Pretextual Stop Restriction and Policing: Evidence from Los Angeles

Kyutaro Matsuzawa
kyutarom@uoregon.edu

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Abstract

This paper explores the impact of the Los Angeles Police Department (LAPD)'s restriction on pretextual stops on policing behavior and public safety. Using data on all California traffic stops, I find compelling evidence that the policy led to an immediate reduction in stops for equipment or non-moving violations. However, I find little evidence that the overall number of total stops decreased in the short run, potentially due to police substitution behavior. This finding is consistent with my economic framework, which suggests that police officers will respond to increased scrutiny placed on some tasks by shifting their behavior to other tasks. At the same time, I find that this policy led to an approximately 15 percent reduction in the number of racial minorities stopped by police officers. Focusing on traffic stop outcomes, I document that the number of stops resulting in a warning decreased, and conversely, the number of stops resulting in a citation may have increased. Moreover, the policy led to fewer searches and contraband found, but little change in contraband seized. Finally, I find little evidence that the number of reported crimes, arrests, and traffic fatalities increased following the restriction of pretextual stops. Together, my findings imply that the LAPD's pretextual stop restriction achieved its intended goal of reducing racial disparities without diminishing public safety.

JEL Codes: J15; J22; K42

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1 Introduction

With recent social justice movements and incidents of police brutality, the public's trust in the police has fallen recently. For instance, in 2023, confidence in the police fell to an all-time low of 43 percent, down 10 percentage points from 2019 and 21 percentage points from a record high in 2004 (Gallup 2024). Furthermore, another Gallup poll shows that 89 percent of Americans believe policing needs major or minor changes (McCarthy 2022). This decline in trust in the police highlights the ongoing principal-agent problem between the public and the police. In this context, the public (principal) aims to maximize social welfare by minimizing crime and traffic accidents while upholding individual rights and respect. However, police officers (agents) face the challenge of allocating their time between multiple tasks and using their discretion when choosing between their tasks. For example, a police officer may enforce traffic laws or patrol neighborhoods to find and deter criminal behavior. Moreover, an officer may decide to stop and search specific types of individuals over others. Thus, according to multitasking theory, police will focus their effort on measured and rewarded tasks at the expense of other tasks (Dumont et al. 2008; Holmstrom & Milgrom 1991 Hong et al. 2018; Johnson et al. 2015; Knutsson & Tyrefors 2022; Reeves 2024).

This paper first provides empirical evidence supporting police multitasking theory by examining the trade-off between conducting pretextual stops and enforcing traffic laws. Pretextual stops involve stopping a driver for minor infractions, such as equipment or non-moving violations, to investigate other suspected criminal activity. For instance, an officer might pull over a driver for a broken taillight or tinted window and ask, "Do you know why you were pulled over?" The officer asks this question, hoping the driver admits to a more serious crime or violation. While police officers claim these stops are effective in detecting crime, several studies have shown that pretextual stops can lead to higher racial disparities in criminal justice (Makofske 2023; Naddeo & Pulvino 2023; Parker et al. 2024; Rushin & Edwards 2021).

While the U.S. Supreme Court ruled that pretextual stops are constitutional, several cities have enacted policies to reduce them. Los Angeles Police Department (LAPD) is one jurisdiction where the use of pretextual stops is limited. Under LAPD's new policy, officers can only stop a driver who is a severe public threat and must articulate why they stopped an individual. By restricting these stops, this policy increased the cost of a specific task that police officers commonly employ.

Using individual police stop data from California and a combination of regression discontinuity and difference-in-discontinuity approaches, I initially compare immediate changes in different types of traffic stops around the policy reform date and the racial impacts of such changes. In addition, I supplement my analysis using data from the City of L.A. Open Data Portal, California Open Justice, Uniform Crime Reporting, and the Statewide Integrated Traffic Records System to investigate changes in traffic stop outcomes, traffic accidents, or crime rates due to the policy. Because the effects of some of my outcomes may evolve more gradually, I also employ a synthetic difference-in-differences estimator, comparing long-run changes in outcomes in Los Angeles relative to synthetic counterfactuals.

I document an immediate 1.8 per 100,000 reduction in traffic stops due to equipment or non-moving violations. However, in the short-run, the overall number of police-initiated stops stayed the same due to increased stops for other minor moving violations, such as speeding or failure to stop at a stop sign. These findings are consistent with the multitasking theoretical model that predicts that this new policy that increases scrutiny placed on some tasks may lead to police substitution behavior such as increased traffic enforcement (Dumont et al. 2008; Holmstrom & Milgrom 1991 Hong et al. 2018; Johnson et al. 2015; Knutsson & Tyrefors 2022; Reeves 2024). Focusing on these effects by race, I find that the policy significantly reduced the number of Black drivers stopped by approximately 10.6 per 100,000 and the number of Hispanic drivers stopped by approximately 2.6 per 100,000. In contrast, there was no change in the number of White drivers stopped.

Focusing on the outcome of traffic stops, I find that the number of warnings has decreased by 2.5 to 2.9 per 100,000 (or 23.8 to 27.0 percent), while the number of citations may have increased. These findings continue to support the potential substitution pattern I am uncovering. I also find that the number of stops that led to searches decreased by 1.2 to 1.3 per 100,000 (or 17.1 to 18.3 percent), and consequently, the number of contraband found decreased by 0.3 per 100,000 (or 16.1 to 16.7 percent). However, I find little evidence that contraband seized decreased. These findings imply that the policy did not lead to significant adverse effects regarding police officers finding less severe contraband.

Finally, I find a statistically insignificant relationship between the pretextual stop restriction and the number of reported crimes in the long term. Moreover, I find no evidence that traffic accidents increased in Los Angeles City or County. These findings suggest that the prohibition of pretextual stops did not adversely impact public safety as measured by the number of traffic accidents or reported crimes.

My paper contributes to the literature in several ways. My first contribution is exploring multitasking behavior and potential trade-offs within the context of police-initiated stops. Much of the existing literature provides empirical evidence supporting that police officers respond to higher incentives or higher costs (Chalfin & Gonçalves 2023; Kim 2022; Luh et al. 2023; Makowsky & Stratmann 2009; Makowsky et al. 2019). However, few studies investigate whether exerting more effort on one task reduces the quality or quantity of other tasks.[1] Garoupa & Klerman (2002) provides theoretical modeling predicting police multitasking among which types of crime to target. Their findings suggest potential trade-offs between major crimes vs. minor crimes. However, their results depend on several conditions, including the offender's wealth and whether the law enforcement is competitive. Reeves (2024) finds that officers reduce traffic enforcement immediately following a collision response, consistent with the multitasking responsibilities that offi-

[1]For instance, Makowsky et al. (2019) finds that officers focus their effort more on arresting for drugs, drunk driving, and prostitution when local fine and forfeiture revenues are higher, but do not study whether this change led to a reduction in other arrests which may not necessarily generate fiscal revenues.

cers face. In this paper, I empirically show whether raising the cost of one type of stop increases the quantity of other types of stops.

This paper also contributes to the literature on the policies that aim to reduce police discrimination. Several studies have explored whether specific policies such as the adoption of body-worn cameras (Ferrazares 2024), Connecticut's collaborative approach (Parker et al. 2024), Consent Decree (Fagan & Geller 2020), mandatory police training (Dube et al. 2023; Mello et al. 2023), federal oversight (Campbell 2023; Long 2019; Shi 2008), and prosecutorial reform aimed to reduce pretextual stops (Naddeo & Pulvino 2023) are effective in curbing racial disparities in police-encounters. I estimate the causal effect of a unique police department-initiated policy reform that aims to improve racial disparities by reducing pretextual stops. The LAPD's policy is unique in two ways. First, this restriction is a department-wide policy that increases the oversight of police officers and requires a police officer to be held accountable for breaking the policy. For this reason, unlike other policies (e.g., Naddeo & Pulvino 2023), which indirectly affect officers, this policy may directly impact police behavior. Second, this policy does not entirely prohibit police officers from stopping a driver for a specific type of violation, and stops due to equipment or non-moving violations do not entirely disappear. Understanding how this policy impacts racial discrimination can have valuable policy implications, as other jurisdictions (i.e., Denver, CO) are implementing such policies to reduce racial disparities.

The third contribution of this paper is exploring how police enforcement practices affect criminality. While increased police presence reduces criminality and dangerous driving (Chalfin et al. 2022; DeAngelo & Hansen 2014; Evans & Owens 2007; Levitt 2004; Matsuzawa 2022; Mello 2019), some policing strategies may not be as effective as others (Abrams et al. 2023; Banerjee et al. 2019; MacDonald et al. 2016; Tebes & Fagan 2022).[2] Naddeo & Pulvino (2023) and Parker et al. (2024) both leverage a policy change that re-

[2]For instance, Banerjee et al. (2019) use a randomized control trial and document that DUI checkpoints placed in fixed locations do not affect drunk driving rates. However, they find that randomly assigned DUI checkpoints have a deterrent effect.

sulted in fewer pretextual stops and find little evidence that crime or traffic accidents changed, suggesting that pretextual stops may not be a valuable tool to promote public safety. Because my policy led to police substitution behavior where the enforcement of other traffic violations increased, I contribute to this literature by investigating whether marginal pretextual stops are effective or ineffective relative to other traffic enforcement.

Finally, I explore pretextual stops in an unexplored setting. Previous studies, which have shown that pretextual stops are ineffective and inequitable, have focused on various jurisdictions, including Louisville, KY (Makofske 2023), Saint Paul, MN (Naddeo & Pulvino 2023), and state of CT, TX, and WA (Feigenberg & Miller 2023; Parker et al. 2024; Rushin & Edwards 2021). I complement their findings by focusing on a different jurisdiction. Los Angeles is the second largest city in the U.S., has the highest number of Hispanic residents, and is significantly more racially diverse than other cities previously examined. Moreover, Los Angeles is known for its high prevalence of driving. Finally, the neighborhoods within the city vary significantly in terms of their characteristics, including public safety, gang presence, and socioeconomic status.[3]

The remainder of my paper is formatted as follows: Section 2 discusses the background of the policy and theoretical prediction of the policy. Section 3 discusses the data. Section 4 discusses my identification strategy. Section 5 presents the results and Section 6 concludes.

2 Background

2.1 Background on Policies Surrounding Pretextual Stops

Police-initiated stops for suspicious activities have been ongoing for at least 60 years. For instance, in 1963, a police officer suspected that three men were about to commit a

[3]For example, according to the 2022 Census, median household income in Los Angeles by zip code ranged from $50,000 to more than $200,000.

robbery, so the officer stopped and detained the suspects. This detainment ultimately led to the police officer finding illegal weapons and arresting two of the suspects. The arrest led to a 1968 *Terry v. Ohio* U.S. Supreme Court case. In this court case, the defendant argued the constitutionality of stopping an individual without a warrant and that it violated the Fourth Amendment rights. The court ruled that police officers can stop and search a person as long as they have “reasonable suspicion."

The U.S. Supreme Court again debated the constitutionality of pretextual stops and stop-and-search after traffic violations in the 1996 *Whren v. United States* court case. In 1993, a police officer stopped a car driving in a high-drug neighborhood for “unreasonable" speed without using their turning signals. The result of the stop was that the officer found drugs and arrested the driver and the passenger. The defendant argued that the traffic stop was a pretext to investigate possible drug crimes without probable cause. However, the U.S. Supreme Court judges unanimously decided that violating any traffic laws constitutes a legitimate reason to stop a driver.

While the U.S. Supreme Court allowed police officers to use pretextual stops and to stop a driver for any traffic violations, recently, many cities and jurisdictions began prohibiting such traffic stops.[4] These bans occur through several ways. First is through a legislative change where a law explicitly prohibits police officers from stopping drivers for specific traffic violations.[5] Another way is through a more indirect way where prosecutors stopped prosecuting felonies that arise from minor traffic violations. These policy reforms began, especially with the recent rise of the Black Lives Matter movement, because some policymakers and the public believe that pretextual stops contribute to increased racial discrimination without being effective in finding contraband and promoting public safety.

Los Angeles Police Department (LAPD) is one such jurisdiction that began limiting

[4]Some cities that imposed a ban include Philadelphia, Saint Paul, Minnesota, and Pittsburgh.

[5]For example, in Philadelphia, the new law states that police officers can no longer stop drivers for a single brake light or headlight out.

the use of pretextual stops.[6] On February 1, 2022, LAPD announced that they are considering the restriction of pretextual stops because they believe that pretextual stops are ineffective and have undermined public trust in the police. After this announcement, the police chief collected public opinion on this matter via email. Between February 1 and 15, LAPD received 123 emails, of which 78 rejected the initial proposal and demanded more stringent measures of banning pretextual stops.[7] On March 1, 2022, the five-member Los Angeles Police Commission unanimously approved the new policy, which aims to limit such stops without eliminating them entirely.

This new policy, effective immediately, now requires police officers only to stop a driver if the driver is a public threat. When a police officer makes a stop, he must articulate a valid reason for stopping in their body-worn cameras.[8] Unlike other city's policies where a stop for certain traffic violations is prohibited, under this new policy, a police officer can still stop for any traffic violations as long as they can articulate why such traffic violations constitute a public threat. A violation of this new policy – failure to articulate the reason – leads to disciplinary consequences, including mandatory counseling and retraining. This policy was intended to be a compromise between the public's demand to fully eliminate pretextual stops and the demand of the police union, which still wanted the right to conduct pretextual stops.

2.2 Economic Framework

To theoretically predict the effect of LAPD's pretextual stop limitation on policing behavior, I develop a simple model about the officer's decision to stop a driver. According to the models by Anwar & Fang (2006), Abrams et al. (2023), and Feigenberg & Miller (2023),

[6] Following the LAPD's policy, on January 1, 2024, California also imposed a statewide restriction in a similar fashion as the LAPD.

[7] Some examples of the emails can be found at https://lapdonlinestrgeacc.blob.core.usgovcloudapi.net/lapdonlinemedia/2 Set-Pretext-Emails-Feb-14-Feb15-Redacted_.pdf.

[8] For instance, a person's race, homeless circumstance, or presence in a high-crime location is not a valid reason.

the officer will stop or search a driver if the benefit of such actions outweighs the cost. I build upon these models by adding an additional layer of costs: the opportunity costs of making such stops. Due to time constraints, if an officer stops the driver he currently observes, he has to forgo the next driver he will observe and can potentially stop.

Suppose an officer encounters a random driver and decides whether to stop or let go of that driver. Following the literature, I assume that the objective function of the officer is to maximize his total utility from stopping and searching a driver minus some costs. An example of such utility includes some benefits that the officer receives from writing a ticket or successfully finding contraband, both of which may lead to a higher probability of the officer getting promoted or getting a higher salary (Join LAPD 2023). An example of the costs is the officer's effort when making these stops.

Let driver i be a random driver that the officer observes. For simplicity, suppose that this driver can be grouped into four groups by type of violations, denoted as t, and by race, denoted as r. I suppose that the two types of traffic violations are minor traffic violation, which is more likely to be pretextual stops, (m) or other traffic violation (o), and the two races are white (w) or black (b).[9] The officer observes each group with a probability ϕ_{tr}. Upon observing the driver, the officer also observes an idiosyncratic signal of the total benefit of stopping and searching that driver, denoted as υ_i.[10] I assume that υ_i is distributed by some random density function that differs between groups, which I denote as $f(\upsilon_i|t,r)$.

The officer's utility for stopping an individual (i) who is committing traffic violation (t) and who is race (r) is as follows:

$$U(\upsilon_i, t, r) = \max_{\substack{\text{stop} \\ \text{let go}}} \{\upsilon_i - c_t, E(U_2)\} \tag{1}$$

[9]Minor traffic violation includes equipment or non-moving violation. Other traffic violation includes moving violation such as speeding.

[10]For example, the officer will observe the severity of the offense (e.g., speed). The officer can use this information to assess how much potential fines he can give to the driver.

In Equation (1), c_t is the direct cost of making each type of traffic stop.[11] $E(U_2)$ is the expected utility the officer receives from stopping the next random driver that he observes (i.e., focusing his effort on the next task he is provided). I set $\beta = 1$, which implies means that the officer is indifferent between stopping the same driver right now vs. in the next period (i.e., a few minutes later), ceteris paribus.

In other words, the maximization problem in Equation (1) suggests that the officer is choosing between doing the task today (and receiving some benefit from stopping the current driver) or waiting and doing the task in the future (and receiving the expected net benefit from the next driver). Under this maximization problem, I can solve for the officer's decision to stop the driver as follows:

$$\text{Stop if } v_i > c_t + E(U_2)$$
$$\text{Let go if } v_i < c_t + E(U_2).$$

The officer will only stop the driver if the net benefit from stopping and searching exceeds a threshold, defined as the total (direct and opportunity) costs of stopping a driver.[12] This finding is consistent to the officer-decision rules shown in Anwar & Fang (2006), Abrams et al. (2023), and Feigenberg & Miller (2023).

Using this decision rule, the probability that the officer stops different types of drivers will become the probability that v_i exceeds a specific stop threshold. Mathematically, I can write the probability of the officer stopping an individual who is violation type (t) and race (r) as follows:

$$P(\text{stop}|t, r) = \int_{c_{t*}}^{\infty} f(v|t, r)dv = 1 - F(c_t^*|t, r),$$

[11]The cost may also vary across individuals (e.g., some individuals may be more violent against an officer than other drivers). I include any of these individual-specific costs in the v_i term. Moreover, I assume that the costs of making each stop are homogeneous across races. My results are qualitatively similar if I assume heterogeneous costs (and heterogenous policy shocks) across races.

