasingly difficult in a politicized context where multiple principals seek the same goal. However, many immigration directives lack clarity in terms of how they motivate officer behavior. Modern §287(g) agreements and Secure Communities do not directly implicate the behavior of street-level police patrols. It is possible individual police officers may increase the degree to which they stop immigrants if they understand their county jail has an agreement with ICE, but this is unclear, and may be conditional on whether departmental or political superiors clarify immigration enforcement is an important goal of the agency. Future studies should attempt to aggregate contemporary traffic stop data to evaluate the effect of less equivocal policies on disproportionate stop rates against Latinxs.

Additionally, future research should move to assess if immigration policing results in anti-Latinx discrimination. Given the import of the question, this would be difficult to credibly implement. I do not assess differences in hit or warning rates between whites and Latinxs in this paper since 93% of all stops (and searches) in Hidalgo/Starr, the treated areas, are Latinx. Moreover, there is insufficient statistical power to make group comparisons in the context of hit rates.[25] Future research should assess immigration policing in regions where Latinxs are less prominent to more credibly determine if immigration policing is discriminatory net of geographic concentration.

In summary, in the context of Operation Strong Safety, unequivocal mandates to increase the scope of policing to include immigration enforcement increases unnecessary stops in predominantly poor and Latinx communities. Even worse, these policing practices are ineffective in preventing crime and resolving ancillary immigration enforcement goals, such as identifying human smugglers and undocumented immigrants. These findings suggest the scope of policing activity should be reduced in order to ensure optimal societal outcomes.

[25]Although the discontinuous increase in warning rates is stronger conditional on whether the driver is Latinx. However, white drivers also experience an increase in warning rates in Hidalgo/Starr.

References

Aguilar, Julián (June 19, 2014). *DPS Addresses New Border Operation*. The Texas Tribune. URL: https://www.texastribune.org/2014/06/19/states-leadership-instructs-dps-increase-patrols-b/ (visited on 09/07/2020).

— (May 26, 2015). *Border Security Bill Passes Senate*. The Texas Tribune. URL: https://www.texastribune.org/2015/05/26/border-security-bill-passes-senate-final-approval-/ (visited on 09/07/2020).

Alsan, Marcella and Crystal Yang (2018). *Fear and the safety net: Evidence from secure communities*. National Bureau of Economic Research.

Altman, Alex and Zeke Miller (2014). *Rick Perry Prepares For 2016*. Time. URL: https://time.com/2902474/rick-perry-president-2016/ (visited on 09/07/2020).

American Immigration Council (2011). *Secure Communities: A Fact Sheet*.

— (2012). *The 287(g) Program: An Overview*. American Immigration Council. URL: https://www.americanimmigrationcouncil.org/research/287g-program-immigration (visited on 08/31/2020).

Armenta, Amada (2017a). *Protect, serve, and deport: The rise of policing as immigration enforcement*. University of California Press Oakland.

— (2017b). "Racializing crimmigration: Structural racism, colorblindness, and the institutional production of immigrant criminality". In: *Sociology of race and ethnicity* 3.1. Publisher: SAGE Publications Sage CA: Los Angeles, CA, pp. 82–95.

Arnold, Carrie L. (2007). "Racial profiling in immigration enforcement: State and local agreements to enforce federal immigration law". In: *Ariz. L. Rev.* 49. Publisher: HeinOnline, p. 113.

Associated Press (June 14, 2018). *ACLU says Texas is racial profiling by sharing information with ICE*. KEYE. URL: https://cbsaustin.com/news/local/aclu-says-texas-is-racial-profiling-by-sharing-information-with-ice (visited on 09/07/2020).

Balch, Robert W. (1972). "The police personality: fact or fiction?" In: *The journal of criminal Law, criminology, and police science* 63.1. Publisher: JSTOR, pp. 106–119.

Baumgartner, Frank R., Derek A. Epp, and Kelsey Shoub (2018). *Suspect citizens: What 20 million traffic stops tell us about policing and race*. Cambridge University Press.

Benen, Steve (July 22, 2014). *Rick Perry's 'Operation Strong Safety'*. MSNBC. URL: http://www.msnbc.com/rachel-maddow-show/rick-perrys-operation-strong-safety (visited on 09/07/2020).

Benning, Tom (Sept. 12, 2016). *Border Patrol, DPS set aside past conflict, buddy up for 'team sport' of border security*. Dallas News. Section: Politics. URL: https://www.dallasnews.com/news/politics/2016/09/12/border-patrol-dps-set-aside-past-conflict-buddy-up-for-team-sport-of-border-security/ (visited on 09/06/2020).

Bohn, Sarah and Robert Santillano (2017). "Local immigration enforcement and local economies". In: *Industrial Relations: A Journal of Economy and Society* 56.2. Publisher: Wiley Online Library, pp. 236–262.

Bonanno, Emma Rose (2015). "An Evidential Review of Police Misconduct: Officer versus Organization". In:

Bosque, Melissa del (2018). *Life Inside Texas' Border Security Zone*. The Texas Observer. URL: https://www.texasobserver.org/the-surge/ (visited on 09/06/2020).

Brehm, John O. and Scott Gates (1999). *Working, shirking, and sabotage: Bureaucratic response to a democratic public.* University of Michigan Press.

Burch, Traci (2013). *Trading democracy for justice: Criminal convictions and the decline of neighborhood political participation.* University of Chicago press.

Burnett, John (2015). *Texas Created Its Own Border Patrol Police, But Is It Necessary?* NPR.org. URL: https://www.npr.org/2015/09/04/437596910/texas-created-its-own-border-patrol-police-but-is-it-necessary (visited on 09/07/2020).

Calonico, Sebastian et al. (2017). "rdrobust: Software for regression-discontinuity designs". In: *The Stata Journal* 17.2. Publisher: SAGE Publications Sage CA: Los Angeles, CA, pp. 372–404.

Carpenter, Daniel P. (1996). "Adaptive signal processing, hierarchy, and budgetary control in federal regulation". In: *American Political Science Review* 90.2. Publisher: Cambridge University Press, pp. 283–302.

CBP (2020). *Stash House Shut Down by Joint Enforcement Action | U.S. Customs and Border Protection.* URL: https://www.cbp.gov/newsroom/local-media-release/stash-house-shut-down-joint-enforcement-action (visited on 09/07/2020).

Christie, Gayre, Simon Petrie, and Perri Timmins (1996). "The effect of police education, training and socialisation on conservative attitudes". In: *Australian & New Zealand Journal of Criminology* 29.3. Publisher: SAGE Publications Sage UK: London, England, pp. 299–314.

Ciancio, Alberto (2017). "The impact of immigration policies on local enforcement, crime and policing efficiency". In:

Dee, Thomas S. and Mark Murphy (2020). "Vanished classmates: The effects of local immigration enforcement on school enrollment". In: *American Educational Research Journal* 57.2. Publisher: SAGE Publications Sage CA: Los Angeles, CA, pp. 694–727.

Department of Public Safety (2000). *Traffic Stop Data Report.*

— (Feb. 2015a). *Operation Strong Safety: Report to the 85th Texas Legislature and Office of the Governor.*

— (Mar. 10, 2015b). *TXDPS - March 10, 2015 Operation Strong Safety Report Highlights Criminal Activity, Vulnerabilities Along Border.* URL: https://www.dps.texas.gov/director_staff/media_and_communications/pr/2015/0310a (visited on 08/23/2020).

Dexheimer, Eric and Jeremy Schwartz (2018). *DPS contribution to border operation drug seizures was modest - News - Austin American-Statesman - Austin, TX.* URL: https://www.statesman.com/NEWS/20180418/DPS-contribution-to-border-operation-drug-seizures-was-modest (visited on 09/07/2020).

DPS (2015). *TXDPS - November 19, 2015 DPS: Texans Can Help Fight Crime, Terrorism Through Tips to State Website.* URL: https://www.dps.texas.gov/director_staff/media_and_communications/pr/2015/1119a (visited on 09/07/2020).

East, Chloe et al. (2018). "The labor market effects of immigration enforcement". In: Publisher: IZA Discussion Paper.

Feigenberg, Benjamin and Conrad Miller (2020). *Racial Disparities in Motor Vehicle Searches Cannot Be Justified by Efficiency.* National Bureau of Economic Research.

Gelman, Andrew, Jeffrey Fagan, and Alex Kiss (2007). "An analysis of the New York City police department's "stop-and-frisk" policy in the context of claims of racial bias". In: *Journal of the American statistical association* 102.479. Publisher: Taylor & Francis, pp. 813–823.

Gerber, Alan S. et al. (2017). "Does incarceration reduce voting? Evidence about the political consequences of spending time in prison". In: *The Journal of Politics* 79.4. Publisher: University of Chicago Press Chicago, IL, pp. 1130–1146.

Gill, Hannah et al. (2009). "Legal and social perspectives on local enforcement of immigration under the Section 287 (g) program". In:

Goel, Sharad, Justin M. Rao, and Ravi Shroff (2016). "Precinct or prejudice? Understanding racial disparities in New York City's stop-and-frisk policy". In: *The Annals of Applied Statistics* 10.1. Publisher: Institute of Mathematical Statistics, pp. 365–394.

Haas, Aaron (2011). "Profiling and Immigration". In: *Wash. & Lee J. Civil Rts. & Soc. Just.* 18. Publisher: HeinOnline, p. 3.

Hausman, Catherine and David S. Rapson (2018). "Regression discontinuity in time: Considerations for empirical applications". In: *Annual Review of Resource Economics* 10. Publisher: Annual Reviews, pp. 533–552.

Hausman, David and Dorothy Kronick (2019). "Policing Police". In: *Available at SSRN 3192908*.

Huber, John D. and Charles R. Shipan (2002). *Deliberate discretion?: The institutional foundations of bureaucratic autonomy*. Cambridge University Press.

Imbens, Guido W. and Thomas Lemieux (2008). "Regression discontinuity designs: A guide to practice". In: *Journal of econometrics* 142.2. Publisher: Elsevier, pp. 615–635.

Jacome, Elisa (2019). "The Effect of Immigration Enforcement on Crime Reporting: Evidence from Dallas". In: *Working paper*.

Johnson, Jeh (June 24, 2014). *Statement by Secretary of Homeland Security Jeh Johnson Before the House Committee on Homeland Security*. Department of Homeland Security. URL: https://www.dhs.gov/news/2014/06/24/statement-secretary-homeland-security-jeh-johnson-house-committee-homeland-security (visited on 09/08/2020).

Johnson, Kevin R. (2009). "How racial profiling in America became the law of the land: United States v. Brignoni-Ponce and Whren v. United States and the need for truly rebellious lawyering". In: *Geo. LJ* 98. Publisher: HeinOnline, p. 1005.

— (2015). "Doubling Down on Racial Discrimination: The Racially Disparate Impacts of Crime-Based Removals". In: *Case W. Res. L. Rev.* 66. Publisher: HeinOnline, p. 993.

Knox, Dean, Will Lowe, and Jonathan Mummolo (2019). "Administrative records mask racially biased policing". In: *American Political Science Review*. Publisher: Cambridge University Press, pp. 1–19.

Komisarchik, Mayya, Maya Sen, and Yamil Velez (2020). "The Political Consequences of Ethnically Targeted Incarceration: Evidence from Japanese-American Internment During WWII". In: *Working Paper*.

Kostandini, Genti, Elton Mykerezi, and Cesar Escalante (2014). "The impact of immigration enforcement on the US farming sector". In: *American Journal of Agricultural Economics* 96.1. Publisher: Oxford University Press, pp. 172–192.

Kuhn, Eroll (2020). "Does policy threat mobilise? 287 (g) and Latino voter registration in North Carolina and Florida". In: *Journal of Ethnic and Migration Studies*. Publisher: Taylor & Francis, pp. 1–20.

KXAN (2015). *Texas troopers ticketing Hispanic drivers as white*. KXAN Austin. URL: https://www.kxan.com/investigations/texas-troopers-ticketing-hispanic-drivers-as-white/ (visited on 09/03/2020).

Laniyonu, Ayobami (2019). "The Political Consequences of Policing: Evidence from New York City". In: *Political Behavior* 41.2. Publisher: Springer, pp. 527–558.

Lerman, Amy E. and Vesla Weaver (2014). "Staying out of sight? Concentrated policing and local political action". In: *The ANNALS of the American Academy of Political and Social Science* 651.1. Publisher: Sage Publications Sage CA: Los Angeles, CA, pp. 202–219.

Lewis, Paul G. and S. Karthick Ramakrishnan (July 1, 2007). "Police Practices in Immigrant-Destination Cities: Political Control or Bureaucratic Professionalism?" In: *Urban Affairs Review* 42.6. Publisher: SAGE Publications Inc, pp. 874–900. ISSN: 1078-0874. DOI: 10.1177/1078087407300752. URL: https://doi.org/10.1177/1078087407300752 (visited on 09/06/2020).

Lineberry, Robert L. (Dec. 1, 1975). "Equality, Public Policy and Public Services: The Underclass Hypothesis and the Limits to Equality". In: *Policy & Politics* 4.2, pp. 67–84. DOI: 10.1332/030557376783186650.

Lipsky, Michael (2010). *Street-level bureaucracy: Dilemmas of the individual in public service*. Russell Sage Foundation.

Luh, Elizabeth (2019). "Not So Black and White: Uncovering Racial Bias from Systematically Misreported Trooper Reports". In: *Available at SSRN 3357063*.

López, Nancy et al. (2018). "What's your "street race"? leveraging multidimensional measures of race and intersectionality for examining physical and mental health status among latinxs". In: *Sociology of Race and Ethnicity* 4.1. Publisher: SAGE Publications Sage CA: Los Angeles, CA, pp. 49–66.

Marrow, Helen (2011). *New destination dreaming: Immigration, race, and legal status in the rural American South*. Stanford University Press.

Marrow, Helen B. (2009). "Immigrant bureaucratic incorporation: The dual roles of professional missions and government policies". In: *American Sociological Review* 74.5. Publisher: Sage Publications Sage CA: Los Angeles, CA, pp. 756–776.

Maynard-Moody, Steven and Shannon Portillo (2010). "Street-level bureaucracy theory". In: *The Oxford handbook of American bureaucracy*.

McCollum, Donna and Jeff Wright (2020). *Attorneys: Illegal arrest, traffic stop quota systems 'widespread' among DPS*. URL: https://www.ktre.com/2020/05/28/attorneys-illegal-arrest-traffic-stop-quota-systems-widespread-among-dps/ (visited on 09/08/2020).

McCubbins, Mathew D., Roger G. Noll, and Barry R. Weingast (1987). "Administrative procedures as instruments of political control". In: *Journal of Law, Economics, & Organization* 3.2. Publisher: JSTOR, pp. 243–277.

McGaughy, Lauren (July 22, 2015). *On traffic stops, experts say comply first, complain later*. HoustonChronicle.com. Section: Houston. URL: https://www.houstonchronicle.com/news/houston-texas/houston/article/On-traffic-stops-experts-say-comply-first-6398263.php (visited on 09/07/2020).

Miller, Gary J. (2005). "The political evolution of principal-agent models". In: *Annu. Rev. Polit. Sci.* 8. Publisher: Annual Reviews, pp. 203–225.

Morin, Rich et al. (Jan. 11, 2017). *Comparing police views and public views*. Pew Research Center's Social & Demographic Trends Project. URL: https://www.pewsocialtrends.org/2017/01/11/police-views-public-views/ (visited on 09/07/2020).

Mummolo, Jonathan (2018). "Modern police tactics, police-citizen interactions, and the prospects for reform". In: *The Journal of Politics* 80.1. Publisher: University of Chicago Press Chicago, IL, pp. 1–15.

Nelsen, Aaron (Mar. 26, 2016). *Border surge fuels sense of safety for many, but also reduces income for small-town police*. ExpressNews.com. Section: News. URL: https:

//www.expressnews.com/news/local/article/Border-surge-fuels-sense-of-safety-for-many-but-7098451.php (visited on 09/08/2020).

Noe Bustamante, Luis, Lauren Mora, and Mark Lopez (Aug. 11, 2020). *Latinx Used by Just 3% of U.S. Hispanics. About One-in-Four Have Heard of It.* Pew Research Center's Hispanic Trends Project. URL: https://www.pewresearch.org/hispanic/2020/08/11/about-one-in-four-u-s-hispanics-have-heard-of-latinx-but-just-3-use-it/ (visited on 09/06/2020).

NOPD (2016). *NEW ORLEANS POLICE DEPARTMENT OPERATIONS MANUAL.* URL: http://nowcrj.org/wp-content/uploads/2016/02/NOPD-anti-bias-policing-policy-Feb-2016.pdf.

Nowrasteh, Alex (2018). *Do immigration enforcement programs reduce crime? Evidence from the 287 (g) program in North Carolina.* JSTOR.

Owens, Michael Leo and Hannah L. Walker (2018). "The civic voluntarism of "custodial citizens": involuntary criminal justice contact, associational life, and political participation". In: *Perspectives on Politics* 16.4. Publisher: Cambridge University Press, pp. 990–1013.

Paluck, Elizabeth Levy and Donald P. Green (2009). "Prejudice reduction: What works? A review and assessment of research and practice". In: *Annual review of psychology* 60. Publisher: Annual Reviews, pp. 339–367.

Persico, Nicola and Petra Todd (2006). "Generalising the hit rates test for racial bias in law enforcement, with an application to vehicle searches in Wichita". In: *The Economic Journal* 116.515. Publisher: Oxford University Press Oxford, UK, F351–F367.

Peyton, Kyle, Michael Sierra-Arévalo, and David G. Rand (2019). "A field experiment on community policing and police legitimacy". In: *Proceedings of the National Academy of Sciences* 116.40. Publisher: National Acad Sciences, pp. 19894–19898.

Pham, Huyen and Pham Hoang Van (2010). "Economic impact of local immigration regulation: an empirical analysis". In: *Immigr. & Nat'lity L. Rev.* 31. Publisher: HeinOnline, p. 687.

Pierson, Emma et al. (2020). "A large-scale analysis of racial disparities in police stops across the United States". In: *Nature human behaviour*. Publisher: Nature Publishing Group, pp. 1–10.

Rhodes, Scott D. et al. (2015). "The impact of local immigration enforcement policies on the health of immigrant Hispanics/Latinos in the United States". In: *American journal of public health* 105.2. Publisher: American Public Health Association, pp. 329–337.

Root, Jay (July 21, 2014). *Perry Sending Guard Troops to the Border.* The Texas Tribune. URL: https://www.texastribune.org/2014/07/21/perry-activate-1000-guard-troops/ (visited on 09/07/2020).

Rosenthal, Brian (Sept. 8, 2015). *What Texas' border surge means: Traffic stops.* HoustonChronicle.com. Section: Gray Matters. URL: https://www.houstonchronicle.com/local/gray-matters/article/A-close-up-lesson-on-police-work-along-the-6490668.php (visited on 09/08/2020).

Ruelas, Richard and Jason Pohl (2018). *Arizona Gov. Doug Ducey's Border Strike Force data raises questions.* URL: https://www.azcentral.com/story/news/politics/border-issues/2018/10/08/arizona-governor-doug-ducey-border-strike-force-data-raises-questions/1450126002/ (visited on 09/06/2020).

Rugh, Jacob S. and Matthew Hall (2016). "Deporting the American dream: Immigration enforcement and Latino foreclosures". In: *Sociological Science* 3, pp. 1053–1076.

Schladen, Marty (2015). *In Rio Grande Valley, officials question the reason for DPS stops*. El Paso Times. URL: https://www.elpasotimes.com/story/archives/2015/03/28/rio-grande-valley-officials-question-reason-dps-stops/73899258/ (visited on 09/07/2020).

— (2016). *DPS tickets, warnings spike in El Paso*. El Paso Times. URL: https://www.elpasotimes.com/story/news/2016/12/17/dps-tickets-warnings-spike-el-paso/94769084/ (visited on 09/08/2020).

Schwartz, Jeremy (2016a). *Border Patrol now says it is DPS partner, but disavows past operation*. Austin American-Statesman. URL: https://www.statesman.com/news/20160904/border-patrol-now-says-it-is-dps-partner-but-disavows-past-operation (visited on 09/08/2020).

— (2016b). *Officials: Border efforts had little impact on drug supply or price*. Austin American-Statesman. URL: https://www.statesman.com/news/20160924/officials-border-efforts-had-little-impact-on-drug-supply-or-price (visited on 09/07/2020).

Schwartz, Jeremy and Kiah Collier (2018). *DPS contribution to border operation drug seizures was modest - News - Austin American-Statesman - Austin, TX*. URL: https://webcache.googleusercontent.com/search?q=cache:v9MuZz4yyTEJ:https://www.statesman.com/NEWS/20180418/DPS-contribution-to-border-operation-drug-seizures-was-modest+&cd=1&hl=en&ct=clnk&gl=us (visited on 09/08/2020).

Sjoberg, Gideon, Richard A. Brymer, and Buford Farris (1966). "BUREAUCRACY AND LOWER CLASS". In: *Sociology and Social Research* 50.3. Publisher: UNIV SOUTHERN CALIF SOCIOL SOC RES UNIV PARK, LOS ANGELES, CA 90089-0032, pp. 325–337.

Skolnick, Jerome H. (2010). "A sketch of the policeman's working personality". In: *Race, ethnicity and policing: New and essential readings*. Publisher: New York University Press New York, NY, pp. 15–32.

Soss, Joe and Vesla Weaver (2017). "Police are our government: Politics, political science, and the policing of race–class subjugated communities". In: *Annual Review of Political Science* 20.

Theodore, Nik and Robert Habans (2016). "Policing immigrant communities: Latino perceptions of police involvement in immigration enforcement". In: *Journal of Ethnic and Migration Studies* 42.6. Publisher: Taylor & Francis, pp. 970–988.

Twersky-Glasner, Aviva (2005). "Police personality: What is it and why are they like that?" In: *Journal of Police and Criminal Psychology* 20.1. Publisher: Springer, pp. 56–67.

Ura, Alexa (Sept. 21, 2016). *DPS Changes in Wake of Sandra Bland Death in Dispute*. The Texas Tribune. URL: https://www.texastribune.org/2016/09/20/dps-training-after-sandra-bland-death/ (visited on 09/07/2020).

Vidales, Guadalupe, Kristen M. Day, and Michael Powe (2009). "Police and immigration enforcement". In: *Policing: An International Journal of Police Strategies & Management*. Publisher: Emerald Group Publishing Limited.

Walker, Hannah, Marcel Roman, and Matt Barreto (2019). "The Direct and Indirect Effects of Immigration Enforcement on Latino Political Engagement". In: *UCLA L. Rev.* 66. Publisher: HeinOnline, p. 1818.

— (2020). "The Ripple Effect: The Political Consequences of Proximal Contact with Immigration Enforcement". In: *Journal of Race, Ethnicity and Politics*. Publisher: Cambridge University Press, pp. 1–36.

Walker, Hannah L. (2014). "Extending the effects of the carceral state: Proximal contact, political participation, and race". In: *Political Research Quarterly* 67.4. Publisher: SAGE Publications Sage CA: Los Angeles, CA, pp. 809–822.

— (2020). "Targeted: The Mobilizing Effect of Perceptions of Unfair Policing Practices". In: *The Journal of Politics* 82.1. Publisher: The University of Chicago Press Chicago, IL, pp. 119–134.

Washington, District of Columbia 1100 Connecticut Ave NW Suite 1300B and Bradley Saacks (2015). *PolitiFact - Rick Perry says 74 percent drop in border apprehensions shows Texas surge worked*. @politifact. URL: https://www.politifact.com/factchecks/2015/jul/26/rick-perry/rick-perry-says-74-percent-drop-border-apprehensio/ (visited on 09/07/2020).

Weaver, Vesla M. and Amy E. Lerman (2010). "Political consequences of the carceral state". In: *American Political Science Review*. Publisher: JSTOR, pp. 817–833.

White, Ariel (2016). "When threat mobilizes: Immigration enforcement and Latino voter turnout". In: *Political Behavior* 38.2. Publisher: Springer, pp. 355–382.

— (2019a). "Family Matters? Voting Behavior in Households with Criminal Justice Contact". In: *American Political Science Review* 113.2. Publisher: Cambridge University Press, pp. 607–613.

— (2019b). "Misdemeanor Disenfranchisement? The demobilizing effects of brief jail spells on potential voters". In: *American Political Science Review* 113.2. Publisher: Cambridge University Press, pp. 311–324.

Williamson, Abigail Fisher (2018). *Welcoming New Americans?: Local Governments and Immigrant Incorporation*. University of Chicago Press.

Williamson, Vanessa, Kris-Stella Trump, and Katherine Levine Einstein (2018). "Black lives matter: evidence that police-caused deaths predict protest activity". In: *Perspectives on Politics* 16.2. Publisher: Cambridge University Press, pp. 400–415.

Wilson, James Q. (1978). *Varieties of police behavior: The Management of law and Order in eight communities, with a new preface by the author*. Harvard University Press.

Winders, Jamie (2012). "Seeing immigrants: Institutional visibility and immigrant incorporation in new immigrant destinations". In: *The ANNALS of the American Academy of Political and Social Science* 641.1. Publisher: Sage Publications Sage CA: Los Angeles, CA, pp. 58–78.

WOLA (2015). *3 Reasons Why the Massive Migration Spike of 2014 Has Faded*. BORDER FACTS: Separating Rhetoric from Reality. URL: https://borderfactcheck.tumblr.com/post/114663296473/3-reasons-why-the-massive-migration-spike-of-2014 (visited on 09/08/2020).

Xu, Yiqing (2017). "Generalized synthetic control method: Causal inference with interactive fixed effects models". In: *Political Analysis* 25.1. Publisher: Cambridge University Press, pp. 57–76.