[12]The direct costs (c_t) include the disutility from putting effort into making these stops. The opportunity costs include the foregone benefit from not stopping the next driver.

where $c_t^* = c_t + E(U_2)$.

To predict how the probability of stopping a certain group (e.g., $P(\text{stop}|t = m)$) changes as a result of a policy restricting pretextual stops, I make two assumptions regarding the LAPD's policy. First, I assume that the policy increased the direct costs of stopping the driver for minor traffic stops because the officer is required to exert more effort in making minor traffic stops (i.e., articulating the reason), and the policy increased scrutiny. Mathematically, I assume that $c_m^{new} > c_m^{old}$ or $dc_m > 0$. In addition, because the policy increased the costs of stopping the driver for minor traffic stop violations, the expected utility from alternative tasks – which may, in some chance, include observing and stopping another driver violating minor traffic stops– decreases. However, I assume that the rate of decrease is less than one because there is some chance that the next driver may not be committing other traffic violations, in which the costs (and hence the net benefit) remain constant. Mathematically, I assume that $-1 < dE(U_2)/dc_m < 0$. Under these two assumptions, I can predict the change in the probability of stopping a specific type of driver.

Prediction 1: LAPD's pretextual stop limitation will lead to fewer minor traffic stops and more stops for other traffic violations (e.g., substitution from one task to another).

To make this prediction, I first solve for the probability that the officer observes a driver violating a minor traffic (or other traffic) violation and stops that driver. Next, I take the derivatives of each probability with respect to c_m. As shown in Appendix A.1, mathematically, I have the following:

$$\begin{aligned}\frac{dP(\text{stop}|t = m)}{dc_m} &= \sum_r -\phi_{mr} f(c_t^*|t = m, r)(1 + \frac{dE(U_2)}{dc_m}) < 0 \\ \frac{dP(\text{stop}|t = o)}{dc_m} &= \sum_r -\phi_{or} f(c_t^*|t = o, r)(\frac{dE(U_2)}{dc_m}) > 0.\end{aligned} \tag{2}$$

Equation (2) implies that the policy reduces the probability of stopping a driver for a minor traffic violation. On the other hand, Equation (2) also implies that the likelihood of stopping a driver for other traffic violation increases when the cost of minor traffic stops (or alternative tasks) increase. Intuitively, an officer will reduce the number of minor traffic stops as the total costs for these stops increase. However, the higher costs of making minor traffic stops lower the opportunity costs of stopping drivers for other violations, as the benefits from alternative tasks (i.e., stopping another driver for a minor violation) diminish. As a result, an officer is more likely to stop drivers committing other traffic violations instead of waiting for the next driver to stop. This prediction is consistent with multitasking theory, where higher costs for one task shift efforts to alternative tasks.

Prediction 2: The aggregate effect of LAPD's pretextual stop limitation on racial disparity is ambiguous. The sign of the effect depends on (i) the probability density function around the pre-policy threshold and (ii) the probability that the officer observes each violation type.

To make this prediction, I solve for the probability that the officer observes Black drivers and stops that driver. As shown in Appendix A.2, the derivative of this probability with respect to c_m will become:

$$\begin{aligned}
\frac{dP(\text{stop}|r=b)}{dc_m} &= -\phi_{mb} f(c_m^*|t=m, r=b)(1+\frac{dE(U_2)}{dc_m}) - \phi_{ob} f(c_o^*|t=o, r=b)(\frac{dE(U_2)}{dc_m}) \\
\frac{dP(\text{stop}|r=b)}{dc_m} &< 0 \text{ if } \frac{\phi_{mb} f(c_m^*|t=m, r=b)}{\phi_{mb} f(c_m^*|t=m, r=b) + \phi_{ob} f(c_m^*|t=o, r=b)} < |\frac{dE(U_2)}{dc_m}| \\
\frac{dP(\text{stop}|r=b)}{dc_m} &> 0 \text{ if } \frac{\phi_{mb} f(c_m^*|t=m, r=b)}{\phi_{mb} f(c_m^*|t=m, r=b) + \phi_{ob} f(c_m^*|t=o, r=b)} > |\frac{dE(U_2)}{dc_m}|
\end{aligned} \tag{3}$$

The prediction in Equation (3) indicates that the probability of Black drivers being stopped depends on four parameters: ϕ_{mb}, ϕ_{ob}, $f(c_m^*|t=m, r=b)$, and $f(c_o^*|t=o, r=b)$. The derivative $\frac{dP(\text{stop}|r=b)}{dc_m}$ decreases as ϕ_{mb} increases and increases as ϕ_{ob} becomes larger. This condition suggests that, all else equal, if an officer is more likely to observe Black drivers committing minor traffic violations, the likelihood of a stop decreases. Conversely, if an officer is more likely to observe Black drivers committing other violations, the probability of a stop increases.

The remaining two parameters, $f(c^*_m|t = m, r = b)$ and $f(c^*_o|t = o, r = b)$, represent the probability density functions for Black drivers violating minor and other traffic violations. The derivative in Equation (3) decreases with the former and increases with the latter, implying that if more drivers are near the threshold for being stopped for minor violations than for other violations, the overall number of Black drivers getting stopped may decline.

Intuitively, the overall change in the number of Black drivers stopped is ambiguous due to police substitution behavior, as uncovered in prediction 1. While the number of Black drivers stopped for minor traffic violations will decrease, this decrease may be offset by increased stops for other violations. Consequently, the theoretical predictions suggest that the policy may not fully achieve its intended goal of reducing racial disparities in police traffic stops.

Because I cannot confidently theoretically predict whether LAPD's policy was successful in reducing racial disparities in traffic stops, I next turn to empirical analyses to investigate this question.

3 Data

3.1 Traffic Stop Data

In 2015, the California legislature passed Assembly Bill 953, the Racial and Identity Profiling Act (RIPA) that requires state and local law enforcement agencies to collect data on all vehicle and pedestrian stops, regardless of the outcome of the stop[13;14] The data

[13]The basis for this law was to identify and eliminate racial biases in traffic stops.
[14]A stop is defined as any police-initiated detention or search.

collection began and started getting reported in July 2018 from the largest police departments with more than 1,000 peace officers.[15] In January 2019, mid-sized police agencies with 667 to 1,000 peace officers began collecting data. By January 1, 2022, over 500 law enforcement agencies in California were reporting data.

The RIPA traffic stop data for all of California between July 2018 and December 2022 is available from California's Department of Justice (DOJ). While DOJ provides complete information for all cities and agencies that report to RIPA, the reporting comes with a lag, with the most recent data only being updated up to 2022. Moreover, the data provided by the DOJ does not include some publicly available details, such as the location of stops, that other data sources provide. To remedy this concern, I utilize the most updated city-level data and only supplement it with DOJ data when the city level is unavailable. For example, I obtain my primary RIPA traffic stop data from the city of Los Angeles open data portal.[16]

Across all cities, the data from the RIPA is at the individual level and provides various information about each stop. First, officers must provide information on all people's perceived demographics, including gender, race, and ethnicity. Second, officers are required to report the reasoning behind the actions taken, such as the violation code for the basis of a stop and why the officer searched. Finally, they also report the outcome of each stop, including whether they found any contraband during the search and whether they arrested, cited, or warned the driver.

Using RIPA data, I construct police agency-by-day panel data.[17] My first set of outcomes is the total number of traffic stops due to (i) minor traffic violations, (ii) other mi-

[15]The eight agencies include California Highway Patrol, City of Los Angeles, San Diego, and San Francisco police departments, and the sheriff departments of Los Angeles, San Bernardino, Riverside, and San Diego Counties.

[16]The data is available at: https://data.lacity.org/Public-Safety/LAPD-RIPA-AB-953-STOP-Person-Detail-from-7-1-2018-/bwdf-y5fe and https://data.lacity.org/Public-Safety/LAPD-RIPA-AB-953-STOP-Incident-Details-from-7-1-20/5gp9-8nrb

[17]I also experiment with weekly and monthly levels. Because treatment happens on Tuesday, for the weekly level, I define a week as starting on Tuesday and ending on Monday.

nor moving violations, and (iii) any violations, which I convert to the rate per 100,000.[18] I define minor traffic violations, which are "likely pretextual stops," as those due to any non-moving or equipment violation. While the raw data indicates whether a traffic violation is classified as a moving, non-moving, or equipment violation, this classification can be inconsistent across officers and agencies. For example, if an officer stops a moving vehicle with a broken taillight, he might record this stop as an equipment violation. However, sometimes, he might record the stop as a moving violation because the car was in motion. To address this inconsistency, I use the most frequent classification for each vehicle violation code to determine whether I should categorize it as a moving, non-moving, or equipment violation.[19] Some common examples of likely pretextual or minor traffic stops are broken taillights and expired, missing, or non-visible registration tags.[20] I also define "other minor moving violations" as any moving infraction violation, such as failure to stop at a stop sign or speeding. Officers generally consider these violations less severe and use more discretion when deciding whether to initiate a stop. Finally, stops due to any violations are simply the total number of stops made on a given day.[21]

My next set of outcomes is the rate of non-Hispanic White, non-Hispanic Black, and Hispanic people stopped per 100,000 people.[22] In addition, I also measure various traffic stop outcomes. First, I count the total number of stops by whether the stop led to (i) a warning, (ii) a citation, (iii) use-of-force, or (iv) arrest, and convert that to rate per 100,000

[18] For a city police agency, I use that city's population as the denominator. For county sheriff's offices, I use that county's population, and for state highway patrol, I use the state's population to calculate the rate per 100,000.

[19] For example, if a particular violation is recorded as an equipment violation 70 percent of the time and as a moving 30 percent of the time, I classify it as an equipment violation.

[20] Appendix Table B.1 provides a full list of vehicle violation codes and descriptions of offenses classified as minor traffic stops.

[21] All traffic stops include the two types of stops and also other types of stops, such as major traffic violations, drunk driving, or stops due to suspicious activities. These violations are severe, and police officers will likely stop a driver upon observing such violations. My results show that LAPD's policy did not affect these stops.

[22] The total number of people stopped is not equivalent to the total number of stops, and that is why I am using a different phrase between the stop and race outcome. For instance, if a stop involves two individuals, I have one total stop but two people stopped. My results are qualitatively similar when my outcome is counting the total number of individuals stopped.

population. Second, I measure the number of person or vehicle searches per 100,000. Third, I also calculate the total number of stops that led to discovering contraband and the total number of stops that resulted in seizing contraband.[23;24] I also convert these outcomes to the rate per 100,000 population. Fourth, I calculate the hit rate, defined as the total number of searches that yielded at least one contraband discovered (or seized) divided by the total number of searches. Finally, I also calculate the average stop time per stop, measuring the time police officers spend per stop.

In Appendix Table B.2, I present the descriptive pre-treatment (October 1, 2021, to February 28, 2022) characteristics of these two types of traffic violations. I document a few findings. First, I find that minor traffic stops account for approximately 31 percent of all traffic stops. Second, Black or Hispanic individuals are more subject to stops for minor traffic violations than other traffic violations (85.2 vs. 69.0 percent). Third, minor traffic violations are more likely to end up in a warning (76.8 vs. 34.1 percent) or searching (27.8 vs. 12.9 percent) and less likely to end up with a citation (28.3 vs. 66.3 percent). These findings imply that the officer's objective function when conducting these two stops may differ. Finally, while the search rate is different across the two types of violations, contraband discovery rates are relatively similar between the two types of traffic stops (23.6 vs. 25.6 percent), implying that the efficiency of searches may not necessarily be different.

3.2 Other Datasets

To supplement my analysis of traffic stops, I obtain other information from various datasets. The city of Los Angeles' open data portal offers valuable datasets about the city. First, I obtain two types of crime data: (i) incident-level crime data for reported offenses

[23] One downside with the RIPA data is that it only indicates whether officers seized any contraband. This shortcoming limits me to count the number of contrabands discovered or seized precisely.

[24] Only about 13 percent of stops with contraband discovered also lead to property seized.

and (ii) arrest data.[25] The former dataset provides information on crime that LAPD reported, regardless of whether the case was solved, but only is restricted to severe crime.[26] The latter dataset provides information on all arrests made by the LAPD and on less severe crimes, such as drunk driving. Using these datasets, I construct daily time series data of the reported crimes and arrests.

To supplement my crime analysis for Los Angeles, I also obtain crime data from the California Data Justice Open portal and the Uniform Crime Report (UCR) to increase the size of my control jurisdictions.[27] Both datasets provide monthly counts of reported index crimes and cleared crimes.[28] Using these data, I construct a police agency-by-month and county-by-month panel of reported crime and cleared crime.[29] My sample window starts in June 2021 to ensure a sufficient pre-treatment period without contaminating it with the COVID-19 period and covers up to December 2022. For my agency-level analysis, I restrict my sample to large police agencies covering more than 100,000 people and report crime in all 19 months during my sample window to ensure I have a balanced panel and can interpret zero crime as truly zero crime happening and not missing. For my county-level analysis, I first restrict my police agency to those reporting all 19 months for the same reason stated earlier and to ensure that the sample within each county stays constant over time. Furthermore, after the aggregation into county-level data, I restrict my sample to large counties where the total population covered by the police agency in my sample exceeds 100,000. My primary sample includes 371 police agencies and 412 counties across the U.S.

I collect information about traffic accidents from the California Statewide Integrated Traffic Records System (SWITRS) to measure any changes in traffic safety. The SWITRS is

[25] These data are available at https://data.lacity.org/Public-Safety/Crime-Data-from-2020-to-Present/2nrs-mtv8 and https://data.lacity.org/Public-Safety/Arrest-Data-from-2020-to-Present/amvf-fr72.

[26] For instance, drunk driving is not included as part of the offense.

[27] Ideally, I want to focus on using the UCR. However, the LAPD does not consistently report to the UCR.

[28] Index crimes are violent crime (aggravated assault, murder, robbery, and rape) and property crime (arson, burglary, larceny-theft, and motor vehicle theft).

[29] Reported crime is any known crime to the police. Cleared crime is any crime the police solved through arrest or exceptional means.

a database that collects and processes data gathered from a collision scene and provides incident-level information on traffic accidents in California. Using SWITRS, I construct a police agency-by-month and county-by-month panel of total traffic collisions, traffic collisions from speeding, and other vehicular violations such as improper turning.[30] I follow the same sample cuts as the UCR outlined earlier. My primary sample includes 85 police agencies and 35 counties across California.[31] Because SWITRS data covers up to 2023, my sample window includes 2023 to ensure a longer post-treatment window.

4 Methods

4.1 Short-run Estimation

I begin by estimating a regression discontinuity in time (RDiT) model. The main idea of RDiT is to compare the changes in my outcomes right around the treatment window of March 1, 2022. One advantage of using an RDiT is that it only requires data for the treatment unit. Thus, RDiT will be the only feasible estimator for certain outcomes, such as arrests, where the data for counterfactuals may not exist.

To estimate an RDiT, I implement a two-step augmented local linear methodology proposed by Hausman & Rapson (2018). First, using daily data from the full sample period between July 1, 2018, and December 31, 2022, I identify important regressors by estimating the following model:

$$Y_t = \alpha_0 + \alpha X_t + \gamma_d + \rho_m + \phi_y + SIPO_t + u_t. \tag{4}$$

In Equation (4), Y_t is my outcome variable of interest at the daily level (t). X_t is a vec-

[30] I note that not all traffic accidents are due to speeding or other vehicular violations because, for example, accidents due to equipment failure can happen when the driver still obeys traffic rules.

[31] Because many traffic accidents occur on highways, the information reported by California highway patrol may be helpful. Hence, for accidents reported by California highway patrol, I split my sample into California highway patrol agencies that cover Los Angeles and those that do not.

tor of control variables, including average daily temperature, average daily precipitation, and total vehicle miles driven.[32] γ_d is the day of the week fixed effects, ρ_m is the month fixed effects, and ϕ_y is the year fixed effects. Because my full sample includes 2020, when COVID-19 was happening, I also include controls for California's COVID-19 stay-at-home orders, defined as $SIPO_t$.[33]

Using the residuals from Equation (4), I estimate the main regression discontinuity specification:

$$e_t = \beta_0 + \beta_1 Post_t + \beta_2\, t + \beta_3 Post_t \cdot t + \varepsilon_t. \tag{5}$$

In Equation (5), e_t is the residual. $Post_t$ is a dichotomous treatment variable denoting whether the LAPD limitation went into effect. t and $Post_t \cdot t$ are linear functions of the running variable, in which the slopes vary before and after the treatment. To account for autocorrelation in the treatment and outcome, I conduct my statistical inference using Newey-West standard errors (Newey & West 1987; Newey & West 1994).[34] To ensure consistency across my various outcome variables, I use a bandwidth of 2 months.[35]

The advantage of using augmented local linear specification is that it increases statistical power relative to a traditional local linear approach. With a traditional local linear approach, I need to separately identify the "Wednesday" effect from the treatment effect of interest. Moreover, because traffic stops can have seasonality, this approach helps me net out March seasonality effects, which is impossible with a traditional local linear because since the treatment occurs on March 1, including March fixed effects with only 2022 data will soak up all my variation. Finally, with augmented local linear, I do not need to worry about including higher-order polynomials for my running variable, which can

[32] The data on temperature and precipitation come from NOAA, and the data on vehicle miles come from CalTrans.

[33] My findings are robust to: (i) excluding time window during COVID-19 stay-at-home-order was in place, (ii) excluding 2020, and (iii) restricting the sample to just 2019.

[34] My estimates are more precise when I estimate heteroskedastic-robust standard errors, which is mathematically equivalent to clusterring around the running variable (time) (Lee & Lemieux 2010)

[35] Appendix Figure B.1 plots the optimal bandwidth for various outcome variables I examined. These bandwidths ranged from 35 to 100 days.

increase the chances of overfitting (Gelman & Imbens 2019).

In Equation (5), my main parameter of interest is β_1, which can be interpreted as the immediate effect of a policy change. The underlying assumption for β_1 to be causal is that there are no other changes in the disturbance term (ε_t) at the cutoff date. Because I aggregate my data to the daily level and the treatment occurs at the beginning of the month, one potential threat to identification can be any changes in enforcement at the beginning of the month vs. the end of the month. Another potential threat to identification is anticipation effects. For instance, I cannot have police officers change their behavior due to the anticipation of such a policy. I conduct several tests to rule out these concerns. First, I experiment with aggregating my data to a lower frequency level, such as the weekly or monthly level.[36] This aggregation helps me soak up any cyclical day-to-day variation. Second, I also experiment with a donut RD where I exclude the 30 days around the threshold, which includes the time window between policy announcement and policy enactment where the anticipations, if any, are most likely to happen.[37]

In addition to a regression discontinuity model, I also experiment with a difference-in-discontinuities estimate (Hansen et al. 2020). The main idea of a difference-in-discontinuities approach is that I compare how discontinuity in LAPD is different relative to a placebo group using other California police agencies.[38;39] To estimate a difference-in-discontinuities model, I estimate the following equation:

$$\begin{aligned} e_{it} =& \beta_0 + \beta_1 Post_{it} + \beta_2 t + \beta_3 \, Post_{it} \cdot t \\ & + \alpha_1 Treat_i \cdot Post_t + \alpha_2 Treat_i \cdot t + \alpha_3 Treat_i \cdot \; Post_t \cdot t \; + \gamma_i \; + \upsilon_{it}. \end{aligned} \tag{6}$$

In Equation (6), e_{it} is the residuals from estimating Equation (4) for police agency i in

[36] Because treatment starts on March 1, 2022 (Wednesday), I define a week starting from Wednesday and ending on Tuesday.

[37] In addition, because the Super Bowl happened in Los Angeles on February 13, 2022, a donut RD can also test if the Super Bowl effect is driving my results.

[38] For my main placebo group, I use 7 police agencies where the RIPA data is available back to 2018.

[39] My results are qualitatively similar when I use LAPD, defining treatment happening in 2019 as my counterfactual.

time t.[40] $Treat_i$ is an indicator variable, which is one for LAPD and zero for other police agencies. γ_i is unit fixed effects. α_1 is the parameter of interest where I compare how the discontinuity differs between LAPD and other placebo units. In this equation, I allow the slope of the running variable to differ between pre-treatment period, post-treatment period, and by treatment unit vs. control unit. This approach will net out any changes around the cutoff, assuming these changes are common across California. I continue my inference using Newey-West standard errors.

4.2 Long-run Estimation

One downside of a regression discontinuity approach is that the Local Average Treatment Effect (LATE) is only specific to the immediate impact around the treatment time. Thus, this model will fail to detect any time-varying, long-run, and dynamic effects of the treatment. While changes in policing behavior may be immediate, changes in some outcomes, such as crime deterrence, may take some time to happen. For this reason, I will next leverage panel data with untreated jurisdictions to estimate the longer-run impact of the policy. Intuitively, I compare how the outcomes in LAPD or Los Angeles changed in the post-treatment period relative to the counterfactual units. Mathematically, I estimate the following model:

$$\underset{\beta_0,\beta_1,\gamma_i,\gamma_t}{\arg\min}\{\sum_{i=1}^{N}\sum_{t=1}^{T}(Y_{it} - \beta_1 LA_i \cdot Post_t - \gamma_i - \rho_t)^2\omega_i\lambda_t)\}. \tag{7}$$

In Equation (7), Y_{it} is the outcome for unit (police agency or county) i in month t. LA_i is a dichotomous variable indicating if the police agency (or county) is LAPD (or Los Angeles County).

A natural question posed when estimating Equation (7), especially with only one treat-

[40]To allow for seasonalities or the effects of weather to vary across agencies, I estimate Equation (4) for each police agencies separately and store their residuals. My baseline estimates are qualitatively similar when I estimate one Equation (4) for all police agencies with agency fixed effects included instead.

ment unit, is what the counterfactuals should be and how much weight each control unit (ω_i) and each time period (λ_t) should receive. To analytically determine the optimal weights for counterfactuals, I estimate Equation (7) using Synthetic Difference-in-Differences (SDiD) where the optimal unit and time weights are computed by minimizing the sum of squared errors between the observed and predicted values (Arkhangelsky et al. 2021). To ensure that the choice of my counterfactuals does not drive my estimates, I also experiment with an unweighted two-way fixed effects (TWFE) difference-in-differences estimator. Mathematically, this estimation is the same as regressing Equation (7) where I use equal unit and time weights for all observations.

Because there is only one treatment unit, I conduct my inference for these estimations using permutation-based p-values. To implement the procedure, I estimate placebo β_1 estimates by estimating additional regressions in each case, replacing the treatment with an indicator for one of the other control units. To calculate the p-values, I can rank the treatment effect and find the relative rankings of Los Angeles. I note that this is a very demanding test, especially when the number of control units is low.[41] For this reason, I supplement my inference using a standard t-test, calculating the standard errors by taking the standard deviations of placebo estimates. This inference assumes homoskedasticity of error term across units (Arkhangelsky et al. 2021).[42] In a supplemental test, I also experiment with conducting hypothesis testing using a rearrangement test (Hagemann 2020). The benefit of such a test is that it allows for inference when I only have one treatment unit and heteroskedasticity of unknown form. However, a downside of such a test is that this test applies to unweighted TWFE estimates, so I cannot conduct inference for synthetic difference-in-differences.

[41]For example, if I have only 30 control units, to achieve statistical significance, this test would require the treatment estimate to be at least 3rd largest in absolute magnitude.

[42]A procedure proposed by Arkhangelsky et al. (2021) draws 500 placebo estimates and taking the standard deviations of the estimates. However, in the case of one treated unit, 500 unique placebo units are not plausible because I can only estimate the placebo for the number of controls I have.

5 Results

5.1 Immediate Impact of Pretextual Stop Restriction on Number of Stops

I first focus on the immediate impact of LAPD's pretextual stop limitation on the change in the number of likely pretextual stops or stops for minor traffic violations. In panel (a) of Figure 1, I find a large discontinuity in the number of minor traffic stops. The point estimates from column (1) of Table 1 suggest a statistically significant reduction in minor traffic stops by 1.8 per 100,000 (or approximately 29 percent relative to the baseline mean). Using a difference-in-discontinuity approach (panel II) and netting out the March 2022 effect, I continue to find that the number of "likely pretextual stops" fell by 2.1 per 100,000 (or 32.5 percent). I note that my estimated "first-stage" effect may be smaller than previously reported (Naddeo & Pulvino 2023). However, the magnitude of my estimated effect is plausible due to the nature of the policy I am examining. Because LAPD did not explicitly ban the use of pretextual stops and instead allowed officers to use them under strict conditions, this policy did not completely eliminate the use of pretextual stops, and the estimated effect on likely pretextual stops may have been smaller.

In contrast to minor traffic violations, I find little evidence of discontinuity in the overall number of police-initiated stops. The point estimate from panel I, column (2) of Table 1 suggests that with 95 percent confidence, I can rule out an immediate decrease in all stops by 3.1 per 100,000 (or 15 percent). I note, however, that panel b of Figure 1 suggests that there is a structural break in the trend after the policy enactment, potentially driven by longer-run changes in the number of stops for minor traffic violations (panel a of Figure 1). This finding implies that the policy may have reduced the overall number of traffic stops in the long-run. Nonetheless, the fact that there is no discontinuity for all stops while there is a large discontinuity in minor traffic violations is an economically significant finding. In column (3) of Table 1 and panel (c) of Figure 1, I document that a potential increase in other minor traffic enforcement by 1.5 per 100,000 may explain the small change in the overall number of stops. This finding is consistent with the possibility of police substitution behavior, which I predict in my theoretical model.

In Appendix Figure C.1, I conduct robustness tests for my regression discontinuity design. In the very left estimate, I show my baseline specification for comparison. In the second estimate, I show that my estimates are robust when using higher-order polynomials. In the next four estimates, I experiment using a different bandwidth of (i) a narrower bandwidth of 45 days, a wider bandwidth of 75 and 90 days, and (iii) optimal bandwidth. In the following estimate, I experiment using a donut RD, excluding 30 days before and after treatment. In the final two estimates, I experiment with weekly and monthly aggregates rather than daily aggregations. These figures continue to show a large statistically significant reduction in minor traffic stops, offsetting increases in stops for minor moving violations, and little overall changes in the number of traffic stops. These findings confirm that the manual selection of my bandwidth, anticipation, or time-varying treatment effects are not driving my results.[43]

Another potential explanation for the police substitution behavior I am uncovering is the changes in how police officers are recording the stops. For instance, a police officer may still stop the driver for a minor traffic violation but may record the reason for the stop as a more severe traffic violation. To address that this reporting issue is not a concern, in Appendix Table C.1, I examine spatial and temporal heterogeneity in the estimated effect of the LAPD's pretextual stop limitation. In columns (1) and (2), I find that the increase in other traffic violations only happens during the daytime, where pretextual stops are less likely to happen, but stops for other traffic violations are more likely to occur during (Appendix Figure B.2). Moreover, in columns (3) to (6), I find that the increase in other traffic stops is larger in zip codes with a higher number of traffic violations or in zip codes with a higher number of traffic accidents, where I may expect police officers to be enforcing

[43] In Appendix Figure C.2, I conduct a regression discontinuity assigning March 1, 2019 as the treatment date. This placebo test confirms little March 1st effect happening in LAPD.

traffic more often.[44] These findings suggest that police officers may enforce traffic stops instead of continuing with minor traffic stops but reporting them as speeding infractions.

In Appendix Table C.2, I examine the spatial distribution of traffic stops by neighborhood income and race. In panel I, I find evidence that this policy has decreased stops for minor traffic violations or likely pretextual stops at a higher rate in low-income zip codes (28.7 vs. 22.9 percent) and in zip codes with a higher share of racial minorities (28.6 percent vs. 23.0 percent), where more pretextual stops were happening during the pre-treatment period. Focusing on heterogeneous treatment effects for the number of stops for all other types of violations (panel II), I do not find heterogeneous treatment effects across high vs. low-income neighborhoods. However, I document that the increase in other traffic enforcement is larger in neighborhoods with more racial minorities.

I investigate the racial impacts of pretextual stop restrictions in Table 2 and Figure 2. In panel I, columns (1) to (3) of Table 2 and panels (a), (c), and (e) of Figure 2, I find that the total number of people stopped for likely pretextual stops significantly fell by 10.6 per 100,000 (or 37.6 percent) and 2.6 per 100,000 (or 30.7 percent) for non-Hispanic Black and Hispanic, respectively, but little decrease for non-Hispanic White.[45] In panel II columns (1) to (3) of Table 2, when using a difference-in-discontinuities approach comparing LAPD to other placebo California police agencies, I continue to find a large reduction in stops for minor traffic violations involving racial minorities. While the difference-in-discontinuities estimate now shows a statistically significant decrease in the number of non-Hispanic White people getting pulled over, the magnitude of the estimated effect, both in levels and percentage points, still shows that racial minorities may have benefitted more from this policy.

In columns (4) to (6) of Table 2 and panels (b), (d), and (f) of Figure 2, I focus on the total

[44] Appendix Figure B.3 shows the zip codes above and below the median for the number of traffic accidents and other traffic violations.

[45] I note that the total number of people stopped are slightly different from the total number of stops, which I have been examining thus far. On average, the number of people involved per stop does not change between pre- and post-period (1.046 vs. 1.051).

number of people stopped for any violation. I find a similar pattern of results as minor traffic violations. The number of Black individuals stopped significantly fell by 11.4 to 13.0 per 100,000 (or 15.0 to 17.2 percent), and the number of Hispanic individuals stopped may have reduced by 1.6 per 100,000. However, the estimates for Hispanic individuals are statistically insignificant. In contrast, I find little change in the number of non-Hispanic White people getting stopped. Taken together, these estimates suggest that the policy limiting the use of pretextual stops achieved one of its intended goals of reducing racial disparities in traffic stops. However, these estimates also suggest that racial disparities persist, as the rate at which officers stop Black individuals remains higher than that of White individuals (64.3 vs. 14.0 per 100,000).

In Appendix Table C.3, I focus on whether there are racial differences in the effects of stops for minor moving violations. While my estimates are imprecisely estimated, my point estimates support the idea that racial disparities regarding the number of traffic stops have reduced. While the number of stops for minor moving violations is increasing for all groups, I find a larger increase for non-Hispanic White individuals (7.4 to 16.6 percent) than Black (1.9 to 5.2 percent) or Hispanic (7.6 to 14.0 percent) individuals. Furthermore, the point estimates suggest that all other stops may have fallen for non-Hispanic Black individuals but not non-Hispanic White individuals; however, none of these estimates are statistically significant.

One concern with examining the racial impact of such policy is that the observed reduction in Black or Hispanic individuals getting stopped can be due to the misreporting of race. For instance, with this new pretextual stop limitation and increased officer accountability, police officers may be more incentivized to misreport the driver's race (Luh 2022). To address this concern, first, I argue that the fact that I do not find large increases in White individuals getting stopped may suggest that police officers are not recording non-White drivers as White drivers. Second, in Appendix Table C.4, I compare neighborhoods with higher versus lower frequencies of stops involving racial minorities before the policy implementation.[46] I find that the reduction in stops for Black and Hispanic individuals is more pronounced in neighborhoods where police officers previously stopped racial minorities at a higher rate. In contrast, there is no significant difference in the rate of change for White stops between these neighborhoods. These differential patterns suggest that the observed reduction in Black or Hispanic individuals getting stopped is unlikely to be due to misreporting of race.

Finally, in Table 3 and Figure 3, I investigate heterogeneous treatment effects across gender. While female and male individuals are being stopped less frequently for minor traffic violations, the reduction is larger for males than females (32.8 to 34.9 percent vs. 20.0 to 27.0 percent). When examining the total number of traffic stops (columns 3 and 4), I observe some evidence that the overall number of stops for male civilians may have decreased, but no changes in the number of female civilians getting stopped. This finding is consistent with the possibility that for male drivers, who are more subject to investigatory traffic stops, LAPD's policy can have heterogeneous effects relative to female drivers (Roach et al. 2022; Smith et al. 2006).

5.2 Immediate Impact of Pretextual Stop Restriction on Stop Outcomes

In the next set of analyses, I investigate whether the effectiveness of traffic stops changed. In columns (1) and (2) of Table 4, and panels (a) and (b) of Figure 4, I first examine whether the number of warnings and the number of traffic citations changed. I document that the number of stops that ended with a warning significantly decreased by 2.5 to 2.9 per 100,000 (or 23.8 to 27.0 percent relative to the baseline mean). On the other hand, I also find that the number of citations issued increased by 1.0 to 1.6 per 100,000 (or 12.9 to 20.3 percent). However, my estimate is only statistically significant at the weakest conventional level for regression discontinuity design (panel I).

These findings from columns (1) and (2) of Table 4 suggest a few valuable insights.

[46] Panel c of Appendix Figure B.3 shows the zip codes above and below the median.

First, these findings continue to show that police substitution behavior is indeed occurring, rather than the officer reporting minor traffic stops as moving violations. Because most minor or likely pretextual traffic stops (48.9 percent) end up with a warning, and most non-pretextual stops and other traffic violations are more likely (53.2 percent) to end up in a citation, the number of citations should increase if police officers enforce other traffic violations. Finally, these findings suggest that while restricting pretextual stops reduced racial disparities, it may had a disproportionate effect on other groups of individuals, such as low-income people, who are more adversely affected by increased citation (Mello 2021).

In columns (3) to (5) of Table 4 and panels (c) to (e) of Figure 4, I examine if search behavior and contraband discovery changed as a result of the policy. I document that police searches decreased significantly by 17.1 percent (or 1.2 per 100,000). Consequently, I also find that the number of contraband the officer found decreased by approximately 16.2 percent. However, I find no evidence that the number of contraband seized decreased. These findings may suggest that the reduction in the number of searches may have little adverse effect because the officer is still finding severe contraband that may pose a serious threat to the community.[47]

In columns (6) and (7) of Table 4 and panels (f) and (g) of Figure 4, I explore the efficiency of search behavior. I find little overall changes when I focus on contraband discovery rates, which I define as the rate of finding something conditional on searches. With 95 percent confidence, I can rule out an increase larger than 4.4 percentage points (or 16.6 percent relative to the baseline mean). On the other hand, I find that contraband seizure rates significantly increased by 33.2 percent. These findings imply that police officers are not efficient in finding contraband but may be more efficient in finding more severe contraband, such as firearms.

[47] In Appendix Table C.5, I disaggregate the contraband that the officer found into firearms, drugs, and other types of contraband. I find that the reduction is driven by drugs (17.6 to 20.8 percent) and other types of contraband (22.1 to 22.4 percent) rather than illegal firearms (2.4 to 6.9 percent).