Part

Appendix

Table of Contents

A Description of specifications

I run the following specifications throughout the paper to test the effect of Operation Strong Safety:

p = 0 (difference-in-means): $Y_i = \alpha + \tau OSS_i + \varepsilon_i$

p = 1 (linear): $Y_i = \alpha + \tau OSS_i + \beta_1 d_i + \beta_2 OSS_i * d_i + \varepsilon_i$

p = 2 (quadratic): $Y_i = \alpha + \tau OSS_i + \beta_1 d_i + \beta_2 (d_i)^2 + \beta_3 OSS_i * d_i + \beta_4 OSS_i * (d_i)^2 + \varepsilon_i$

p = 3 (cubic): $Y_i = \alpha + \tau OSS_i + \beta_1 d_i + \beta_2 (d_i)^2 + \beta_3 (d_i)^3 + \beta_4 OSS_i * d_i + \beta_5 OSS_i * (d_i)^2 + \beta_6 OSS_i * (d_i)^3 + \varepsilon_i$

B OSS area of operations

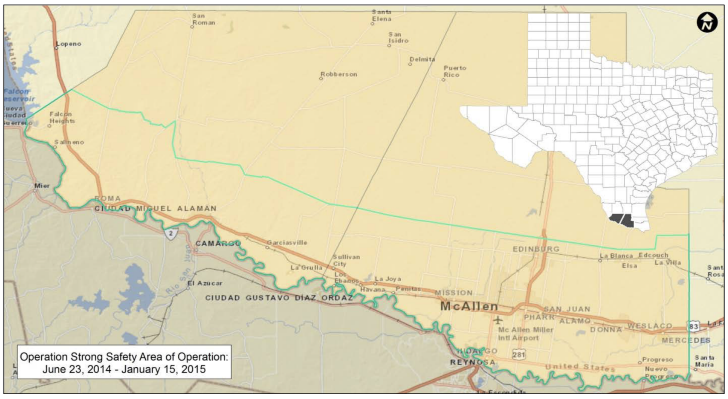

Figure B.1: DPS Operation Strong Safety area of operations

C DPS operational regions

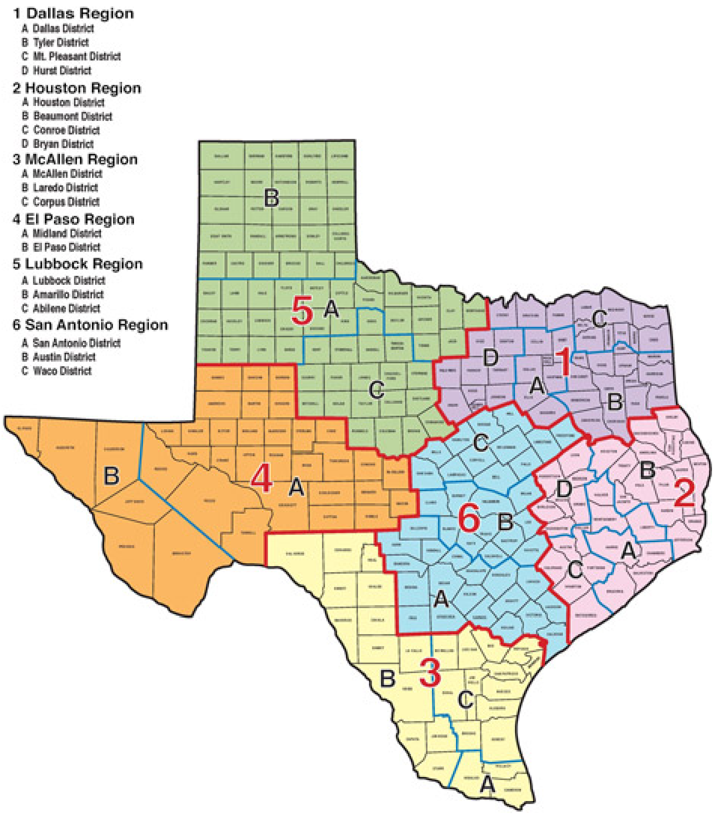

Figure C.2: DPS Highway Patrol regions

D Texas border security spending over time

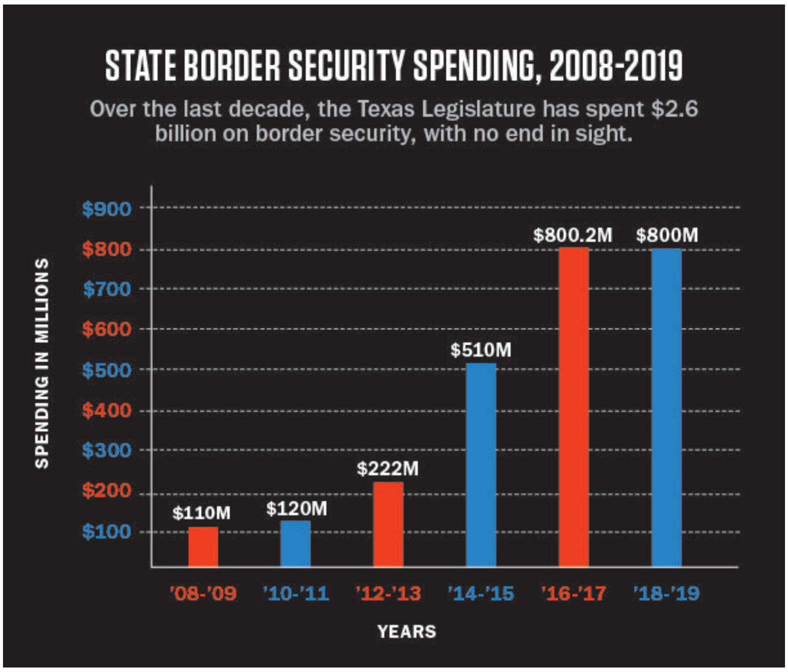

Figure D.3: Texas State Legislature spending (in millions, y-axis) on border security over time (x-axis) (2008-2019) (Source: Texas Observer).

E Understanding officer movement after OSS

E.1 Effect of OSS on where stops happen

Table E.1: Effect of Operation Strong Safety on Pr(stop location) at officer-level

Panel A: Pr(H/S)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.12^{***}	0.17^{***}	0.18^{***}	0.13^{***}	0.17^{***}	0.17^{***}	0.19^{***}	0.12^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Pr(Region 1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.04^{***}	-0.05^{***}	-0.04^{***}	-0.02^{***}	-0.04^{***}	-0.05^{***}	-0.05^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Panel C: Pr(Region 2)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.03^{***}	-0.04^{***}	-0.04^{***}	-0.02^{***}	-0.04^{***}	-0.04^{***}	-0.03^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel D: Pr(Region 3)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.01	-0.02^{***}	-0.02^{***}	-0.03^{***}	-0.02^{***}	-0.02^{***}	-0.02^{***}	-0.02^{***}
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel E: Pr(Region 4)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.01	-0.01	-0.03^{***}	-0.02^{***}	-0.02^{***}	-0.02^{***}	-0.04^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel F: Pr(Region 5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.03^{***}	-0.03^{***}	-0.02^{***}	-0.01^{*}	-0.02^{***}	-0.02^{***}	-0.02^{***}	-0.01^{**}
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel G: Pr(Region 6)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.03^{***}	-0.02^{***}	-0.02^{***}	-0.02^{***}	-0.03^{***}	-0.03^{***}	-0.03^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	2301034	2301034	2301034	2301034	2301034	2301034	2301034	2301034
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models use stop data aggregated at the officer/day level. The outcome is the probability of stops in a respective location (denoted by the panel labels). The locations are split by the treated region (Hidalgo/Starr, denoted as "H/S" on the table), and non-treated regions defined by Texas Highway Patrol regions (see C.2). Panel A is the Hidalgo/Starr region, Panel B is region 1, Panel C is region 2, Panel D is region 3 (excluding Hidalgo and Starr counties), Panel E is region 4, Panel F is region 5, Panel G is region 6. The reference location for Panel A is all other areas in Texas. The reference location for Panels B-G are all other areas in Texas as well. Robust standard errors clustered by officer in parentheses.

Table E.2: Effect of Operation Strong Safety on Pr(stop location) at officer-level (Hidalgo/Starr reference for region outcome estimates)

Panel A: Pr(Region 1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.29^{***}	-0.36^{***}	-0.38^{***}	-0.27^{***}	-0.37^{***}	-0.38^{***}	-0.40^{***}	-0.26^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	697283	697283	697283	697283	697283	697283	697283	697283
Panel B: Pr(Region 2)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.29^{***}	-0.37^{***}	-0.38^{***}	-0.29^{***}	-0.37^{***}	-0.38^{***}	-0.40^{***}	-0.26^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	659040	659040	659040	659040	659040	659040	659040	659040
Panel C: Pr(Region 3)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.33^{***}	-0.40^{***}	-0.43^{***}	-0.37^{***}	-0.43^{***}	-0.43^{***}	-0.44^{***}	-0.34^{***}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	422062	422062	422062	422062	422062	422062	422062	422062
Panel D: Pr(Region 4)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.28^{***}	-0.37^{***}	-0.44^{***}	-0.36^{***}	-0.43^{***}	-0.44^{***}	-0.50^{***}	-0.36^{***}
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
N	375404	375404	375404	375404	375404	375404	375404	375404
Panel E: Pr(Region 5)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.35^{***}	-0.40^{***}	-0.40^{***}	-0.30^{***}	-0.41^{***}	-0.41^{***}	-0.43^{***}	-0.30^{***}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	491548	491548	491548	491548	491548	491548	491548	491548
Panel F: Pr(Region 6)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.31^{***}	-0.36^{***}	-0.38^{***}	-0.30^{***}	-0.38^{***}	-0.39^{***}	-0.41^{***}	-0.29^{***}
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
N	591044	591044	591044	591044	591044	591044	591044	591044
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models use stop data aggregated at the officer/day level. The outcome is the probability of stops in a respective location (denoted by the panel labels). The locations are split by non-treated regions defined by Texas Highway Patrol regions (see C.2). Panel A is region 1, Panel B is region 2, Panel C is region 3 (excluding Hidalgo and Starr counties), Panel D is region 4, Panel E is region 5, Panel F is region 6. The reference location for Panels A-F is Hidalgo/Starr counties. Robust standard errors clustered by officer in parentheses.

E.2 Effect of OSS on number of officers at border

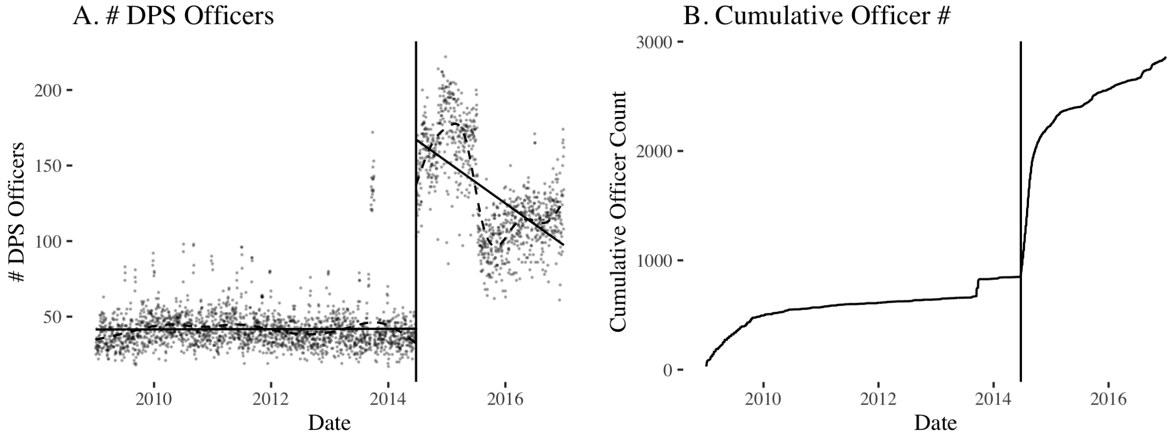

Figure E.4: Panel A displays the number of DPS officers (y-axis) operating in Hidalgo/Starr region over time (x-axis) at the daily level. Solid black line characterizes a linear model. Dashed black line characterizes a loess model. The vertical black line characterizes the point OSS is implemented. Panel B displays the cumulative number of DPS officers (y-axis) who have operated in the Hidalgo/Starr region over time (x-axis)

F OSS effects on hit and search counts in border

Table F.3: Effects of Operation Strong Safety on search and hit counts (border region)

	# Hits				# Searches			
OSS	0.25***	0.30***	0.67***	0.68***	7.03***	7.03***	12.62***	11.28***
	(0.03)	(0.07)	(0.11)	(0.14)	(0.35)	(0.35)	(0.51)	(0.68)
N	2741	2741	2741	2741	2741	2741	2741	2741
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models use data aggregated to the daily level from the stop dataset. The outcome for the first 4 models is the number of hit rates. The outcome for models 5-8 is the number of searches. All models adjust for year, month, and weekday fixed effects.

G Assessing spillover effects

G.1 Using all data

Table G.4: Effects of Operation Strong Safety in non-treated spillover border counties

A. Support only	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.05^{***}	0.01	-0.09^{**}	-0.01	0.00	0.01	0.01	0.09^{*}
	(0.01)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
N	8247	8247	8247	8247	8247	8247	8247	8247
B. All EP counties	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.02^{**}	-0.09^{***}	-0.04	0.09^{**}	0.01	0.02	0.01	0.09^{*}
	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.04)
N	15506	15506	15506	15506	15506	15506	15506	15506
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models use stop-and-search data only from non-treated border counties. Panel A uses data from Cameron, Webb, Zapata, Maverick and Val Verde. These are understood as "support" counties by the Department of Public Safety (Department of Public Safety, 2015). Panel B uses data from the counties used in the models for Panel B but with the inclusion of Presidio, El Paso, and Hudspeth counties, which have entry points from Mexico into Texas. Models 1-4 do not include temporal controls. Models 5-8 adjust for year, month, and weekday fixed effects.