In the remainder columns and panels of Table 4 and Figure 4, I examine other traffic stop outcomes. In column (8) of Table 4 and panel (h) of Figure 4, I document little change in the average minutes of total officer-civilian interaction. With 95 percent confidence, I can rule out a reduction and increase of more than 3.5 minutes in total time an officer spends during a traffic stop. This null finding suggests that police substitution behavior did not change the time police officers spend on each traffic stop (i.e., from writing more citations), limiting the total hours spent on other enforcement. In panels (i) and (j) of Figure 5 and columns (9) and (10) of Table 5, I investigate whether use-of-force and arrest after police-initiated stops changed. While my estimated effect is consistent with the possibility that the LAPD's pretextual restriction reduced police use of force and arrests, my estimates are imprecisely estimated to draw a firm conclusion.

The analysis, thus far, focused on the change in the number of traffic stops across all types of stops. In Appendix Table C.6, I investigate how the outcome of the traffic stop changed for the different types of traffic stop violations. Overall, minor traffic stops led to a larger reduction in warnings, citations, searches, contraband discoveries, use-of-force, and arrests than other traffic violations. These reductions may be mechanical due to the total number of minor traffic stops, but the number of other traffic violations increased. An economically significant result from these exercises is that the number of warnings, searches, and contraband discovered during other moving traffic stops decreased.

Given that I am uncovering some racial heterogeneity in the number of stops, in Appendix Table C.7, I re-analyze Table 4 columns (1) to (10) by the three race groups. In column (1), I find that while all three groups are experiencing a reduction in the number of warnings, I find that the number of warnings reduced at a higher rate, in both absolute and relative magnitude, for Black (12.0 per 100,000 or 29 percent) and Hispanic people (2.7 per 100,000 or 22.6 percent) than White individuals (0.8 per 100,000 or 20.5 percent). This finding is also consistent with the findings that racial minorities experienced a larger reduction in minor traffic stops and thus have experienced a larger reduction in stops that resulted in a warning. Focusing on the number of citations (column 2), I find that the number of citations may have increased at a higher rate for White individuals (23.1 percent) than Black individuals (9.4 percent), though my estimates are imprecisely estimated.

In column (3) of Appendix Table C.7, I also document some evidence of heterogeneous treatment effects for the number of searches performed. I find that racial minorities experienced a larger reduction in the number of searches performed. This finding also suggests that LAPD's policy improved racial disparities by not just reducing the number of racial minorities getting stopped but also lowering the number of racial minorities being subject to search. In the remaining columns of Appendix Table C.7, I focus on the efficiency of traffic stops. I find some evidence that the officers are discovering less contraband possessed by racial minorities, which is consistent with the earlier findings that the number of searches reduced for racial minorities. Moreover, I find some evidence that the hit rates for White individuals had a larger change (19 percent increase) than racial minorities (5.4 percent increase). However, I cannot rule out if these estimated effects are statistically different across these groups. In the final three columns, I find little significant or meaningful heterogenous treatment effects on average stop time, use-of-force, or arrest rate across races.

5.3 Impact of Pretextual Stop Restriction on Reported Arrests & Traffic Accidents

Given that the number of searches fell, one may be concerned that this policy may lead to fewer detection of crime and less deterrence. In panels (a) to (d) of Figure 5 and columns (1) to (4) of Table 5, I investigate whether restricting pretextual stops affected the number of reported crimes. I find little evidence that crime or arrest rates increased following the policy reform. I find little evidence that the number of arrests and reported crimes increased in Los Angeles in the short run. With 95 percent confidence, I can rule out an 6.8 and 1.8 percent increase in reported property crime and violent crime, respectively.

In panels (e) to (g) of Figure 5 and columns (5) to (7) of Table 5, I examine whether the policy led to an immediate change in traffic accidents. I find no immediate change in traffic accidents following the policy reform. This null effect on traffic accidents suggests that the driver's behavioral changes (i.e., drivers driving more recklessly due to the policy change) did not drive my increase in other traffic violation stops and continues to show support for the police substitution pattern.

While these short-run results provide valuable insights, the analysis thus far captures only part of the picture of whether public safety changed due to restricting pretextual stops. Theoretically, the change in how police officers enforce traffic and the increased number of citations can reduce speeding accidents. However, the effects may be more dynamic and happen in the long term rather than the week of policy implementation. Similarly, criminal behaviors may change over time as people learn about such policies. Thus, I next turn to a difference-in-differences estimator to investigate the dynamic, longer-run impact of restricting pretextual stops.

In Table 6, I present my preferred synthetic difference-in-differences estimates where I find optimal unit weights for each control jurisdiction.[48] In columns 1 to 3, I compare the changes in crime rates reported by the LAPD to those reported by other police agencies. In columns 4 to 6, I compare the changes in crime reported in Los Angeles counties to other counties. In addition, in Appendix Table C.8, I present my TWFE difference-in-differences where my time weights and unit weights are equal across my observations.

I note a few findings. First, in panels I and II, I find similar results when I use all of California vs. the rest of the country, suggesting that the data artifacts from combining two different crime data sources are not driving my results. Second, my results are qualitatively similar between synthetic difference-in-differences and TWFE, implying that the

[48]The unit and time weights used for the synthetic difference-in-differences estimations are available upon request.

data-driven choice of counterfactual units is not driving my results. Third, I note that my estimated coefficient is positive, which is consistent with the possibility that restricting pretextual stops may harm public safety and increase crime, such as burglary or theft, because criminals may have less fear of getting stopped and searched. Another potential explanation for these increases is coincidental increases in crime post-pandemic. Nonetheless, the estimated effect is small (at most a 4.1 percent increase and upper bound of 95 percent confidence interval of 16 percent increase) and statistically insignificant across different inference techniques (p-values ranging from 0.333 to 0.877).[49] Taken together, these findings suggest that restricting marginal pretextual stops had little public safety consequence in terms of increased reported crime in the long run.

In Appendix Table C.9, I present the synthetic difference-in-differences estimates for crime clearance. Another unintended consequence of reducing pretextual stops is that the number of cleared crimes (i.e., solved crimes) can decrease because police officers are not finding conclusive evidence. I find some (7.2 percent) reduction in the total number of clearances for Los Angeles County, but this reduction is not apparent when I focus on LAPD (column 1). Moreover, my estimated effects are imprecise to draw firm conclusions.

One threat to identification is the violation of the parallel trends assumption. In Figure 6, I present synthetic difference-in-differences event studies using the rest of the U.S. as my counterfactuals (Clarke et al. 2023).[50] These figures show little divergence in pretreatment trends between Los Angeles and synthetic counterfactuals, providing evidence supporting the common trends assumption.

Another potential threat to the null effect can be due to long-run changes in traffic stops. For instance, while minor traffic stops sharply declined immediately, this number may have converged back to the pre-treatment levels in the long run. In Appendix Figure B.4, I rule out this possibility. I show that the raw trend in the number of minor traffic

[49] Inference for Appendix Table C.8 using a re-arrangement test (Hagemann 2020) continues to fail to reject the null hypothesis of no effect under all possible maximum relative heterogeneity parameters.

[50] Appendix Figure C.3 show the event studies using only California as my counterfactuals.

stops has reduced and remained low in the post-treatment period, whereas the number of all police-initiated stops remained similar over time.[51]

In Table 7, I present the long-run effect on traffic accidents using synthetic difference-in-differences where I compare LAPD (or Los Angeles County) to other police jurisdictions (or counties) in California.[52;53] Moreover, in Figure 7, I present the synthetic difference-in-differences event study figures. The pre-treatment trend is flat and statistically indistinguishable from zero, suggesting the validity of my parallel trends assumption. Moreover, in the post-treatment window, the estimated effect is negative and somewhat large, implying approximately an 8.0 percent reduction in traffic accidents in Los Angeles County and an 11.6 percent reduction in traffic accidents reported by LAPD. These estimated effects are consistent with the possibility of a potential deterrence effect from changes in police traffic enforcement. However, my estimates from Table 7 are imprecisely estimated, so I cannot draw a firm conclusion on whether deterrence occurred. Nonetheless, these estimates continue to imply little adverse effect of LAPD's pretextual stop limitation regarding increased dangerous driving. With 95 percent confidence, I can rule out an 11.3 percent and 12.5 percent increase in traffic incidents reported by LAPD and traffic incidents in Los Angeles County, respectively.

[51]Estimating a longer-run effect of the change in traffic stops is intriguing. However, I note that the monthly trend in traffic stops in LAPD may be unique relative to other police agencies in California, where the number of traffic stops significantly increases during the summer (panel a of Appendix Figure B.5). I also note that my control variables (i.e., vehicle miles traveled or weather) do not explain these unique summer increases (panel b of Appendix Figure B.5). Thus, a longer-run difference-in-differences style estimation may not be feasible because of the lack of valid counterfactuals and the violation of parallel trends. Nonetheless, in Appendix Figure C.4, I estimate a medium-run effect of LAPD's pretextual restrictions on traffic stops using weekly data from January to April and synthetic difference-in-difference. My findings support the idea that the effect may be persistent rather than temporary.

[52]The unit and time weights used for the synthetic difference-in-differences estimations are available upon request.

[53]Appendix Table C.10 shows the results using TWFE.

6 Conclusion

While many police departments utilize pretextual stops, a considerable number of policymakers question their effectiveness and fairness. Advocates of pretextual stops argue that these stops are indispensable tools for detecting and preventing crime. Thus, they are concerned that removing pretextual stops can increase crime. Conversely, critics argue that these stops contribute to racial discrimination as police officers may disproportionately target racial minorities during these stops.

In this paper, I shed light on this debate by focusing on a unique policy reform that took place in the Los Angeles Police Department (LAPD). This reform restricted the use of pretextual stops, thereby providing a valuable case study for our understanding of the intended and potential unintended consequences of limiting this practice.

Using stop-level data for all police-initiated stops in California and a regression discontinuity estimator, I find that following LAPD's limitation of pretextual stops, there was an approximately 30 percent reduction in minor traffic stops, which are likely pretextual stops. However, I find little evidence that the number of all stops decreased, potentially offset by changes in police enforcement and an increase in other traffic stops. These findings are consistent with the multitasking theory, where higher relative costs of making one particular cost lead to a change in who the officers stop. Focusing on the racial impacts of such policy, I find evidence that this policy led to a 15 to 17 percent reduction in police-initiated stops involving Black civilians but a statistically insignificant reduction in police-initiated stops involving White civilians.

Examining the impact of pretextual stop limitations on traffic stop outcomes and public safety, I document several findings. First, I find evidence that the number of stops that resulted in a warning decreased by 23.8 to 27.0 percent, and conversely, some evidence that the number of citations has increased. Second, I find that the number of searches and contraband found decreased. However, I also find that this reduction in searches led to increased contraband seizure rates. Moreover, I document that this policy change and changes in policing behavior did not lead to statistically significant changes in reported crimes. Finally, I find little evidence that this policy change increased dangerous driving and traffic accidents.

In conclusion, the unique policy introduced by LAPD that restricted the use of pretextual stops achieved some of its goal of reducing, but not entirely eliminating, racial disparities without having many adverse consequences regarding public safety. These findings imply that pretextual stops are more inequitable but not more effective than other police stops, such as enforcing traffic. Taken together, this paper sheds empirical evidence of how increasing scrutiny on some tasks can lead to more socially efficient outcomes.

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8 Figure

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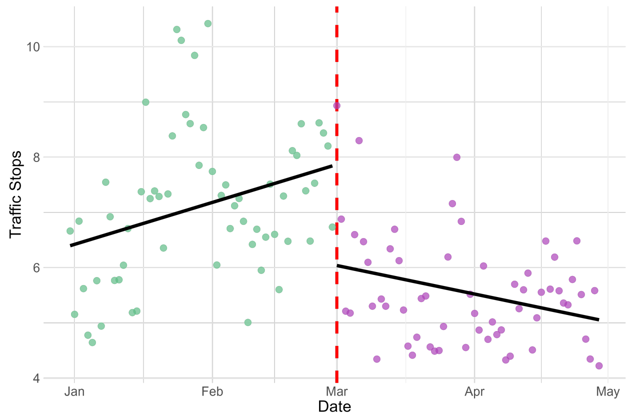
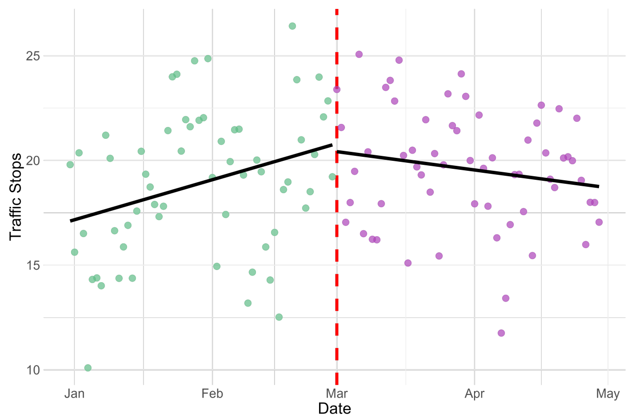
Figure 1: RDiT Estimate: Police-Initiated Stops

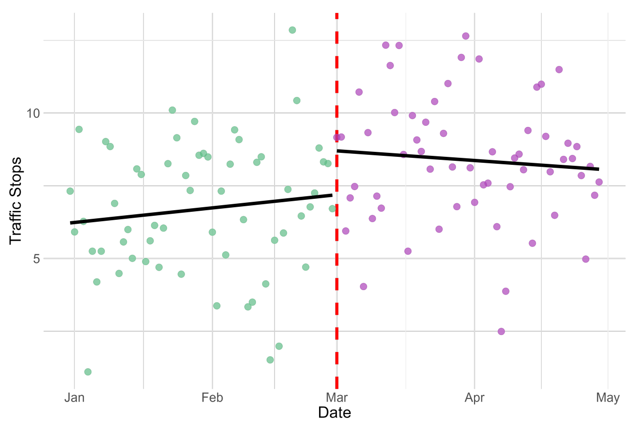
(a) Minor Traffic Violation



(b) All Stops



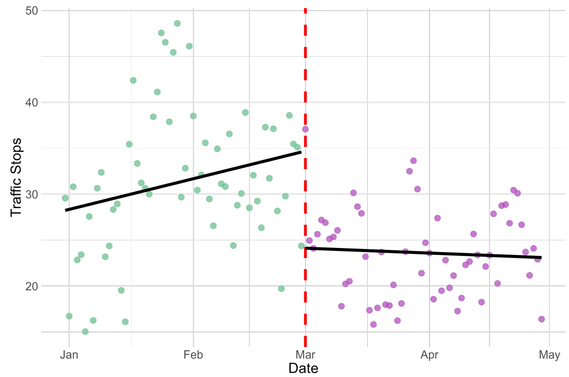
(c) Other Minor Traffic Violation



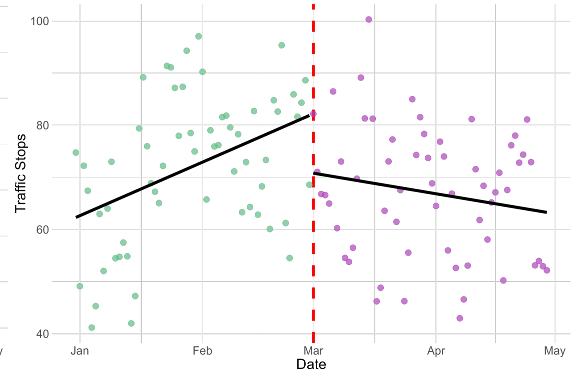
Notes: The residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022 are shown. For ease of interpretation, I added the residuals with the baseline mean of the outcome. The green color points represent my observation in the pre-treatment period. The purple color points represent my observation in the post-treatment period. The black line represents linear fit for both the pre- and post-treatment periods.

Figure 2: RDiT Estimate by Race

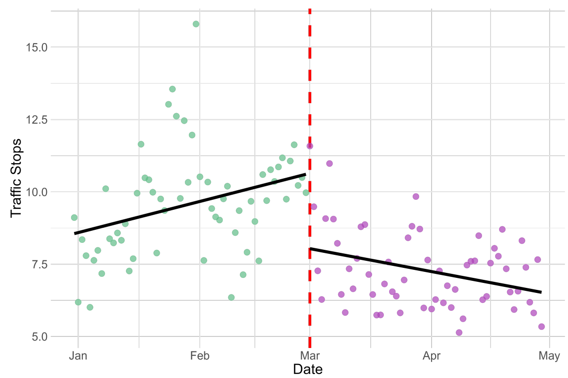
(a) Black: Minor Traffic



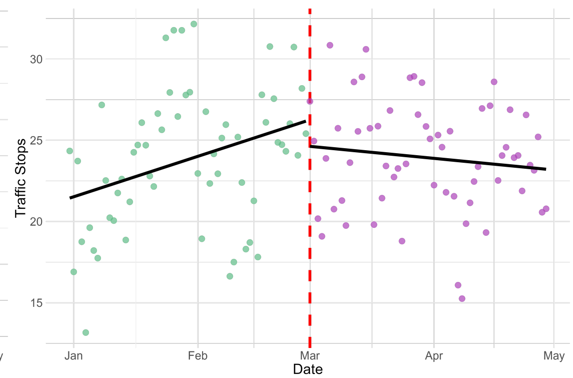
(b) Black: All



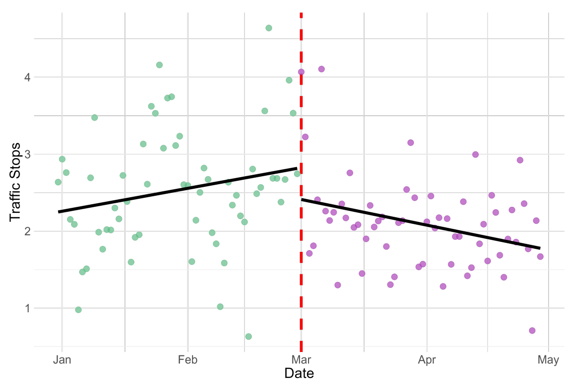
(c) Hispanic: Minor Traffic



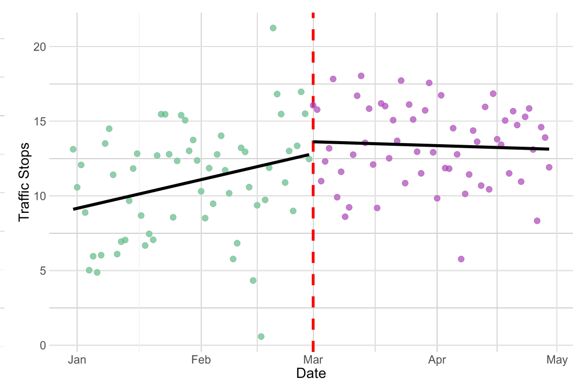
(d) Hispanic: All



(e) Non-Hispanic White: Minor Traffic

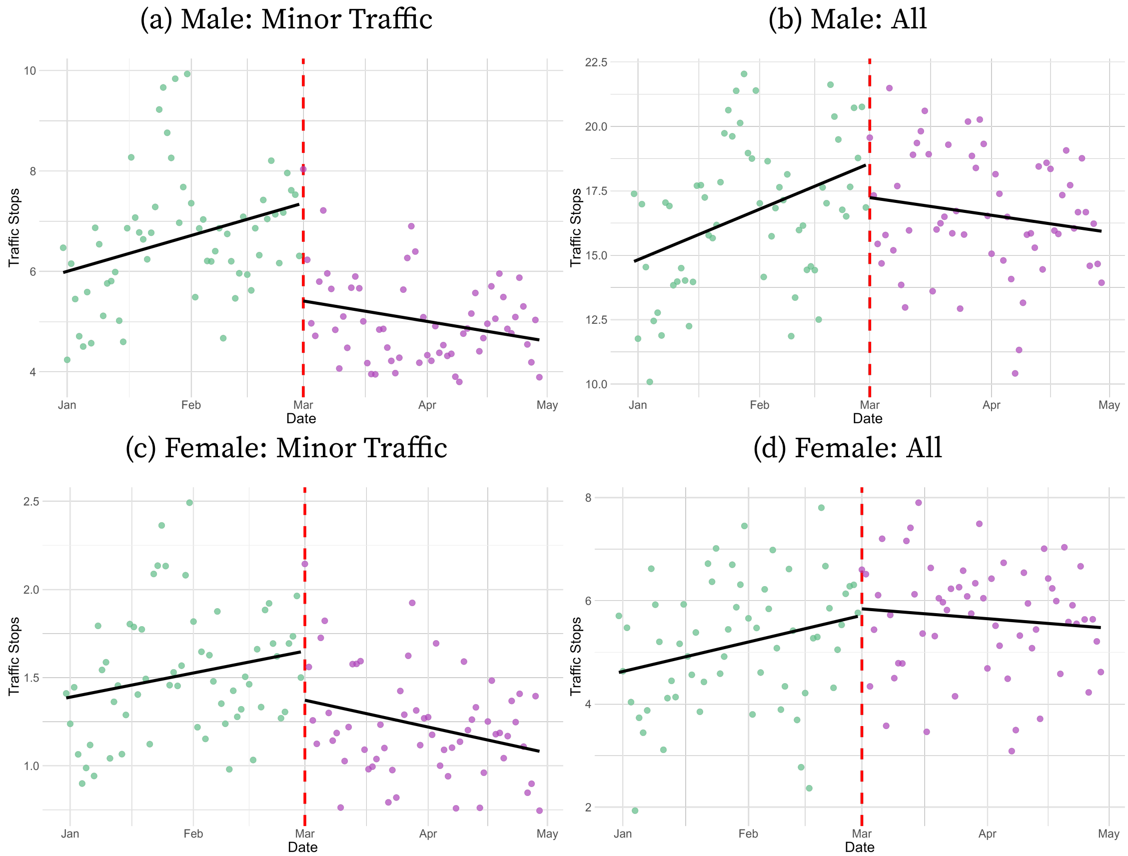


(f) Non-Hispanic White: Minor Traffic



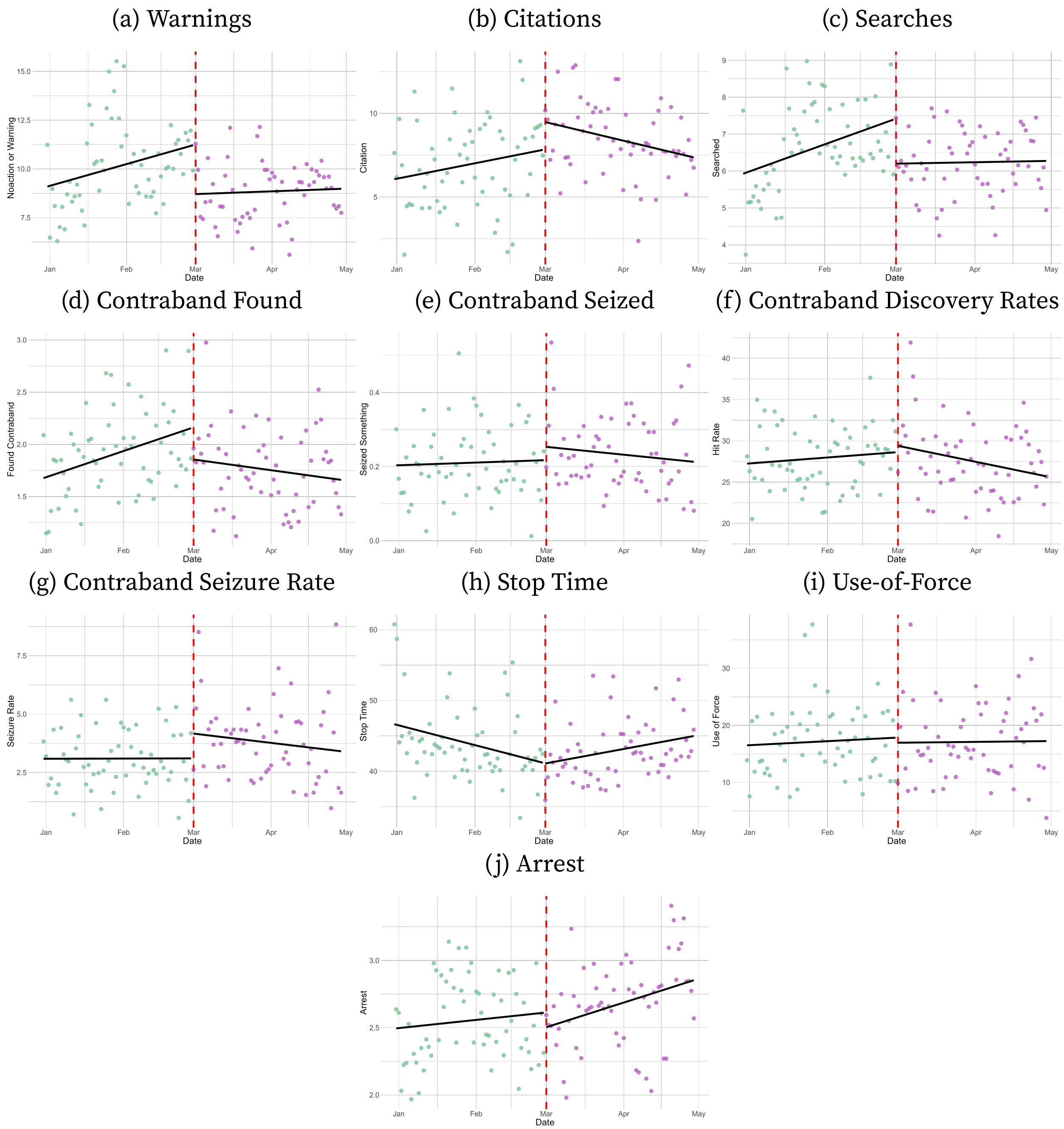
Notes: The residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022 are shown. For ease of interpretation, I added the residuals with the baseline mean of the outcome. The green color points represent my observation in the pre-treatment period. The purple color points represent my observation in the post-treatment period. The black line represents linear fit for both the pre- and post-treatment periods.

Figure 3: RDiT Estimate by Gender



Notes: The residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022 are shown. For ease of interpretation, I added the residuals with the baseline mean of the outcome. The green color points represent my observation in the pre-treatment period. The purple color points represent my observation in the post-treatment period. The black line represents linear fit for both the pre- and post-treatment periods.

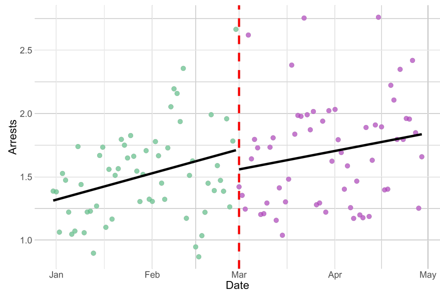
Figure 4: RDiT Estimate Stop Outcomes: All Stops



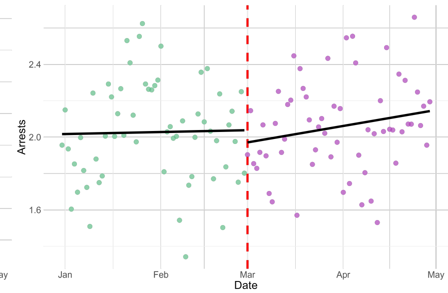
Notes: The residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022 are shown. For ease of interpretation, I added the residuals with the baseline mean of the outcome. The green color points represent my observation in the pre-treatment period. The purple color points represent my observation in the post-treatment period. The black line represents linear fit for both the pre- and post-treatment periods.

Figure 5: RDiT Estimate: Arrests, Crime, & Traffic Accidents

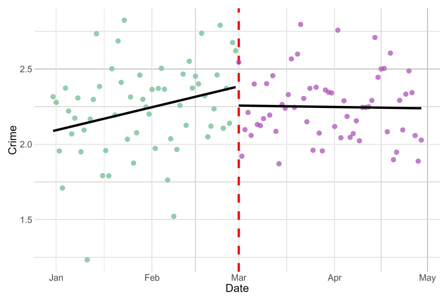
(a) Arrest: Misdemeanor



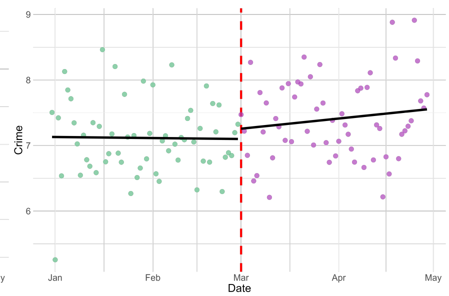
(b) Arrest: Felony



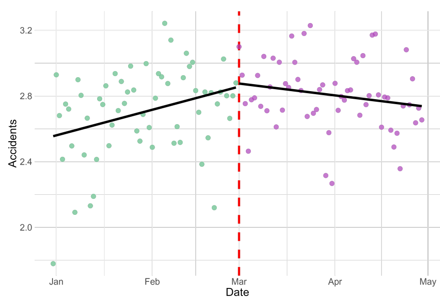
(c) Violent Crime



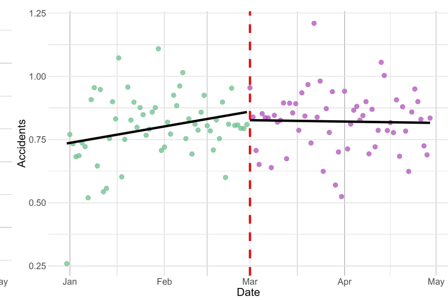
(d) Property Crime



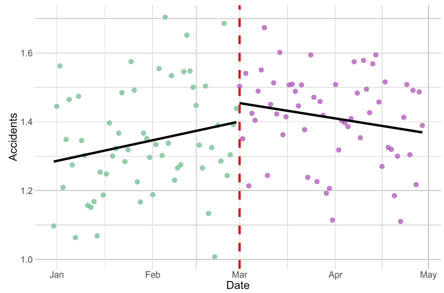
(e) Traffic Accidents



(f) Speeding Accidents



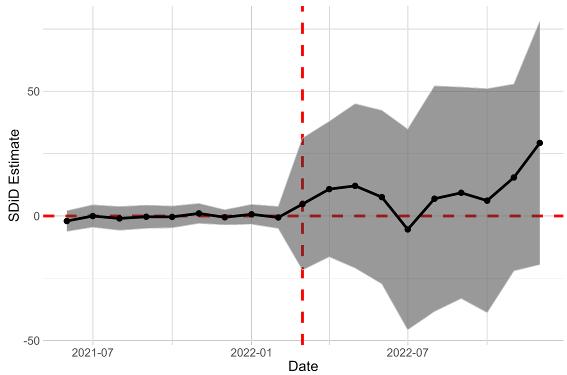
(g) Traffic Accidents from Other Vehicle Violations



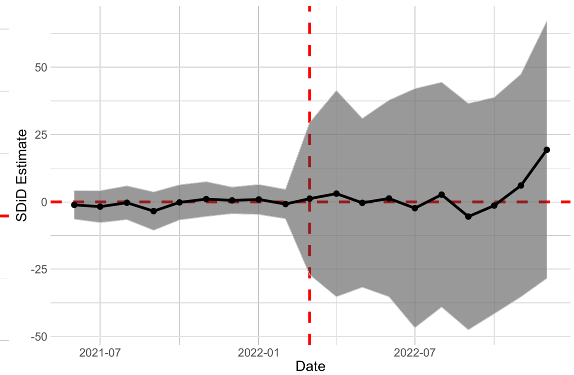
Notes: The residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022 are shown. For ease of interpretation, I added the residuals with the baseline mean of the outcome. The green color points represent my observation in the pre-treatment period. The purple color points represent my observation in the post-treatment period. The black line represents linear fit for both the pre- and post-treatment periods.

Figure 6: SDiD Estimate: Crime Comparing Los Angeles to the Rest of U.S.

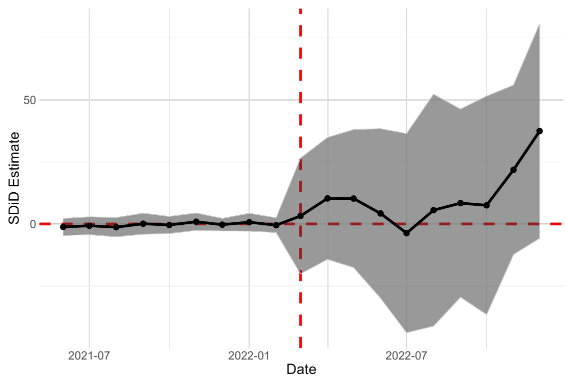
(a) All Part I: Agency-Level



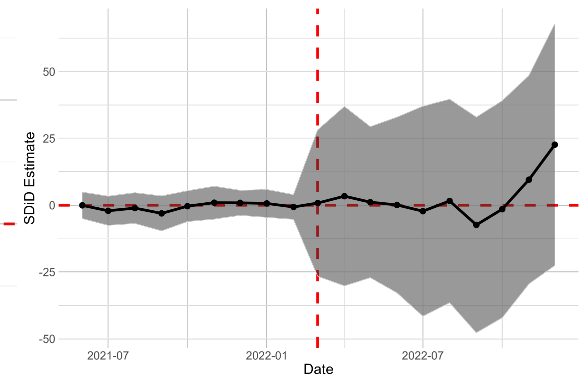
(b) All Part I: County-Level



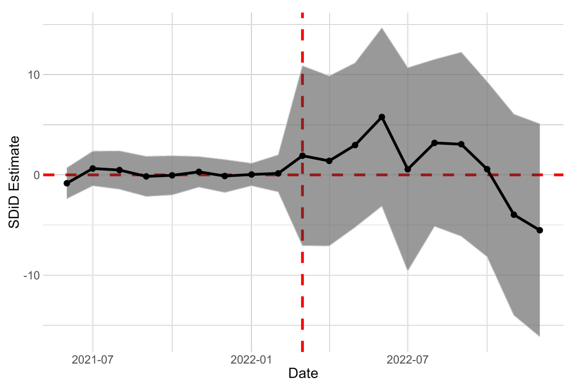
(c) Property: Agency-Level



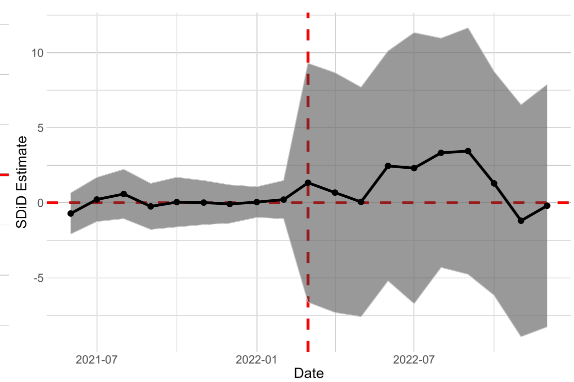
(d) Property: County-Level



(e) Violent: Agency-Level



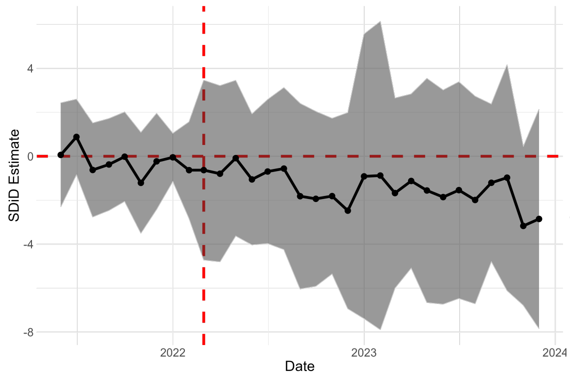
(f) Violent: County-Level



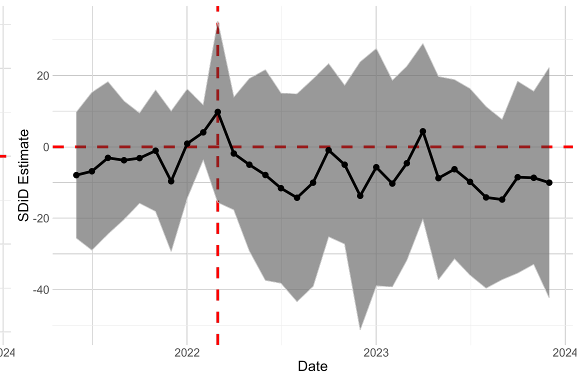
Notes: The synthetic difference-in-differences model is estimated. The left side of the panel uses agency-level data and compares LAPD (treated) to other large police agencies not in Los Angeles County. The right side of the panel uses county-level data and compares the county of Los Angeles (treated) to other large counties. The gray area represents 95 percent confidence intervals generated using placebo-based standard errors. The sample is restricted to June 2021 to December 2022.