G.2 Iterations near bandwidth

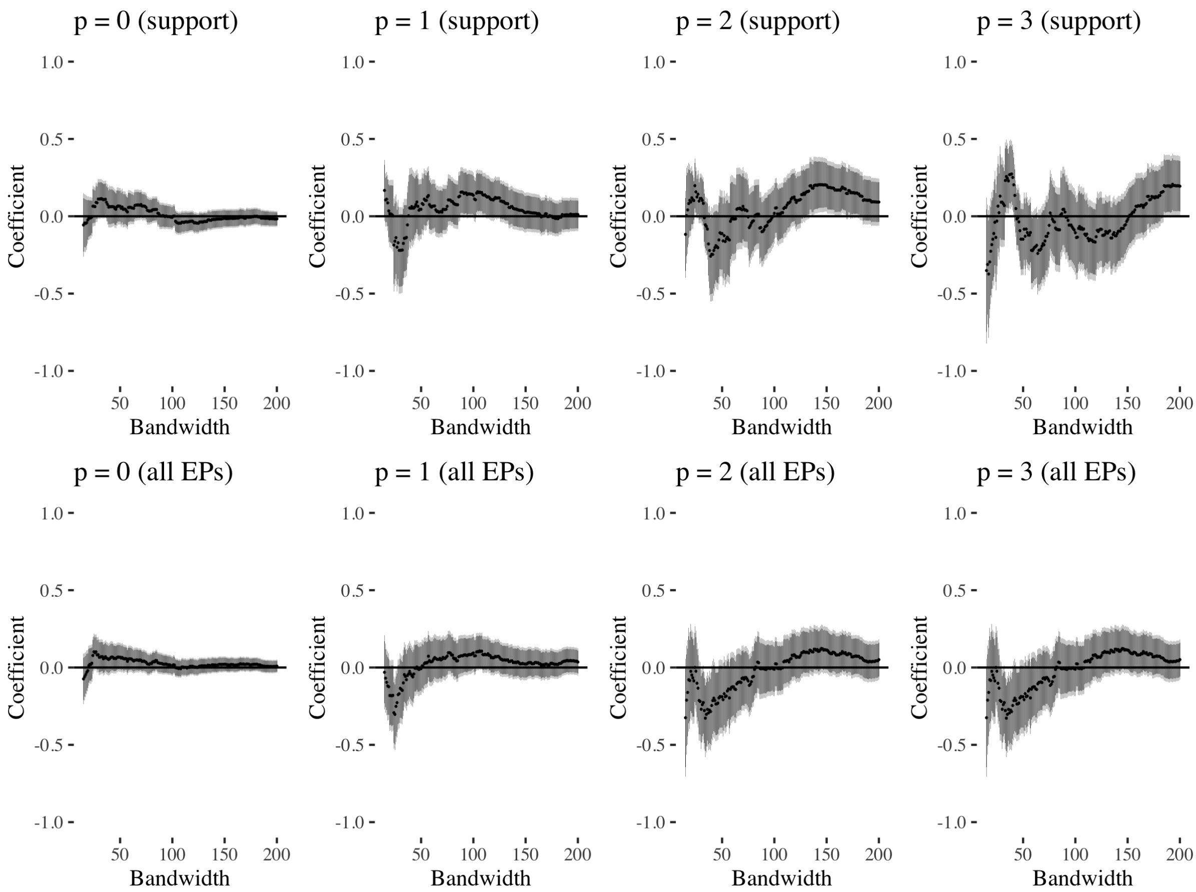

Figure G.5: Effect of Operation Strong Safety (y-axis) on contraband hit rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$). Top 4 panels display effects using stop-and-search data from "support" non-treated border counties (Cameron, Webb, Zapata, Maverick, Val Verde). Bottom 4 panels display effects using data from all non-treated border counties with entry points to Mexico (including Presidio, El Paso, and Hudspeth).

H Heterogeneity between border, non-border

H.1 Warning stops

Table H.5: Heterogenous discontinuous effects of Operation Strong Safety conditional on Hidalgo/Starr location on warning stops

	Warning Stop Rate (0 = Citation)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS x Border	0.18***	0.25***	0.30***	0.30***	0.18***	0.26***	0.30***	0.30***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	17570187	17570187	17570187	17570187	17570187	17570187	17570187	17570187
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models use all stop data throughout Texas. Models with controls adjust for year, month, and day-of-year fixed effects. Robust standard errors in parentheses.

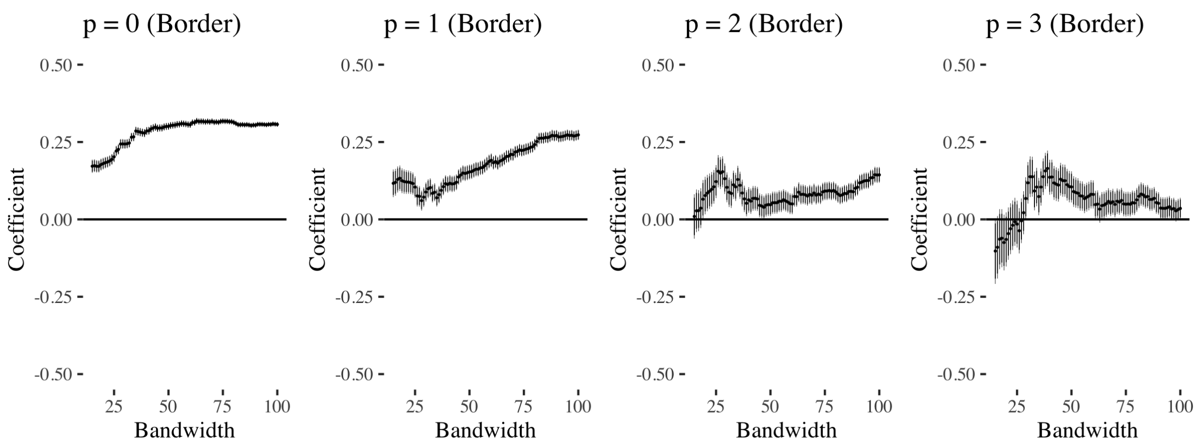

Figure H.6: Heterogenous effect of Operation Strong Safety (y-axis) conditional on Hidalgo/Starr region on contraband hit rate using different bandwidths (x-axis, 10-100 days) and degrees (by panel e.g. $p = 0 \ldots 3$).

H.2 Hit rates

Table H.6: Heterogenous discontinuous effects of Operation Strong Safety conditional on Hidalgo/Starr location on hit rates

	Hit Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS x Border	-0.09^{***}	-0.05^{***}	-0.05^{**}	-0.06^{*}	-0.09^{***}	-0.06^{***}	-0.05^{**}	-0.06^{*}
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
N	319597	319597	319597	319597	319597	319597	319597	319597
Controls	N	N	N	N	Y	Y	Y	Y
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models use all stop-and-search data throughout Texas. Models with controls adjust for year, month, and day-of-year fixed effects. Robust standard errors in parentheses.

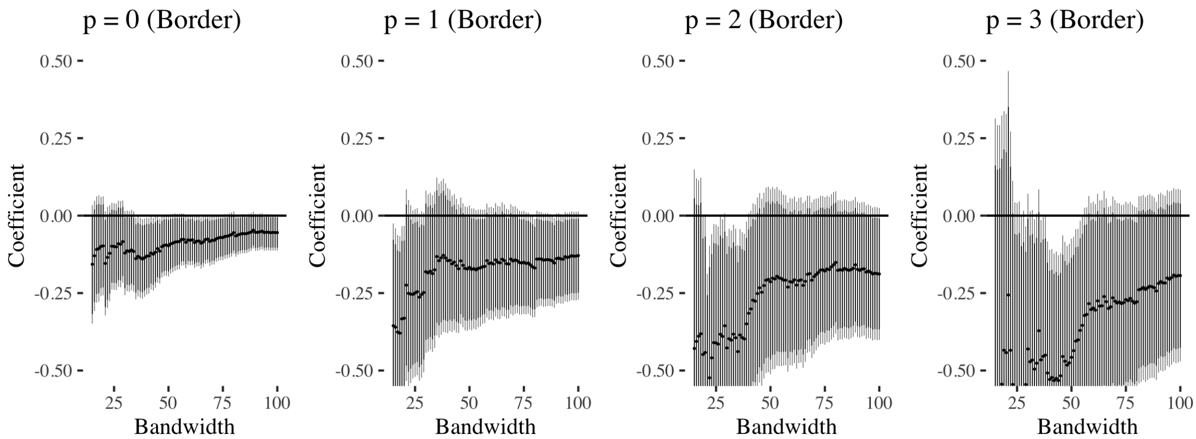

Figure H.7: Heterogenous effect of Operation Strong Safety (y-axis) conditional on Hidalgo/Starr region on contraband hit rate using different bandwidths (x-axis, 10-100 days) and degrees (by panel e.g. $p = 0 \dots 3$).

I Hit rate outcome re-estimation with optimal bandwidth

Table I.7: Estimation of effect of Operation Strong Safety on hit rates with optimal bandwidth selection across kernels and degrees

Estimate	SE	p-value	Bandwidth	N	Kernel	Degree
-0.07	0.04	0.02	129.63	208	Triangular	0.00
-0.10	0.04	0.00	335.03	1046	Triangular	1.00
-0.10	0.06	0.14	425.61	1301	Triangular	2.00
-0.08	0.08	0.32	480.42	1491	Triangular	3.00
-0.05	0.03	0.03	115.00	175	Uniform	0.00
-0.12	0.04	0.00	333.84	1041	Uniform	1.00
-0.11	0.05	0.00	457.07	1420	Uniform	2.00
-0.07	0.08	0.54	405.25	1255	Uniform	3.00
-0.07	0.04	0.02	111.90	169	Epanechnikov	0.00
-0.09	0.04	0.04	277.15	836	Epanechnikov	1.00
-0.09	0.06	0.13	400.74	1245	Epanechnikov	2.00
-0.06	0.08	0.47	442.32	1367	Epanechnikov	3.00

J Temporal placebos

J.1 Warning rates

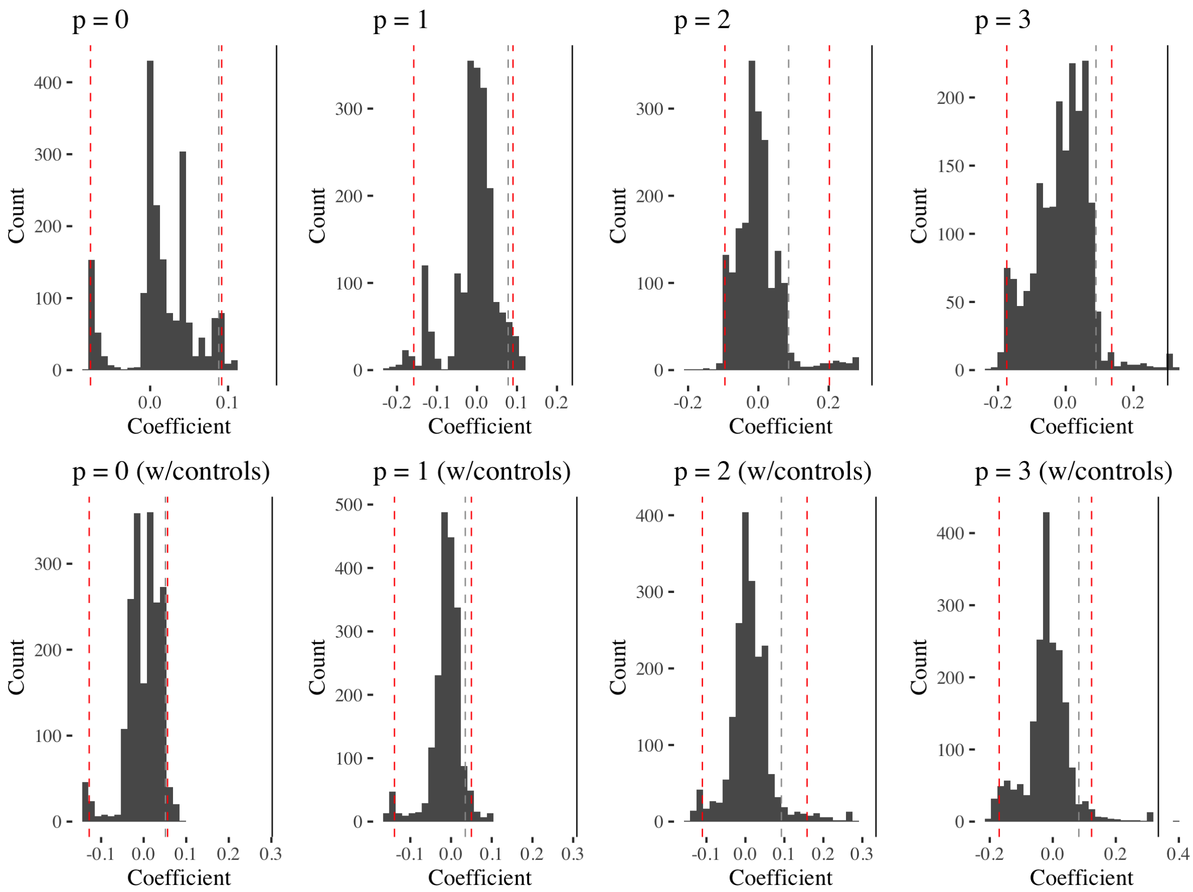

Figure J.8: Comparing true estimate with placebo estimates on pre-treatment discontinuities between 30 days after the end and beginning of the pre-treatment data (censoring post-OSS data). These estimates use all stop-and-search data. Solid vertical line is the "true" effect of OSS. The dashed red lines are 95% confidence intervals generated from the placebo estimates. The dashed grey line is the point where 95% of the coefficients are larger (i.e. a 1-tailed test threshold). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