Figure 7: SDiD Estimate: Accidents

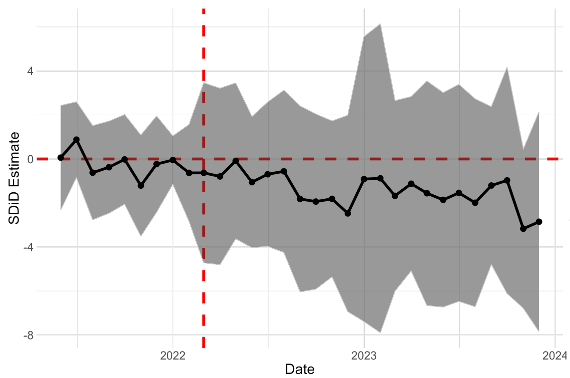
(a) All Accidents: Agency-Level



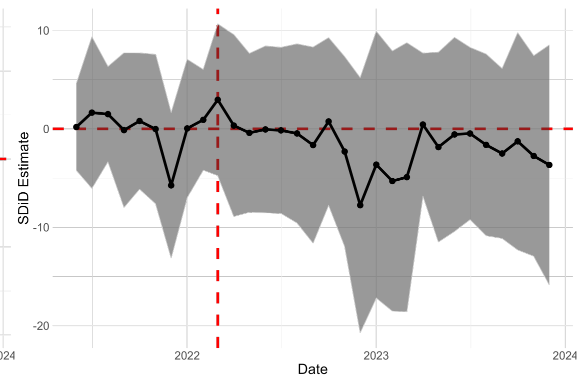
(b) All Accidents: County-Level



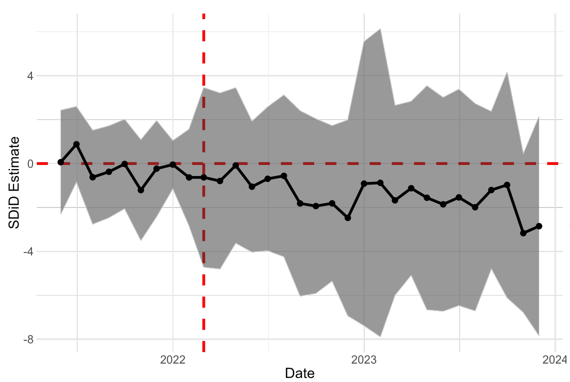
(c) Speeding Accidents: Agency-Level



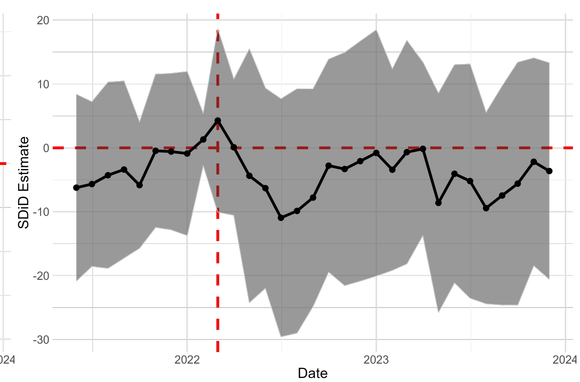
(d) Speeding Accidents: County-Level



(e) Accidents from Other Traffic Violation: Agency-Level



(f) Accidents from Other Traffic Violation: County-Level



Notes: The synthetic difference-in-differences model is estimated. The left side of the panel uses agency-level data and compares LAPD (treated) to other large police agencies not in Los Angeles County. The right side of the panel uses county-level data and compares the county of Los Angeles (treated) to other large counties. The gray area represents 95 percent confidence intervals generated using placebo-based standard errors. The sample is restricted to June 2021 to December 2023.

9 Table

Table 1: RDiT: Number of Stops

	Minor Traffic (1)	All Stops (2)	Minor Moving (3)
	Panel I: RDiT		
Post	-1.827***	-0.386	1.508*
	(0.500)	(1.286)	(0.911)
	Panel II: Differences in Discontinuties		
Post*LAPD	-2.077***	-1.429	0.910
	(0.510)	(1.323)	(0.935)
Mean of DV	6.390	20.799	8.440

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Table 2: RDiT: Number of People Stopped by Race

	Minor Stops			All Stops		
	Black (1)	Hispanic (2)	White (3)	Black (4)	Hispanic (5)	White (6)
	Panel I: RDiT					
Post	-10.569***	-2.606***	-0.414	-11.398**	-1.633	0.799
	(2.681)	(0.695)	(0.289)	(4.640)	(1.546)	(1.396)
	Panel II: Differences in Discontinuties					
Post*LAPD	-10.669***	-2.741***	-0.722**	-13.052***	-2.414	-0.474
	(2.722)	(0.708)	(0.319)	(4.736)	(1.599)	(1.451)
Mean of DV	28.041	8.492	2.464	75.763	26.030	13.246

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Table 3: RDiT: Number of People Stopped by Gender

	Minor Stops		All Stops	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
	Panel I: RDiT			
Post	-1.949***	-0.278**	-1.328	0.127
	(0.464)	(0.129)	(1.042)	(0.461)
	Panel II: Differences in Discontinuties			
Post*LAPD	-2.069***	-0.377***	-1.919*	-0.277
	(0.471)	(0.135)	(1.064)	(0.479)
Mean of DV	5.934	1.394	18.155	5.777

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Table 4: RDiT: Traffic Stop Outcomes

	Warning	Citation	Searched	Found Some-thing	Seized Some-thing	Discovery Rate	Seizure Rate	Avg. Stop Time	Use-of-Force	Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel I: RDiT										
Post	-2.543***	1.634*	-1.219***	-0.308**	0.036	0.762	1.060**	-0.019	-0.903	-0.110
	(0.717)	(0.904)	(0.350)	(0.147)	(0.035)	(1.878)	(0.431)	(1.781)	(1.801)	(0.105)
Panel II: Differences in Discontinuties										
Post*LAPD	-2.889***	1.038	-1.297***	-0.319**	0.036	0.601	2.711	0.417	-1.420	-0.129
	(0.734)	(0.930)	(0.356)	(0.148)	(0.038)	(2.317)	(2.340)	(1.850)	(2.047)	(0.111)
Mean of DV	10.684	8.049	7.100	1.907	0.226	26.756	3.190	40.922	18.467	2.648

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Table 5: RDiT: Arrest, Crime, & Accidents

	Arrests		Crime		Accidents		
	Misdemeanor	Felony	Property	Violent	All	Speeding	Other Violation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	-0.159	-0.067	0.157	-0.128	0.019	-0.034	0.053
	(0.192)	(0.094)	(0.166)	(0.086)	(0.089)	(0.042)	(0.052)
Mean of DV	1.679	2.078	7.115	2.234	2.704	0.797	1.342

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Table 6: SDiD: Reported Crime

	LAPD vs. Other Agencies			LA vs. Other Counties		
	All	Property	Violent	All	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel I: California Only					
Post*LA	9.694	10.510	0.994	2.416	2.811	1.349
	(17.642)	(16.758)	(3.062)	(17.038)	(15.804)	(2.734)
Rank Based P-Value	{0.381}	{0.333}	{0.690}	{0.833}	{0.829}	{0.640}
Mean of DV	276.529	207.754	68.776	244.913	195.107	49.805
	Panel II: All					
Post*LA	9.370	8.925	0.271	10.129	11.441	0.981
	(17.671)	(15.974)	(4.547)	(12.765)	(12.195)	(5.807)
Rank Based P-Value	{0.477}	{0.464}	{0.877}	{0.455}	{0.438}	{0.676}
Mean of DV	276.529	207.754	68.776	244.913	195.107	49.805

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: The synthetic difference-in-differences model is estimated using monthly data. Columns 1 to 3 use an agency-by-month panel and compare the LAPD (treated) to other large police agencies not in Los Angeles County. Columns 4 to 6 use a county-by-month panel and compare the county of Los Angeles (treated) to other large counties. The standard errors estimated using placebo-based tests are reported inside the parenthesis. Rank-based p-value where the p-value is computed using a relative ranking of the average treatment effect is shown inside a curly bracket. The sample is restricted to June 2021 to December 2022, and the counterfactual includes the rest of California (panel I) or the rest of the U.S. (panel II).

Table 7: SDiD: Reported Traffic Accidents

	LAPD vs. Other Agencies			LA vs. Other Counties		
	All	Speeding	Other Violation	All	Speeding	Other Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LA	-3.304	-1.436	-1.138	-7.162	-1.676	-4.289
	(3.332)	(1.549)	(2.237)	(9.354)	(3.207)	(6.518)
Rank Based P-Value	{0.281}	{0.276}	{0.280}	{0.314}	{0.490}	{0.308}
Mean of DV	28.483	7.215	12.943	89.045	27.586	43.774

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: The synthetic difference-in-differences model is estimated using monthly data. Columns 1 to 3 use an agency-by-month panel and compare the LAPD (treated) to other large police agencies not in Los Angeles County. Columns 4 to 6 use a county-by-month panel and compare the county of Los Angeles (treated) to other large counties. The standard errors estimated using placebo-based tests are reported inside the parenthesis. Rank-based p-value where the p-value is computed using a relative ranking of the average treatment effect is shown inside a curly bracket. The sample is restricted to June 2021 to December 2023, and the counterfactual includes the rest of California.

ONLINE APPENDIX

Appendix A. Proof

A.1 Proof of Prediction 1

Given that drivers from two different racial groups (Black and White) are violating each traffic violation, the probability of stopping someone for a traffic violation t is the probability of observing Black drivers violating t and stopping that driver or observing White drivers violating t and stopping that driver. Mathematically, I can express the probability of stopping someone for a traffic violation as follows:

$$\begin{aligned}
P(\text{stop}|t=m) =& \underbrace{\phi_{mb}P(\text{stop}|t=m,r=b)}_{\substack{\text{Observe \& Stop}\\ \text{Violation m, Race b}}} + \underbrace{\phi_{mw}P(\text{stop}|t=m,r=w)}_{\substack{\text{Observe \& Stop}\\ \text{Violation m, Race w}}} \\
=&\phi_{mb}[1-F(c_m^*|t=m,r=b)] + \phi_{mw}[1-F(c_m^*|t=m,r=w)] \\
=&\phi_{mb}+\phi_{mw}-\phi_{mb}F(c_m^*|t=m,r=b)-\phi_{mw}F(c_m^*|t=m,r=w) \\
P(\text{stop}|t=o) =& \underbrace{\phi_{ob}P(\text{stop}|t=o,r=b)}_{\substack{\text{Observe \& Stop}\\ \text{Violation o, Race b}}} + \underbrace{\phi_{ow}P(\text{stop}|t=o,r=w)}_{\substack{\text{Observe \& Stop}\\ \text{Violation o, Race w}}} \\
=&\phi_{ob}[1-F(c_o^*|t=o,r=b)] + \phi_{ow}[1-F(c_o^*|t=o,r=w)] \\
=&\phi_{ob}+\phi_{ow}-\phi_{mb}F(c_o^*|t=o,r=b)-\phi_{ow}F(c_o^*|t=o,r=w)
\end{aligned} \tag{A.1}$$

Taking the derivative of Equation (A.1) with respect to c_m, I can mathematically show prediction 1.

$$\begin{aligned}
\frac{dP(\text{stop}|t=m)}{dc_m} =& -\phi_{mb}\frac{dF(c_m^*|t=m,r=b)}{dc_m} - \phi_{mw}\frac{dF(c_m^*|t=m,r=w)}{dc_m} \\
=& -\phi_{mb}\frac{dF(c_m^*|t=m,r=b)}{dc_m^*}\cdot\frac{dc_m^*}{dc_m} - \phi_{mw}\frac{dF(c_m^*|t=m,r=w)}{dc_m^*}\cdot\frac{dc_m^*}{dc_m} \\
=& -\phi_{mb}f(c_m^*|t=m,r=b)\cdot\frac{d(c_m+E(U_2))}{dc_m} - \phi_{mw}f(c_m^*|t=m,r=w)\cdot\frac{d(c_m+E(U_2))}{dc_m} \\
=& -\phi_{mb}f(c_m^*|t=m,r=b)(1+\frac{dE(U_2)}{dc_m}) - \phi_{mw}f(c_m^*|t=m,r=b)(1+\frac{dE(U_2)}{dc_m}) \\
=& \sum_r -\underbrace{\phi_{mr}}_{>0}\underbrace{f(c_t^*|t=m,r)}_{>0}\underbrace{(1+\underbrace{\frac{dE(U_2)}{dc_m}}_{\in(-1,0)})}_{\in(0,1)} < 0 \qquad \blacksquare
\end{aligned}$$

$$\begin{aligned}
\frac{dP(\text{stop}|t=o)}{dc_m} =& -\phi_{ob}\frac{dF(c_o^*|t=o,r=b)}{dc_m} - \phi_{ow}\frac{dF(c_o^*|t=o,r=w)}{dc_m} \\
=& -\phi_{ob}\frac{dF(c_o^*|t=o,r=b)}{dc_m^*}\cdot\frac{dc_o^*}{dc_m} - \phi_{ow}\frac{dF(c_o^*|t=o,r=w)}{dc_m^*}\cdot\frac{dc_o^*}{dc_m} \\
=& -\phi_{ob}f(c_o^*|t=o,r=b)\cdot\frac{d(c_o+E(U_2))}{dc_m} - \phi_{mw}f(c_o^*|t=o,r=w)\cdot\frac{d(c_o+E(U_2))}{dc_m}
\end{aligned}$$

$$
\begin{aligned}
&= -\phi_{ob} f(c_o^*|t=o, r=b)(\frac{dE(U_2)}{dc_m}) - \phi_{mw} f(c_m^*|t=o, r=b)(\frac{dE(U_2)}{dc_m}) \\
&= \sum_r - \underbrace{\phi_{mr}}_{>0} \underbrace{f(c_t^*|t=m, r)}_{>0} \underbrace{(\frac{dE(U_2)}{dc_m})}_{\in(-1,0)} > 0 \qquad \blacksquare
\end{aligned}
$$

A.2 Proof of Prediction 2

To make this prediction, I first find the probability of stopping a Black driver, defined as the probability of observing a Black driver committing a minor traffic violation and stopping them or the probability of observing a Black driver committing other traffic violation and stopping them.

$$
\begin{aligned}
P(\text{stop}|r=b) =& \underbrace{\phi_{mb} P(\text{stop}|t=m, r=b)}_{\substack{\text{Observe \& Stop} \\ \text{Violation m, Race b}}} + \underbrace{\phi_{ob} P(\text{stop}|t=o, r=b)}_{\substack{\text{Observe \& Stop} \\ \text{Violation o, Race b}}} \\
=& \phi_{mb}[1 - F(c_m^*|t=m, r=b)] + \phi_{ob}[1 - F(c_o^*|t=o, r=b)] \\
=& \phi_{mb} + \phi_{ob} - \phi_{mb} F(c_m^*|t=m, r=b) - \phi_{ob} F(c_o^*|t=o, r=b)
\end{aligned}
$$

Taking the derivative of this probability function with respect to c_m, I can derive the following:

$$
\begin{aligned}
\frac{dP(\text{stop}|r=b)}{dc_m} &= -\phi_{mb} \frac{dF(c_m^*|t=m, r=b)}{dc_m} - \phi_{ob} \frac{dF(c_o^*|t=o, r=b)}{dc_m} \\
&= -\phi_{mb} \frac{dF(c_m^*|t=m, r=b)}{dc_m^*} \cdot \frac{dc_m^*}{dc_m} - \phi_{ob} \frac{dF(c_o^*|t=o, r=b)}{dc_o^*} \cdot \frac{dc_o^*}{dc_m} \\
&= -\phi_{mb} f(c_m^*|t=m, r=b) \cdot \frac{d(c_m + E(U_2))}{dc_m} - \phi_{ob} f(c_o^*|t=o, r=b) \cdot \frac{d(c_o + E(U_2))}{dc_m} \\
&= -\phi_{mb} f(c_m^*|t=m, r=b)(1 + \frac{dE(U_2)}{dc_m}) - \phi_{ob} f(c_o^*|t=o, r=b) \frac{dE(U_2)}{dc_m}
\end{aligned} \tag{A.2}
$$