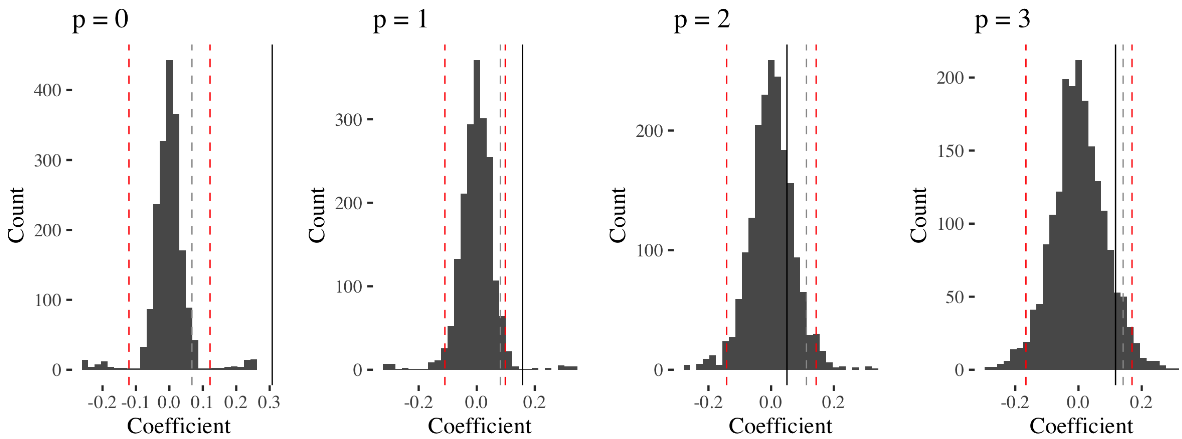

Figure J.9: Comparing true estimate with placebo estimates on pre-treatment discontinuities between 50 days after the end and beginning of the pre-treatment data (censoring post-OSS data). These estimates use stop data 50 days out from the respective discontinuities. Solid vertical line is the "true" effect of OSS (for a 50 day bandwidth). The dashed red lines are 95% confidence intervals generated from the placebo estimates. The dashed grey line is the point where 95% of the coefficients are larger (i.e. a 1-tailed test threshold). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

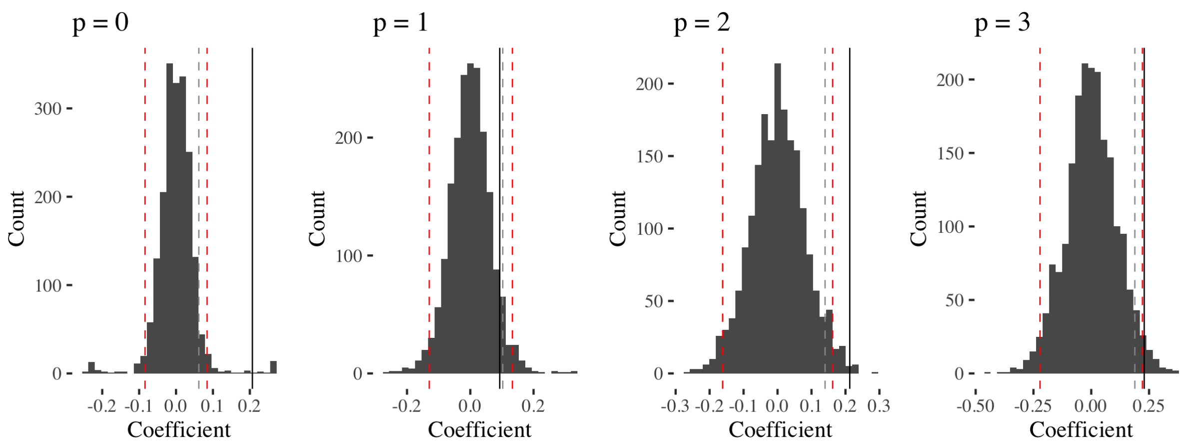

Figure J.10: Comparing true estimate with placebo estimates on pre-treatment discontinuities between 25 days after the end and beginning of the pre-treatment data (censoring post-OSS data). These estimates use stop data 25 days out from the respective discontinuities. Solid vertical line is the "true" effect of OSS (for a 25 day bandwidth). The dashed red lines are 95% confidence intervals generated from the placebo estimates. The dashed grey line is the point where 95% of the coefficients are larger (i.e. a 1-tailed test threshold). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

J.2 Hit rates

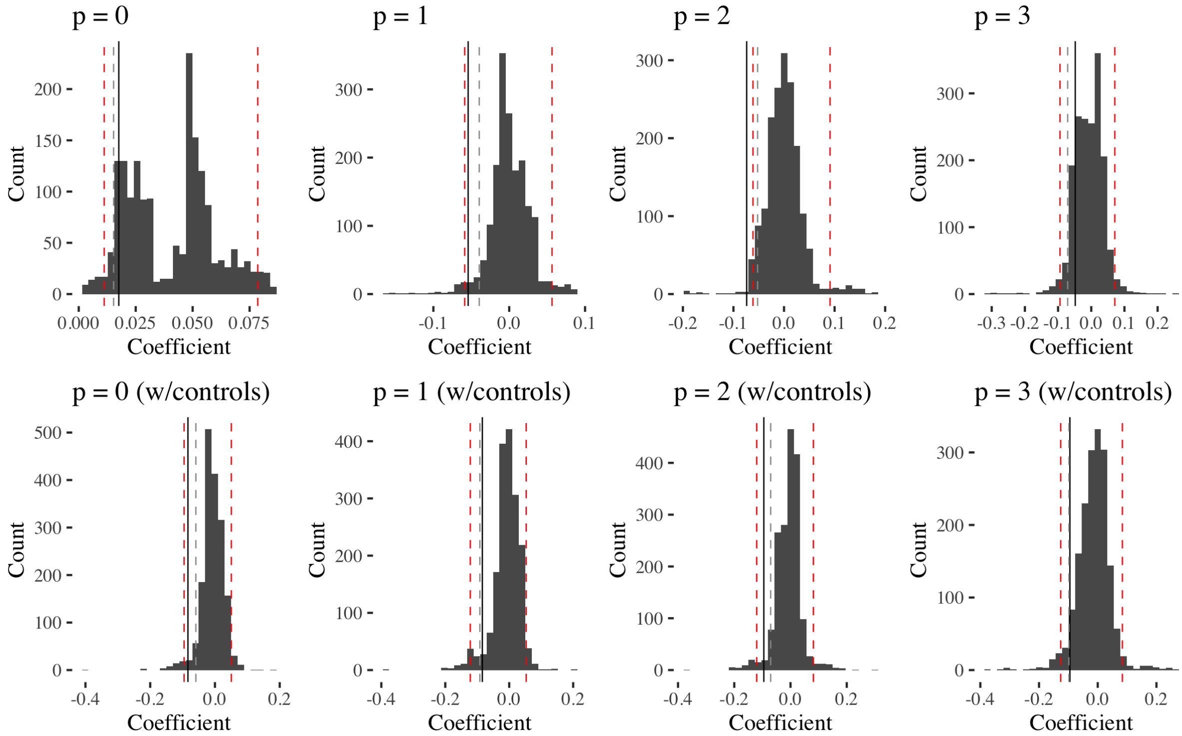

Figure J.11: Comparing true estimate with placebo estimates on pre-treatment discontinuities between 30 days after the end and beginning of the pre-treatment data (censoring post-OSS data). These estimates use all stop-and-search data. Solid vertical line is the "true" effect of OSS. The dashed red lines are 95% confidence intervals generated from the placebo estimates. The dashed grey line is the point where 95% of the coefficients are larger (i.e. a 1-tailed test threshold). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

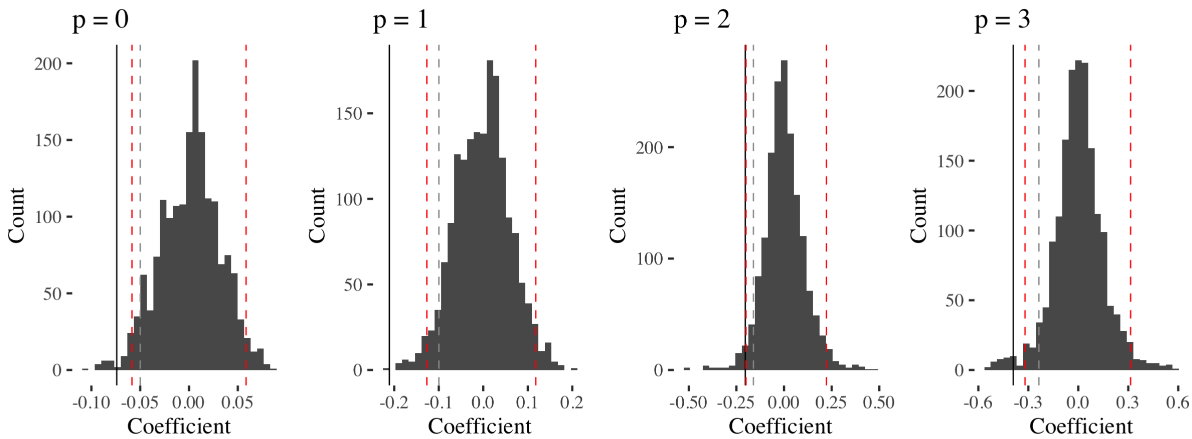

Figure J.12: Comparing true estimate with placebo estimates on pre-treatment discontinuities between 50 days after the end and beginning of the pre-treatment data (censoring post-OSS data). These estimates use stop-and-search data 50 days out from the respective discontinuities. Solid vertical line is the "true" effect of OSS (for a 50 day bandwidth). The dashed red lines are 95% confidence intervals generated from the placebo estimates. The dashed grey line is the point where 95% of the coefficients are larger (i.e. a 1-tailed test threshold). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

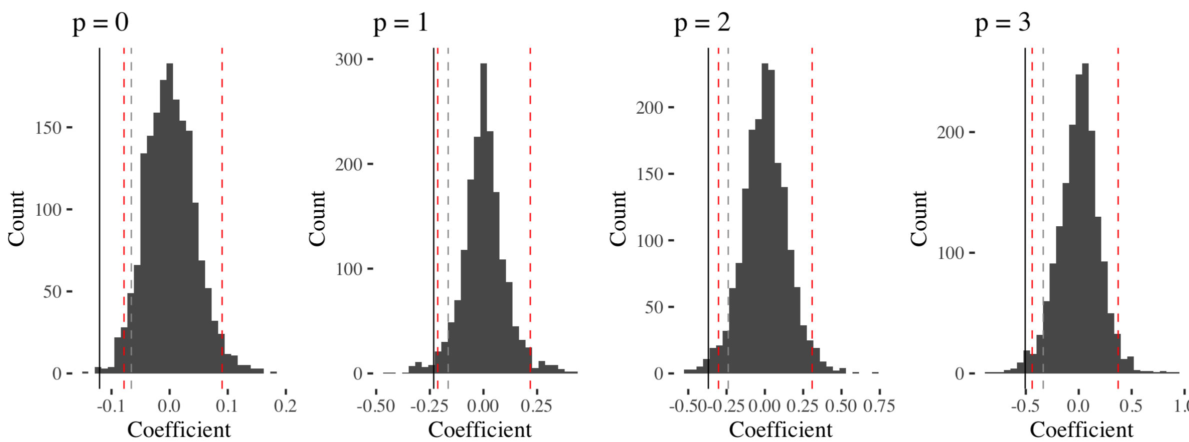

Figure J.13: Comparing true estimate with placebo estimates on pre-treatment discontinuities between 25 days after the end and beginning of the pre-treatment data (censoring post-OSS data). These estimates use stop-and-search data 25 days out from the respective discontinuities. Solid vertical line is the "true" effect of OSS (for a 25 day bandwidth). The dashed red lines are 95% confidence intervals generated from the placebo estimates. The dashed grey line is the point where 95% of the coefficients are larger (i.e. a 1-tailed test threshold). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

K Ruling out anticipatory effects

K.1 Visual evaluation

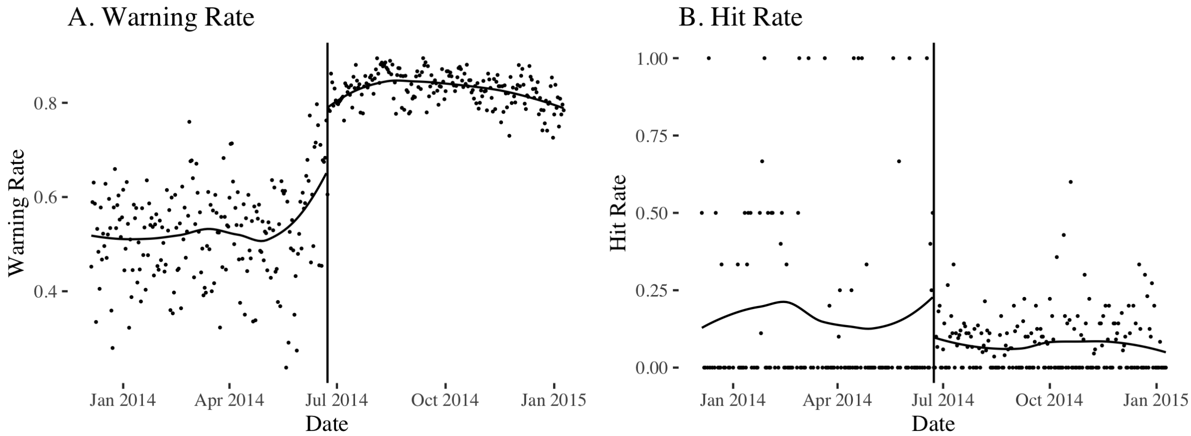

Figure K.14: Warning rates (Panel A, y-axis) and hit rates (Panel B, y-axis) over time (x-axis) 200 days after and before the initialization of Operation Strong Safety (denoted by the vertical black line). Loess models fit on each side of the OSS discontinuity.