Unlike in Prediction 1, where the sign of the derivative is clearly one direction, the sign of the derivative in Prediction 2 is more complex and cannot be determined directly. This is because in Prediction 2, the stopping probability for Black drivers depends on many parameter values. To determine the conditions under which this derivative is positive or negative, I derive the optimal condition as follows:

$$
\begin{aligned}
&\frac{dP(\text{stop}|r=b)}{dc_m} > 0 \\
\Rightarrow & -\phi_{mb} f(c_m^*|t=m, r=b)(1 + \frac{dE(U_2)}{dc_m}) - \phi_{ob} f(c_o^*|t=o, r=b)(\frac{dE(U_2)}{dc_m}) > 0
\end{aligned}
$$

$$\Rightarrow -\phi_{mb}f(c_m^*|t=m,r=b) - \phi_{mb}f(c_m^*|t=m,r=b)\frac{dE(U_2)}{dc_m} > \phi_{ob}f(c_o^*|t=o,r=b)\frac{dE(U_2)}{dc_m}$$
$$\Rightarrow -\phi_{mb}f(c_m^*|t=m,r=b) > \phi_{ob}f(c_o^*|t=o,r=b)\frac{dE(U_2)}{dc_m} + \phi_{mb}f(c_m^*|t=m,r=b)\frac{dE(U_2)}{dc_m}$$
$$\Rightarrow -\phi_{mb}f(c_m^*|t=m,r=b) > [\phi_{ob}f(c_o^*|t=o,r=b) + \phi_{mb}f(c_m^*|t=m,r=b)]\frac{dE(U_2)}{dc_m}$$
$$\Rightarrow -\underbrace{\frac{\phi_{mb}f(c_m^*|t=m,r=b)}{\phi_{ob}f(c_o^*|t=o,r=b) + \phi_{mb}f(c_m^*|t=m,r=b)}}_{>0} > \underbrace{\frac{dE(U_2)}{dc_m}}_{<0}$$
$$\Rightarrow \frac{\phi_{mb}f(c_m^*|t=m,r=b)}{\phi_{ob}f(c_o^*|t=o,r=b) + \phi_{mb}f(c_m^*|t=m,r=b)} > |\frac{dE(U_2)}{dc_m}| \qquad \blacksquare$$

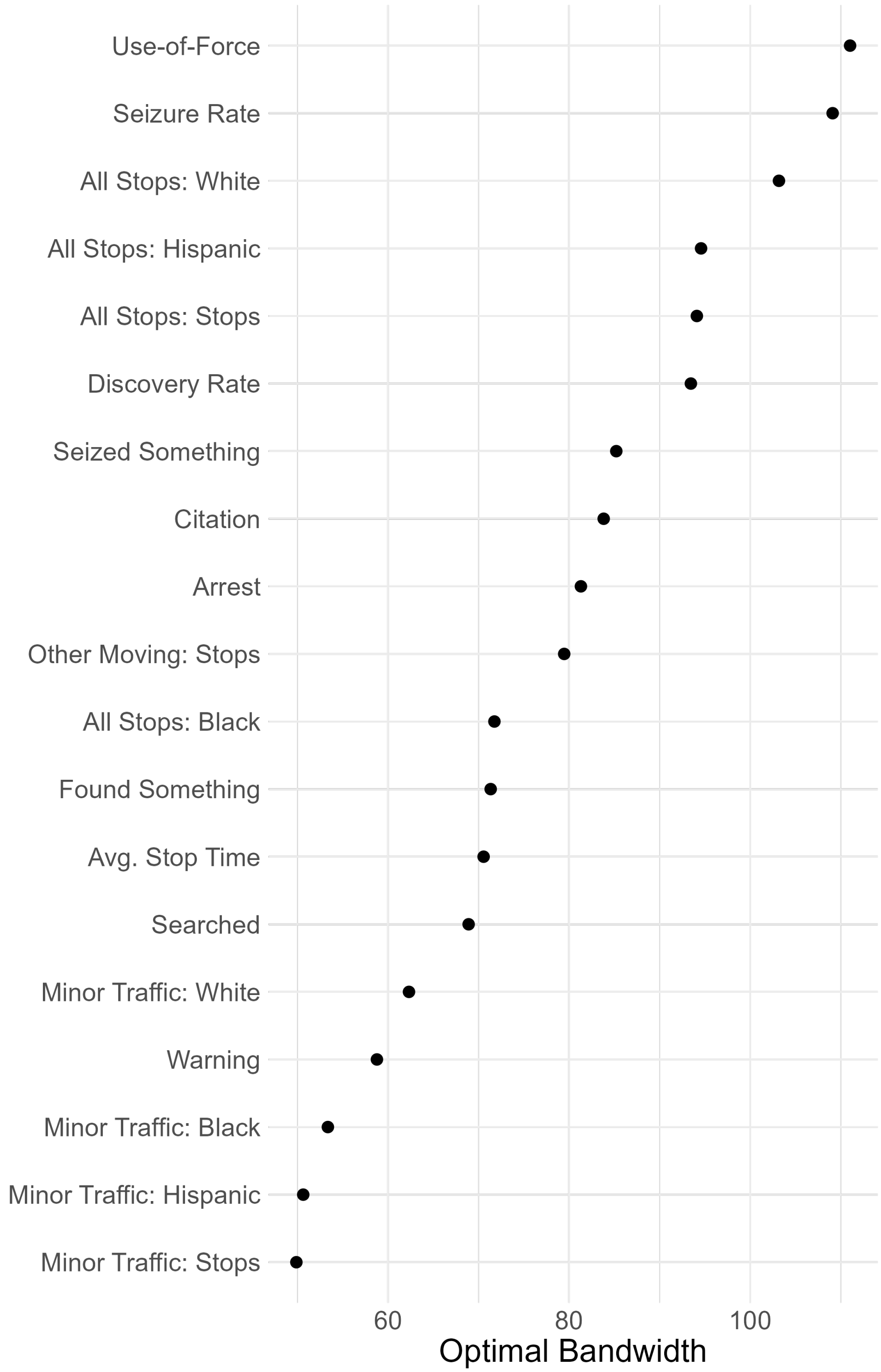
This result implies that the sign of the change in the total number of drivers getting stopped depends on ϕ_{mb}, ϕ_{ob}, $f(c_o^*|t=m,r=b)$ and $f(c_o^*|t=o,r=b)$. To find how these parameters, I will find the cross derivatives to determine the sign of each:

$$\frac{dP(\text{stop}|r=b)}{dc_m d\phi_{mb}} = -f(c_m^*|t=m,r=b)(1+\frac{dE(U_2)}{dc_m}) < 0$$
$$\frac{dP(\text{stop}|r=b)}{dc_m d\phi_{ob}} = -f(c_o^*|t=o,r=b)\frac{dE(U_2)}{dc_m} > 0$$
$$\frac{dP(\text{stop}|r=b)}{dc_m df(c_m^*|t=m,r=b)} = -\phi_{mb}(1+\frac{dE(U_2)}{dc_m}) < 0$$
$$\frac{dP(\text{stop}|r=b)}{dc_m df(c_o^*|t=o,r=b)} = -\phi_{ob}\frac{dE(U_2)}{dc_m} > 0$$

The cross derivatives above imply that the change in the probability that the officer stops a Black driver will more likely be negative (in sign) as ϕ_{mb} and $f(c_m^*|t=m,r=b)$ increases. On the other hand, the cross derivatives also suggest that this change in the probability will more likely be positive as ϕ_{ob} and $f(c_o^*|t=m,r=b)$ increases.

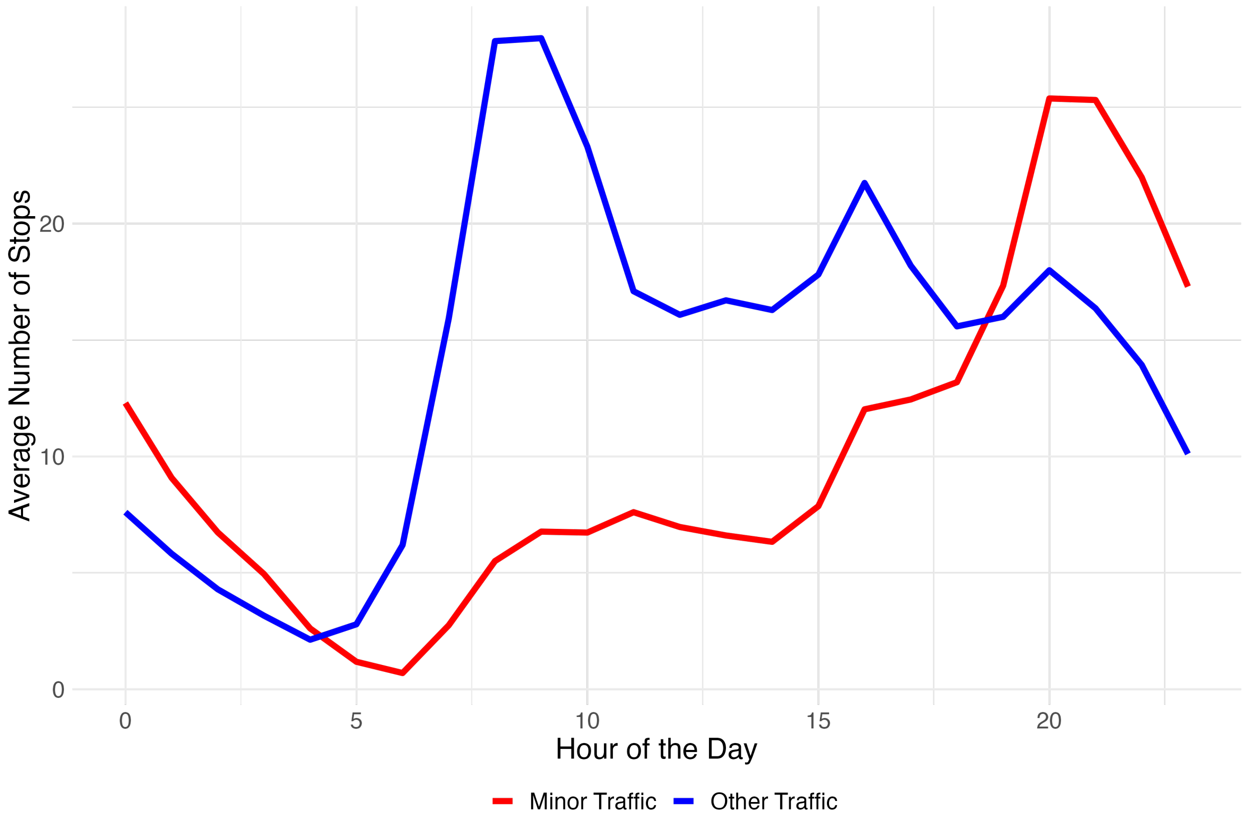
Appendix B. Descriptive Figures & Tables

Appendix Figure B.1: Optimal RDiT Bandwidth by Outcome



Notes: This figure plots the MSE-optimal bandwidth for each RIPA outcome I examined. The optimal bandwidth is determined using procedures laid out by Calonico et al. (2015) and R package `rdrobust`.

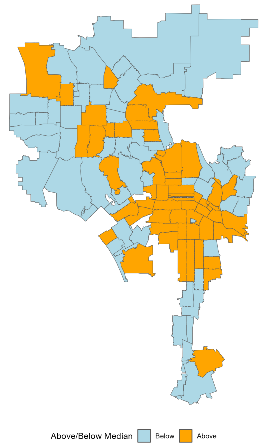
Appendix Figure B.2: Traffic Stops by Hour



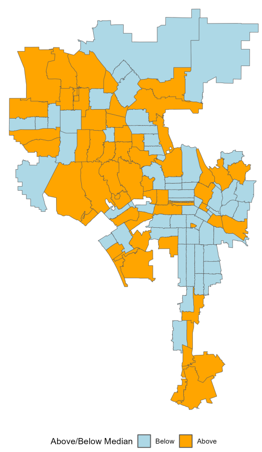
Notes: The average number of stops by each hour of the day is shown. Minor Traffic is defined as any equipment or non-moving violations. Other traffic is defined as any other traffic stops.

Appendix Figure B.3: Spatial Distribution of Accidents & Stops by Zip Code

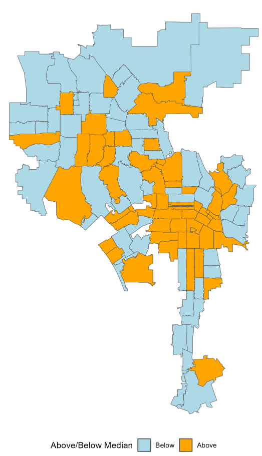
(a) Accidents per Capita



(b) Percent of Stops that are Minor Infraction

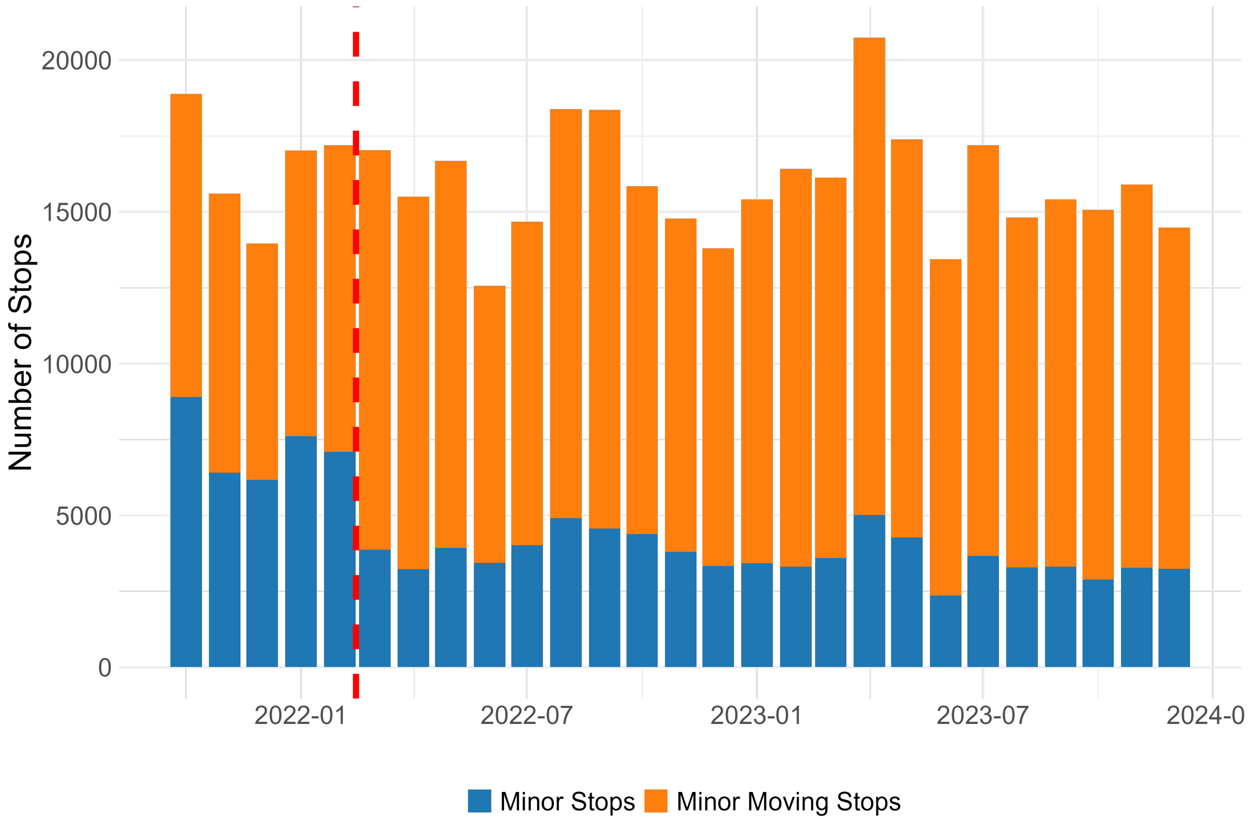


(c) Stops Involving Racial Minorities



Notes: These figures show whether each zip code is above or below the median accident per capita (panel a), stops for minor infractions per capita (panel b), and any stops involving racial minorities per capita (panel c). I use the pre-treatment window (July 2018 to Feb 2022) to measure the total count.

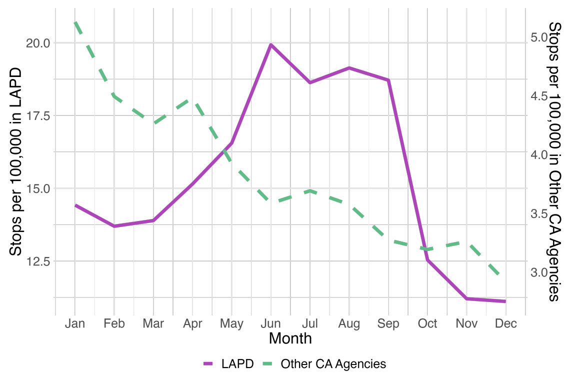
Appendix Figure B.4: Time Series Plot: Number of Stops



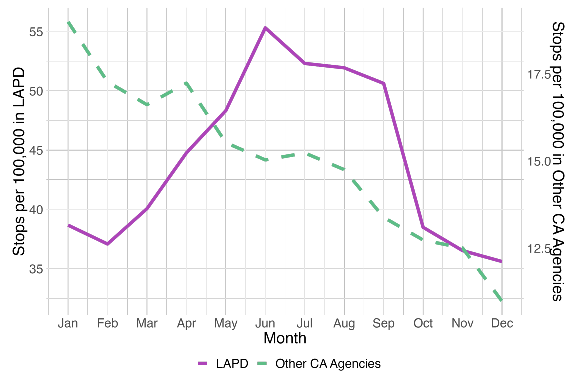
Notes: This figure shows the total monthly counts of stops made by the LAPD.