K.2 Donut RD on warnings outcome

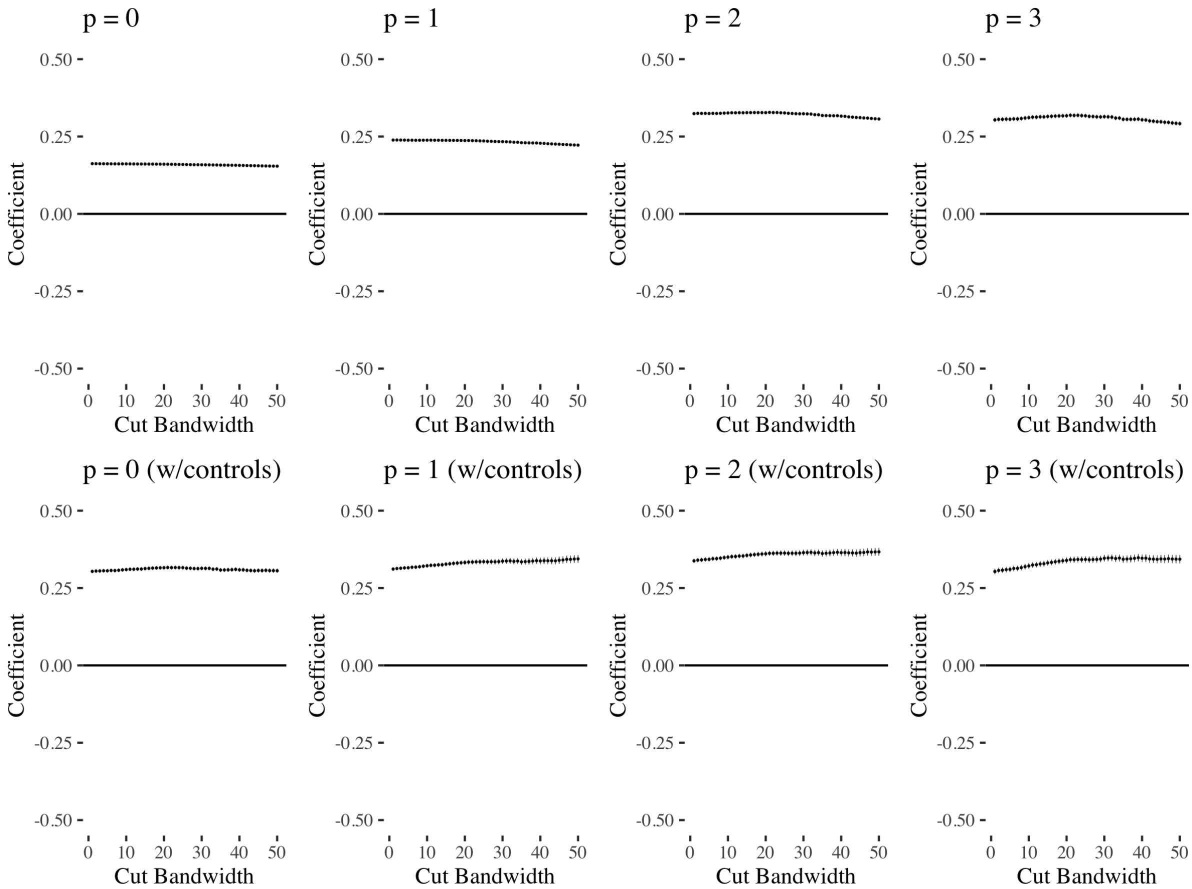

Figure K.15: Effect of Operation Strong Safety (y-axis) region on warning rate using all data from the Hidalgo/Starr region and cutting data 1-50 days from the discontinuity (x-axis). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$).

K.3 Donut RD on hit rate outcome

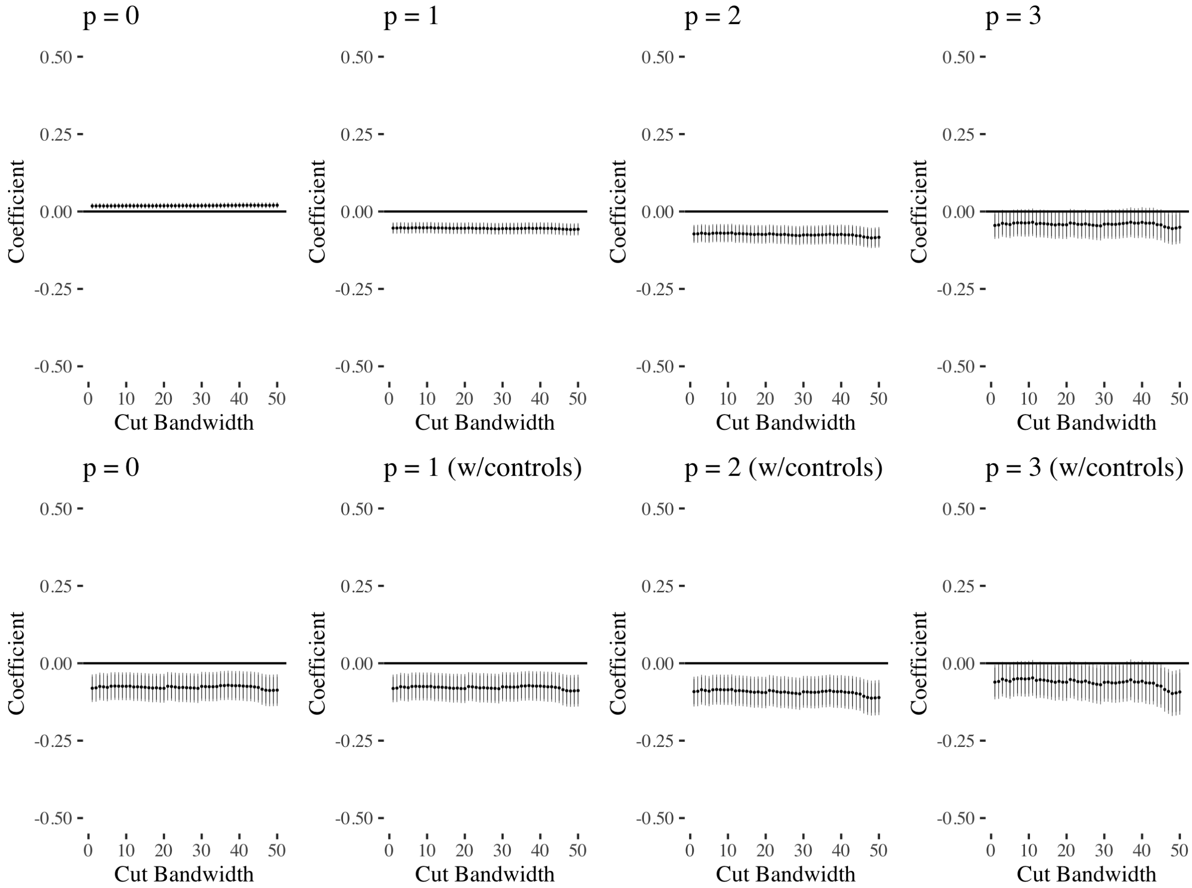

Figure K.16: Effect of Operation Strong Safety (y-axis) region on hit rate using all stop-and-search data from the Hidalgo/Starr region and cutting data 1-50 days from the discontinuity (x-axis). Each panel characterizes estimates where the running variable is to the *pth* degree (by panel e.g. $p = 0 \ldots 3$). The top 4 panels do not adjust for temporal fixed effects. The top 4 panels adjust for year, month, and day of week fixed effects.

L Adjusting for Lagged DV

L.1 All data

Table L.8: Effect of Operation Strong Safety on warning and hit rates (adjusting for prior day's rate)

Panel A: Warning Rate	(1)	(2)	(3)	(4)
OSS	0.16***	0.18***	0.19***	0.17***
	(0.00)	(0.00)	(0.00)	(0.00)
N	980880	980880	980880	980880
Panel B: Hit Rate	(1)	(2)	(3)	(4)
OSS	−0.08***	−0.08***	−0.09***	−0.07*
	(0.02)	(0.02)	(0.02)	(0.03)
N	16187	16187	16187	16187
Controls	Y	Y	Y	Y
Degree	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models adjust for year, month, and day-of-week fixed effects in addition to the prior day's warning rate.

L.2 Close to bandwidth

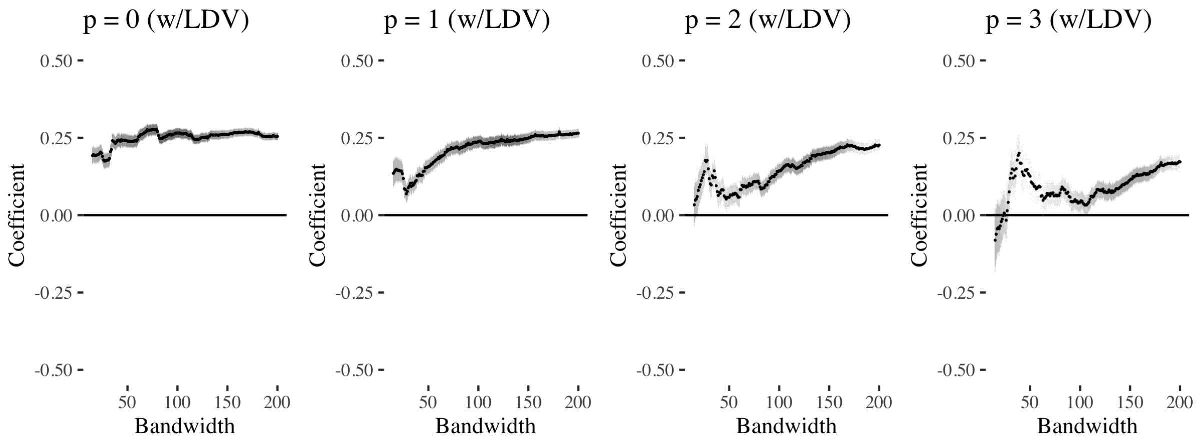

Figure L.17: Effect of Operation Strong Safety (y-axis) on warning stop rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$) in the Hidalgo/Starr region. These estimates adjust for the prior day's warning stop rate.

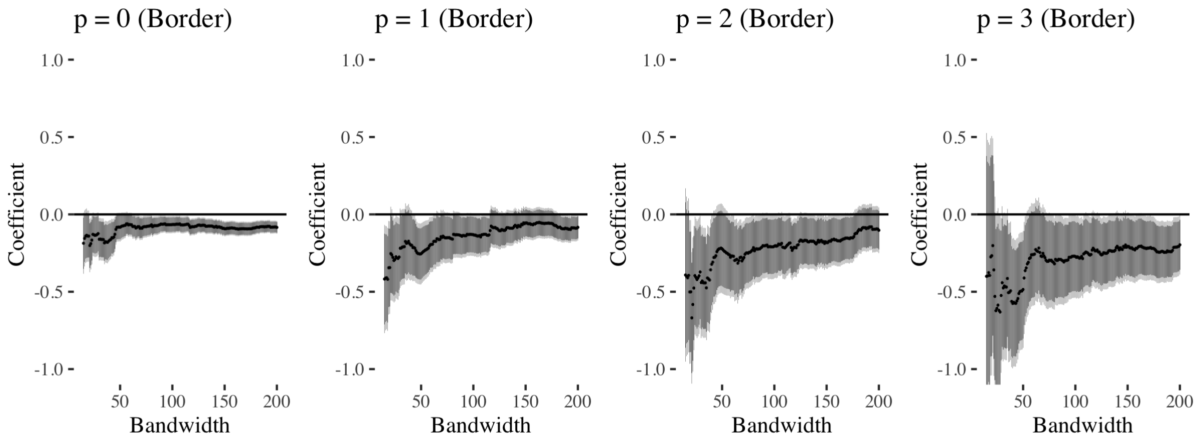

Figure L.18: Effect of Operation Strong Safety (y-axis) on stop-and-search hit rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$) in the Hidalgo/Starr region. These estimates adjust for the prior day's hit rate.

M Adjusting for weather

M.1 Weather placebo tests

Table M.9: Effect of Operation Strong Safety on standardized weather covariates (Hidalgo/Starr averages)

Panel A: Temperature	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.08	−0.04	0.45***	1.65***	0.10	0.03	−0.10	0.46***
	(0.04)	(0.09)	(0.12)	(0.13)	(0.07)	(0.07)	(0.08)	(0.09)
Panel B: Rainfall	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.13**	0.31***	0.23	−0.06	0.29**	0.33**	0.33**	−0.09
	(0.04)	(0.09)	(0.12)	(0.15)	(0.10)	(0.10)	(0.13)	(0.15)
N	2921	2921	2921	2921	2921	2921	2921	2921
Degree	0	1	2	3	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models are aggregated to the daily level. Panel A assesses the discontinuous effect of OSS on average maximum temperature throughout the Hidalgo/Starr region (in Fahrenheit, standardized). Panel B assesses the discontinuous effect of OSS on average precipitation (in inches, standardized) throughout the Hidalgo/Starr region.

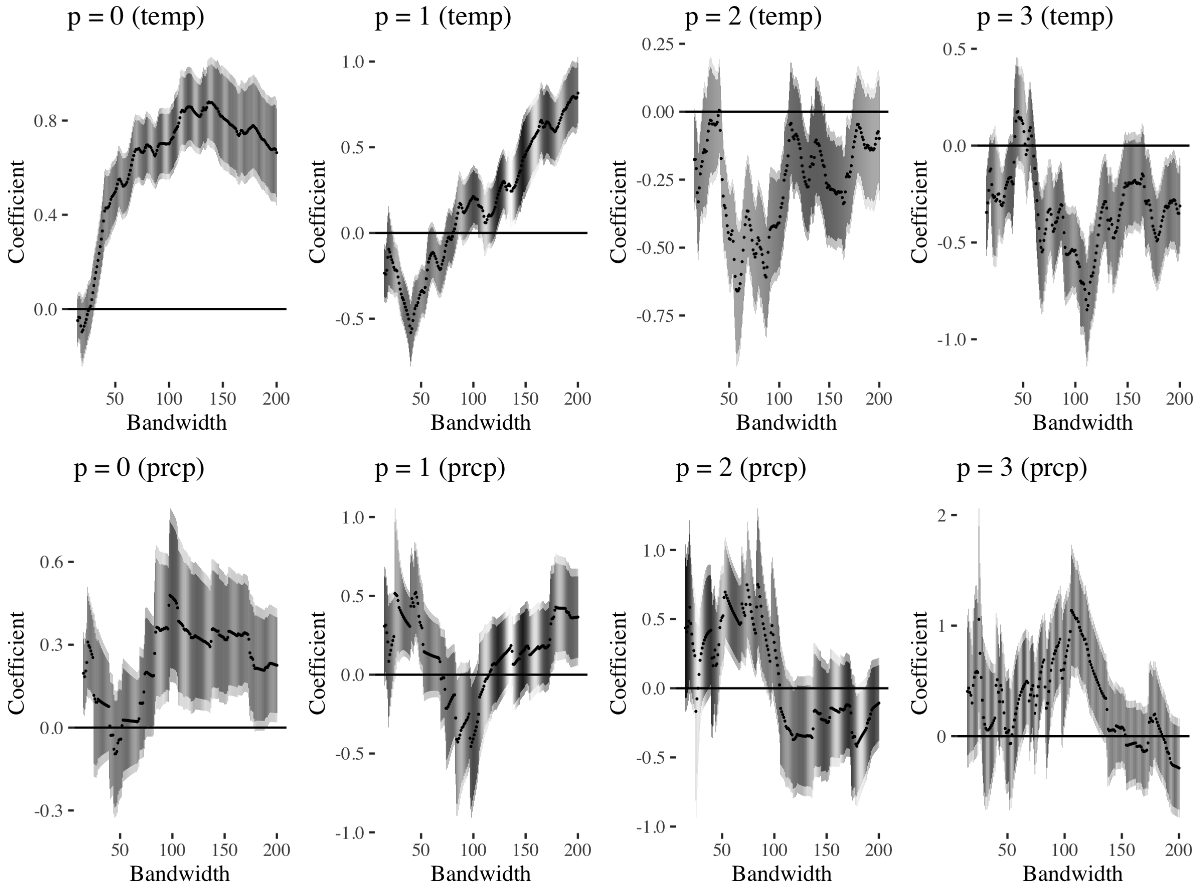

Figure M.19: Effect of Operation Strong Safety (y-axis) on standardized daily weather outcomes using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$) in the Hidalgo/Starr region. The top 4 panels evaluate the discontinuous effect of OSS on average maximum temperature throughout the Hidalgo/Starr region (in Fahrenheit, standardized). The bottom 4 panels evaluate the discontinuous effect of OSS on average precipitation (in inches, standardized) throughout the Hidalgo/Starr region.

M.2 Estimates adjusting for weather

M.2.1 All data

Table M.10: Effect of Operation Strong Safety on warning and hit rates (adjusting for prior day's rate)

Panel A: Warning Rate	(1)	(2)	(3)	(4)
OSS	0.16***	0.17***	0.19***	0.16***
	(0.00)	(0.00)	(0.00)	(0.00)
N	980880	980880	980880	980880
Panel B: Hit Rate	(1)	(2)	(3)	(4)
OSS	−0.08***	−0.08***	−0.10***	−0.06*
	(0.02)	(0.02)	(0.02)	(0.03)
N	16187	16187	16187	16187
Controls	Y	Y	Y	Y
Degree	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models adjust for year, month, and day-of-week fixed effects in addition to the prior day's warning rate and standardized weather covariates (temperature in Fahrenheit, precipitation in inches).

M.2.2 Close to bandwidth

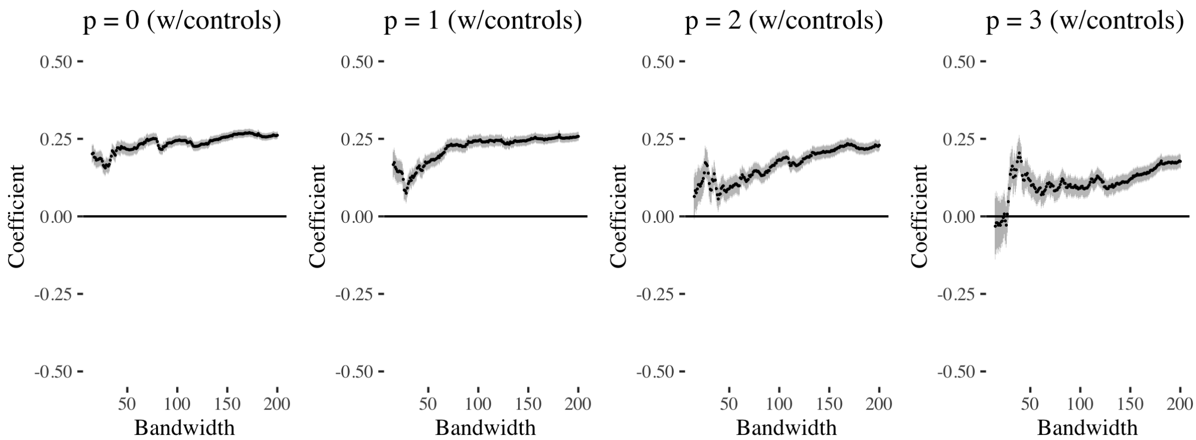

Figure M.20: Effect of Operation Strong Safety (y-axis) on warning stop rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$) in the Hidalgo/Starr region. These estimates adjust for the prior day's warning stop rate in addition to standardized weather covariates (temperature in Fahrenheit, precipitation in inches).

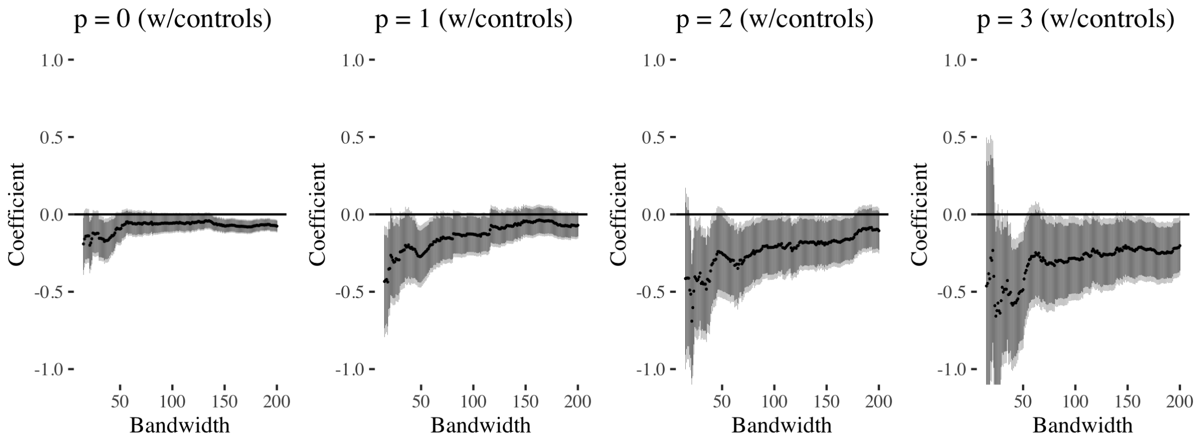

Figure M.21: Effect of Operation Strong Safety (y-axis) on stop-and-search hit rate using different bandwidths (x-axis, 10-200 days) and degrees (by panel e.g. $p = 0 \ldots 3$) in the Hidalgo/Starr region. These estimates adjust for the prior day's hit rate in addition to standardized weather covariates (temperature in Fahrenheit, precipitation in inches)

N Evaluating effects on crime

N.1 Crime trends

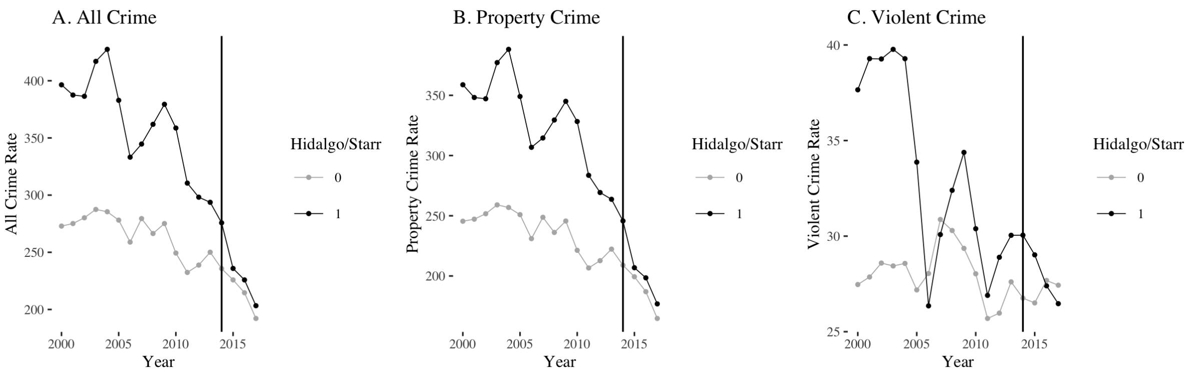

Figure N.22: Crime rates (y-axis) over time (by year, x-axis) between counties treated and non-treated by Operation Strong Safety in Texas. The black line characterizes crime rates in Hidalgo/Starr counties. The grey line characterizes crime rates throughout the rest of Texas. The vertical black line is the year Operation Strong Safety was implemented. Panel A displays all crimes, Panel B displays property crimes, and Panel C displays violent crimes.

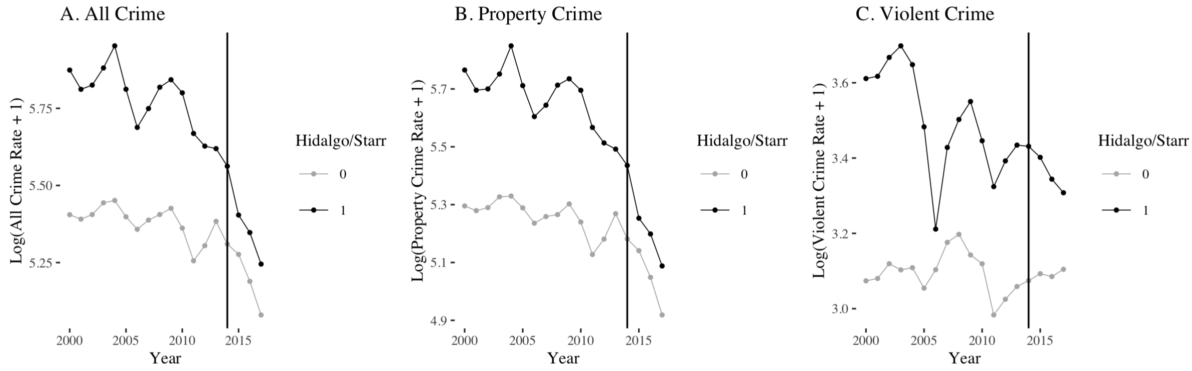

Figure N.23: Log(crime rates + 1) (y-axis) over time (by year, x-axis) between counties treated and non-treated by Operation Strong Safety in Texas. The black line characterizes crime rates in Hidalgo/Starr counties. The grey line characterizes crime rates throughout the rest of Texas. The vertical black line is the year Operation Strong Safety was implemented. Panel A displays all crimes, Panel B displays property crimes, and Panel C displays violent crimes.

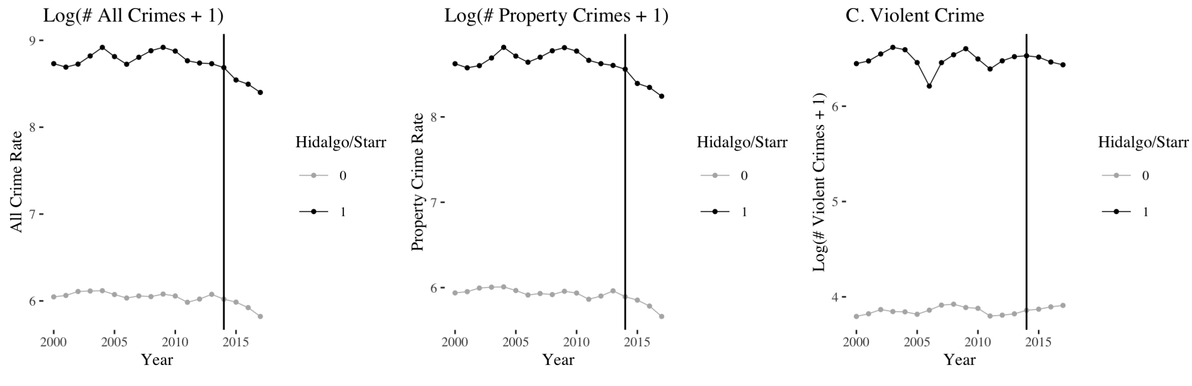

Figure N.24: Log(crimes) (y-axis) over time (by year, x-axis) between counties treated and non-treated by Operation Strong Safety in Texas. The black line characterizes crime rates in Hidalgo/Starr counties. The grey line characterizes crime rates throughout the rest of Texas. The vertical black line is the year Operation Strong Safety was implemented. Panel A displays all crimes, Panel B displays property crimes, and Panel C displays violent crimes.

N.2 Synthetic control estimates

Table N.11: Synthetic control OSS effect estimates on crime

Panel A: Rate (per 10,000)	All	Property	Violent
OSS	-107.55	-72.52	8.91
	(181.9)	(143.17)	(25.64)
Pre-OSS μ (H/S)	362.70	329.24	33.47
Panel B: Log(Rate (per 10,000) + 1)	All	Property	Violent
OSS	-0.09	-0.17	-0.10
	(0.7)	(0.32)	(0.45)
Pre-OSS μ (H/S)	8.8	8.69	6.49
Panel C: Log(Count + 1)	All	Property	Violent
OSS	-0.09	-0.09	-0.03
	(0.29)	(0.23)	(0.33)
Pre-OSS μ (H/S)	5.78	5.67	3.5
N	4572	4572	4572
Counties	254	254	254

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. All models display generalized synthetic control estimates using tools by Xu (2017). Models 1, 2, and 3 are derived from counts of all crimes, property crimes, and violent crimes. Models from Panel A use rate outcomes. Panel B uses logged rate outcomes (plus 1 to ensure identification). Panel C uses logged count outcomes (plus 1 to ensure identification). All estimates adjust for two-way fixed effects. Standard errors derived from a parametric bootstrap procedure using 1000 replicates.

N.2.1 Event study plots

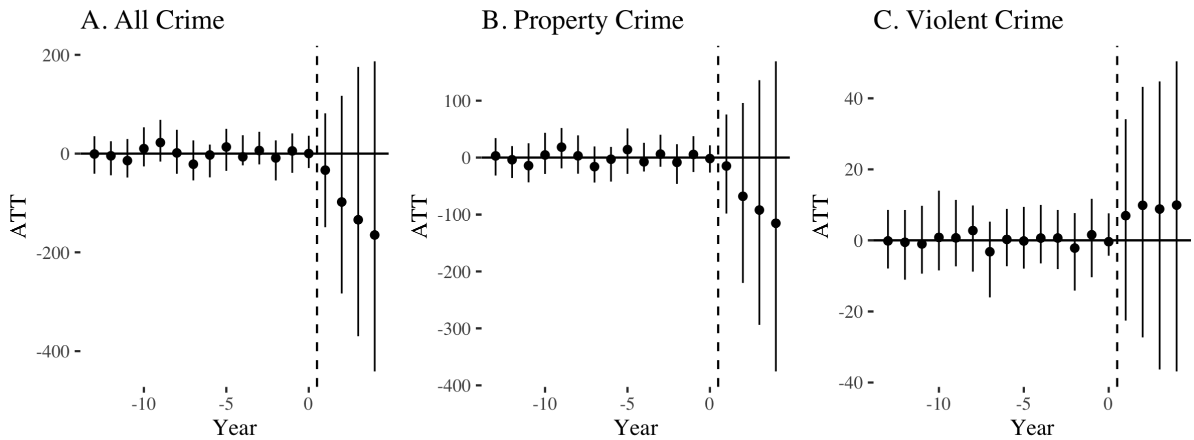

Figure N.25: Effect of OSS on crime rates (y-axis, normalized by every 10,000 people in county population) over time (x-axis, by year). Dashed vertical line denotes the time OSS was implemented. Panel A characterizes estimates for all crimes. Panel B characterizes estimates for property crime. Panel C characterizes estimates for violent crime.

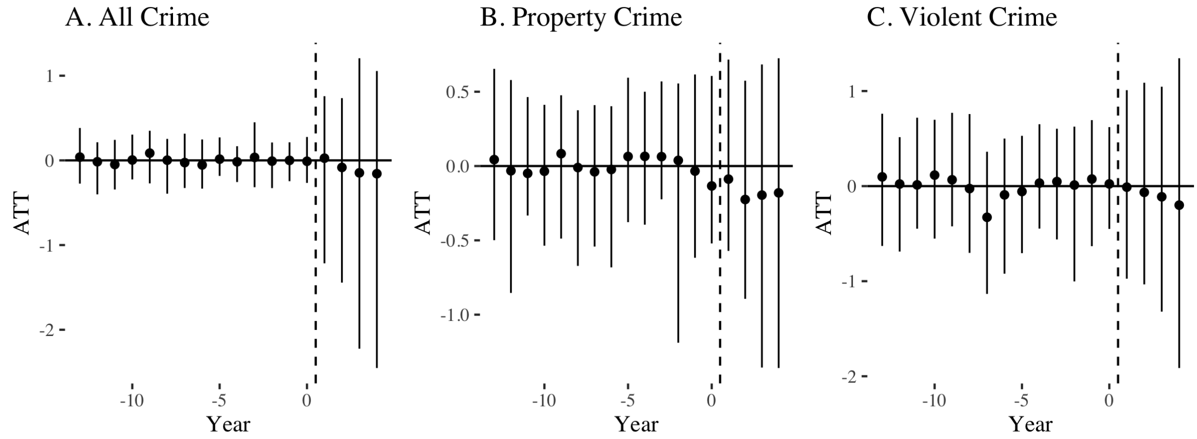

Figure N.26: Effect of OSS on logged crime rates (y-axis, plus 1 to ensure identification, rate normalized by every 10,000 people in county population) over time (x-axis, by year). Dashed vertical line denotes the time OSS was implemented. Panel A characterizes estimates for all crimes. Panel B characterizes estimates for property crime. Panel C characterizes estimates for violent crime.

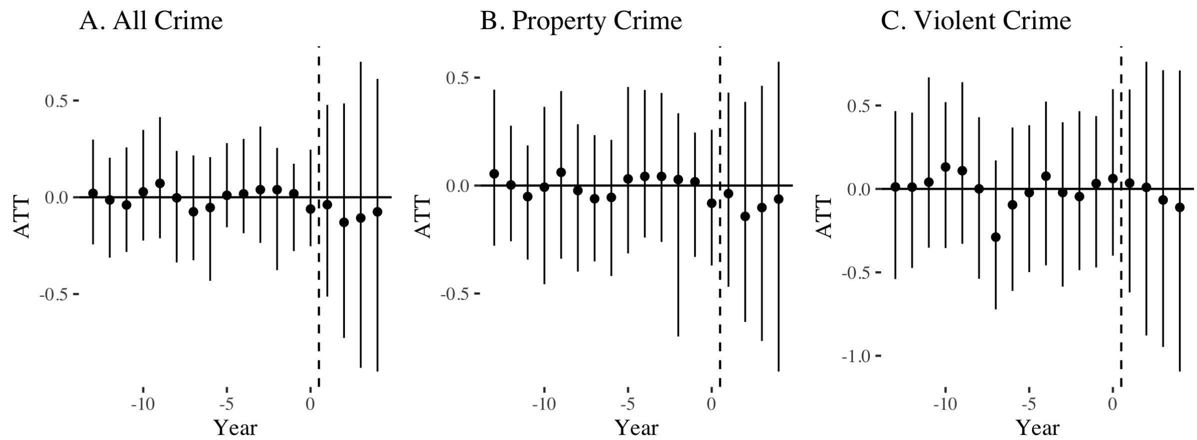

Figure N.27: Effect of OSS on logged crime counts (y-axis, plus 1 to ensure identification) over time (x-axis, by year). Dashed vertical line denotes the time OSS was implemented. Panel A characterizes estimates for all crimes. Panel B characterizes estimates for property crime. Panel C characterizes estimates for violent crime.

O Low-level vs. ticketable speeding offenses

O.1 Descriptive statistics

Table O.12: Demonstrating the difference between discretionary and ticketable speeding stops on Pr(warning) outcome.

	Discretionary speeding stops		**Ticketable speeding stops**	
Pr(warning, 0 = ticket)	Pre-OSS	Post-OSS	Pre-OSS	Post-OSS
Border	0.90	0.85	0.07	0.03
Non-border	0.90	0.90	0.03	0.03

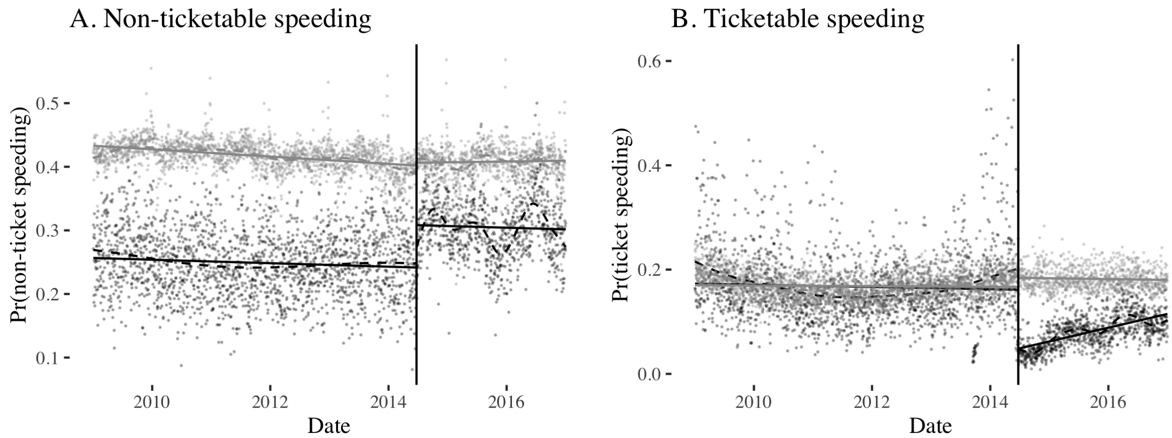

Figure O.28: Non-ticketable speeding stop rate (Panel A, y-axis) and ticketable speeding stop rate (Panel B, y-axis) over time (x-axis). Solid and dashed lines characterize linear and loess models fit on each side of the discontinuity. Black lines denote border stops. Grey lines denote stops outside of Hidalgo/Starr. The vertical line denotes when Operation Strong Safety is implemented.

O.2 RD estimates

Table O.13: Effect of Operation Strong Safety on discretionary speed stops

Panel A: Border	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.06^{***}	0.07^{***}	0.06^{***}	0.07^{***}	0.07^{***}	0.08^{***}	0.06^{***}	0.03^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	981046	981046	981046	981046	981046	981046	981046	981046
Panel B: Non-border	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	16589141	16589141	16589141	16589141	16589141	16589141	16589141	16589141
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Panel A use stop data from the Hidalgo/Starr region. Panel B use stop data from the rest of Texas. Models with controls adjust for year, month, and day-of-week fixed effects. Robust standard errors in parentheses.

Table O.14: Effect of Operation Strong Safety on ticketable speed stops

Panel A: Border	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	-0.08^{***}	-0.10^{***}	-0.16^{***}	-0.17^{***}	-0.16^{***}	-0.16^{***}	-0.18^{***}	-0.17^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	981046	981046	981046	981046	981046	981046	981046	981046
Panel B: Non-border	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OSS	0.01^{***}	0.02^{***}	-0.01^{***}	0.01^{***}	0.00^{***}	0.00^{***}	-0.01^{***}	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	16589141	16589141	16589141	16589141	16589141	16589141	16589141	16589141
Degree	0	1	2	3	0	1	2	3
Controls	N	N	N	N	Y	Y	Y	Y

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Panel A use stop data from the Hidalgo/Starr region. Panel B use stop data from the rest of Texas. Models with controls adjust for year, month, and day-of-week fixed effects. Robust standard errors in parentheses.

P The role of prior experience

P.1 Warning outcome

Table P.15: Effect of Operation Strong Safety on Warning Rates (subset to experienced officers)

Panel A: > 50% H/S Pre-OSS	(1)	(2)	(3)	(4)
OSS	0.09***	0.15***	0.22***	0.20***
	(0.00)	(0.00)	(0.01)	(0.01)
N	513870	513870	513870	513870
Panel B: > 90% H/S Pre-OSS	(1)	(2)	(3)	(4)
OSS	0.09***	0.14***	0.21***	0.19***
	(0.00)	(0.00)	(0.01)	(0.01)
N	450606	450606	450606	450606
Controls	Y	Y	Y	Y
Degree	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

P.2 Hit rate outcome

Table P.16: Effect of Operation Strong Safety on Hit Rates (subset to experienced officers)

Panel A: > 50% H/S Pre-OSS	(1)	(2)	(3)	(4)
OSS	-0.13^{***}	-0.13^{***}	-0.15^{***}	-0.13^{***}
	(0.03)	(0.03)	(0.03)	(0.04)
N	8752	8752	8752	8752
Panel B: > 90% H/S Pre-OSS	(1)	(2)	(3)	(4)
OSS	-0.14^{***}	-0.13^{***}	-0.15^{***}	-0.14^{***}
	(0.03)	(0.03)	(0.03)	(0.04)
N	7632	7632	7632	7632
Controls	Y	Y	Y	Y
Degree	0	1	2	3

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

References

Department of Public Safety (Feb. 2015). *Operation Strong Safety: Report to the 85th Texas Legislature and Office of the Governor.*