Appendix Figure B.5: Monthly Traffic Stop

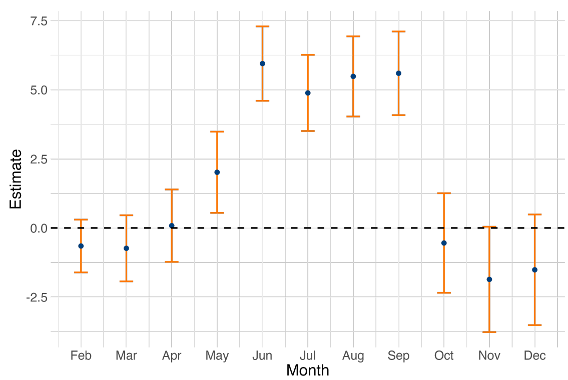
(a) Raw Trend: Minor Traffic Stops



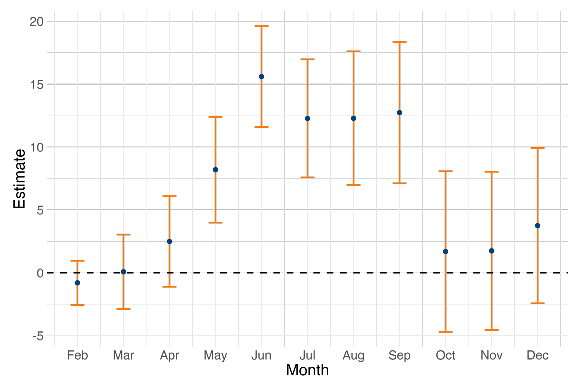
(b) Raw Trend: All Stops



(c) Monthly Coefficients Interacted w/ LAPD: Minor Traffic Stops



(d) Monthly Coefficients Interacted w/ LAPD: All Stops



Notes: Panels (a) and (b) present the aggregate count of traffic stops by month. Panels (c) and (d) present the point estimate and 95 percent confidence intervals generated using standard errors clustered at the agency level for the interaction between monthly coefficients (relative to January) and the dummy variable for LAPD. In panels (c) and (d), estimates are estimated at the daily level and control for daily temperature, precipitation, vehicle miles traveled, and agency and day-of-the-week fixed effects.

Appendix Table B.1: Offense Classified as Minor Traffic (Non-Moving & Equipment) Violations

Code	Offense	Code	Offense
25063	SHO ON VEH/GIV PO FLS DOC	54443	FRONT RUNNING LAMPS VIOL
25107	PASS FALSE CLEAN AIR STKR	54444	IMPROPER: UNDOC VESSEL
42123	DRNK ALC/MARIJ AS PASSNGR	54451	VEHICLE HORN VIOLATION
42125	ALC/MARIJ AS PASSENGR HWY	54457	UNLAW DIRCT DRV OPR/HWY
44054	IMPERSONATE CHP OFFICER	54458	OPR VEH/ETC W/O LIC TYP
48054	FALSE REP THEFT TO DECEIV	54468	VEH BRK SYS VIOL:MTR FAIL
54013	UNLAWFUL OPERATION OF VEH	54469	HIRE VEH/ETC:ID DSPL VIOL
54014	HEADLAMP:OPR/AMT/SIZE:VIO	54471	TOW TRK/ETC ID INFO VIOL
54015	WINDOW INSTAL/ETC MAT VIO	54473	UNREG CA BASED VEHICLE
54016	FAIL NOTE DMV:OWN TRANSFR	54474	IDENT PLATE DISPLAY VIOL
54018	NO PROOF:FINANCE RESP:VEH	54478	TOW TRUCK TAILLAMP VIOL
54090	POSS OPEN CONTAINER:DRIVE	54479	TOW TRK TAIL/STOP LMP VIO
54099	NO REG:VEH/TRAILER/ETC	54480	HEADLAMP VIOLATION
54101	FAIL PROVE FIN RSP:PO REQ	54483	FAIL NOTE DMV:VEH SEL/ETC
54102	NO EVID:ID/INS/ETC:ACCDNT	54493	INOP SIGNL LAMP:ARM SIGNL
54109	FAIL MAINT VEH LITE EQUIP	54499	REAR VEH REFLECTOR VIOL
54110	FAIL MAINT LIC PLATE LAMP	54500	REF:VEH MFG/REG A/1-1-65
54116	INADEQUATE MUFFLERS	54504	FAIL OBEY TRAF LANE SIGN
54138	DEFECTIVE WINDSHIELD/ETC	54510	EXHAUST PIPE VIOLATION
54140	DRIVE W/O VALID LICENSE	54513	UNAUTH VEH IN FIRE AREA
54141	BIKE HEADLIGHT/ETC VIOL	54515	LAMP VOLT:85 PER REQ VOLT
54142	NO WINDSHIELDS	54516	TIRE N/CONFORMANCE W/REG
54143	BRAKE SYS CONDITION VIOL	54518	STOPLAMPS VIOL:N/VISIBLE
54144	STOPLAMPS VIOL:SPEC VEH	54528	OBSTRUCT OF LIC PLATE
54148	FAIL PROVIDE VEH REG:PO	54531	MISUSE INSTRUCTION PERMIT
54150	OPR VEH:VIOL LIC RESTRCTN	54533	SINGLE BEAM:PROPER ADJUST
54165	ILL MOD EXHAUST SYS:NOISE	54534	NOT EQUIPED W/SMOG DEVICE
54168	EXPIRED TABS/FAIL DISPLAY	54536	REAR PROJECTION VIOLATION
54171	VEH LAMPS/ETC COLOR VIOL	54540	IDENTIFICATION PLATE VIOL
54172	FOREIGN COM VEH:NO PERMIT	54543	FAIL COMPLY:MOUNTING REQ
54190	STORE OPEN/ETC ALC IN VEH	54545	CHILD 6- ALONE IN VEHICLE
54193	TAILLAMP VIOLATIONS	54548	TRESP W/VEH ETC:PUB GRNDS
54194	STOPLAMP VIOLATIONS	54549	METAL TIRE:EXCESS 6MPH
54195	NO LAMP/FLAG/ETC EXT LOAD	54552	OBST DRIVER VIEW/CONTROL
54204	WRONG COLOR:WINDO/ETC MAT	54553	SELL/USE UNAP LIGHT EQUIP
54205	TIRE TREAD DEPTH VIOL	54571	OPR VEH:WINDOW OBSTRUCTED
54206	FAIL REG FOREIGN VEH:CA	54572	TRNSP 10/MORE USED TIRES
54208	NO REGISTRATION IN VEH	54574	STOPLAMPS:VEH MUST HAVE
54211	LICENSE PLATE DISPLAY VIO	54584	PERSON FAIL TO PAY TOLL
54214	SPEC VEH FENDER/ETC VIOL	54586	OP MOTORSCTR:DRK HWY:LAMP

Appendix Table B.1: Offense Classified as Minor Traffic (Non-Moving & Equipment) Violations *(continued)*

Code	Offense	Code	Offense
54216	LICENSEE POSS/ETC 1+ CDL	54587	OP MOTRSCTR:DRK HWY:R/REF
54221	PARK IN SPACE FOR DISABLE	54591	UNAUTH FLASHING BLU LIGHT
54222	AUXILIARY DRIVE LAMP VIOL	54594	DIFFUSED LIGHT:NO RED
54223	VEHICLE BUMPER VIOLATION	54604	LAMP REQUIREMENT VIOLATIO
54226	VEH SIDEVIEW MIRROR VIOL	54605	MOTORCYC H/LAMP:1REQ/2PRM
54227	LOST/ETC DMV REG/ETC VIOL	54608	FRM LABR VEH:WOUT SEATBLT
54229	FUEL TANK CAP VIOLATION	54612	ILLEGAL FLASHING LIGHTS
54230	MOTORCYCLE HEADLAMP VIOL	54614	OPR VEH:WINDOW OBSTRUCTED
54233	SAFETY GLAZING MATRL VIOL	54617	UNLAWFUL DISPLAY ID PLATE
54234	LICENS PLATE POSITION VIO	54618	INADEQUATE BRAKE SYSTEM
54300	GASTIGHT EXHAUST SYS VIOL	54619	FAIL TO DISPLAY WGT DECAL
54301	HITCH/ETC MOUNT VIOLATION	54626	OPR BIKE W/O BRAKES
54305	FAIL NOTE DMV ADD CHG:CDL	54628	FAIL UNLOCK LIMO:EMERGNCY
54307	BACKUP LAMPS VIOLATION	54644	DISPLAY LIC PLATES WRONG
54308	REARVIEW MIRROR VIOLATION	54645	DISPLY ONE LIC PLATE WRNG
54311	ABANDON VEHICLE ON HIWAY	54649	STOPLAMPS:VEH 2 REQUIRED
54313	NONRES:DRIVE W/O MED CERT	54657	NO REG:VEH/TRAILER/ETC
54314	SERVICE BRAKES VIOLATION	54663	NO PARK/STOP ETC FIRE LN
54316	VIOL VISBLTY REQ:TURN SIG	54666	UNREG COMM MTR VEH 10000+
54321	SELL/ETC UNLAWF EQUIPMENT	54667	COMMERC VEH WGHT FEES DUE
54324	EXCESSIVE EXHAUST VIOL	54670	EMPL ALLW DRIVER COMM VEH
54330	PARKING/ETC VIO:SPEC CIRC	54672	AUX LMPS NOT COVERD W/DRV
54331	MODIFIED VEH RIM HGT VIOL	54675	5TH WHL CONNECT DEVC REQ
54334	FAIL TO REP WT ALTER/ETC	54676	5TH WHL LOCKING DEVC REQ
54338	DISPLAY ALTERED LIC PLATE	54683	UNINTERRUPTED TOWS
54340	WINDSHIELD WIPER VIOL	54685	WARNING LAMPS ON TOW TRK
54345	DRIVE W/O COMERCL VEH LIC	54687	FLOOD LAMP EXCEEDS 75 FT
54346	PARK/ETC BY FIRE HYDRANT	54688	TEMP LIC PLATE NOT RPLCED
54347	DRIVE W/PARK LIGHTS ONLY	54691	BREAKAWAY DEVICE REQ VEH
54349	OPR CARRIER:NO ID NUMBER	54693	OPERATE AFTER NTC BY OFCR
54351	VENDING ON/NEAR FREEWAY	54695	VEH PNEU TIRES EXCD WIDTH
54354	NO PROOF $ RESP:ACCIDENT	54697	VEH/LOAD EXCEDS HGT 14 FT
54357	EQUIP MODIFY DEVICE VIOL	54698	BOOM/MAST NOT SECURE
54358	FT/COMPLY:INSPEC RULE/REG	54699	VEHICLE EXCEEDS LEN 40 FT
54359	REG/ETC:SMOG CERTS VIOL	54702	GROSS VEH WGHT VIOL COMBO
54376	VEH WITH UNLAWFUL LAMPS	54706	WRN SIG NOT RMVED W/O LD
54377	FOG TAILLAMPS VIOL	54707	IMPRP USE WRN LAMPS PILOT
54379	PARALLEL PARKING VIOL	54708	FLR/DISP COMP NM PILOT CR
54393	OPR UNDOC VES W/O NUMBERS	54709	FLR/DISP ID SIG PILOT CAR
54396	OPR VEH W/O LIC:PARK LOT	54712	NME/TRDMK ON FOR-HIRE VEH

Appendix Table B.1: Offense Classified as Minor Traffic (Non-Moving & Equipment) Violations *(continued)*

Code	Offense	Code	Offense
54399	LIGHT DIMMER SWITCH VIOL	54713	VIS SIGN VEH LIQ PET/GAS
54407	LOWBEAM GLARE VIOLATION	54714	LGHT REFL TRK/TRLR FR/SID
54408	FRONT FENDER/ETC LAMP VIO	54715	LT REFL 30+TRK/TRLR FR/SD
54410	OPR/ETC GROSS POLLUTER	54716	FLR DISP REFL MAT ON TRLR
54412	O-O-S VEH:NO REG/SMOG CRT	54717	CLEAR/SIDE MARKR LAMP REQ
54413	OPR UNAUTH POLICE VEHICLE	54718	BRAK REQ FOR TRLERS 6000+
54414	LITE ON HWY:IMPAIR VISION	54720	INADEQUATE PARKING BRAKE
54415	BIKE HANDLEBAR ABV SHLDS	54721	VEH W/AIRBRKS PRES GA REQ
54436	FOG LAMPS VIOL	54722	AIR PRESS WARNING DEV REQ
54438	NO COMMERCIAL VEH LIC/ETC	54723	OBSTRUCT OF LIC PLATE
54441	LIC LOST/ETC:DESTROY ORIG	54726	LIC PLT:LVSTCK TRLR VIOL
54442	SIDE LAMPS VIOL	-	-

Notes: All traffic offense names and codes for violations that are considered minor traffic stops (equipment or non-moving) violations are listed. I define a specific violation as minor traffic stops if more than a third of the total offense was classified as non-moving or equipment violation by the police officer.

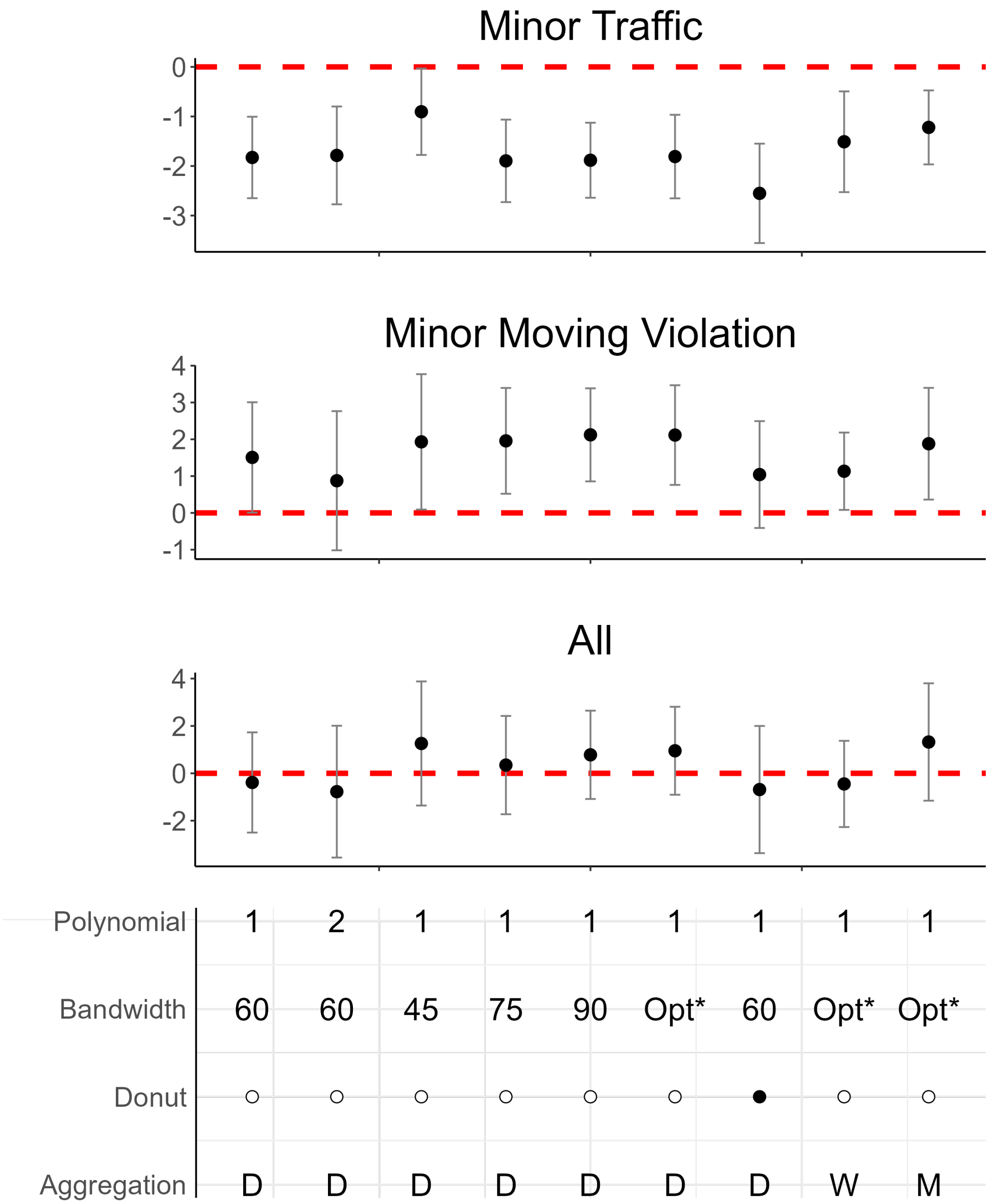
Appendix Table B.2: Characteristics of Minor & Other Minor Moving Violation

	Minor Stops	Other Minor Moving
Share of All Stops	31.027	44.144
Percent Black	30.631	20.408
Percent Hispanic	54.598	48.557
Percent Warning	67.431	31.759
Percent Citation	24.855	61.739
Percent Searched	24.406	12.023
Contraband Discovery Rate	23.596	25.508

Notes: The sample is restricted to October 1, 2021, to February 28, 2022. Column 1 presents the characteristics of minor, likely pretextual stops, while Column 2 shows the characteristics of stops due to other minor moving violations. The share of all stops is calculated by dividing the number of each type of traffic stop by the total number of police-initiated stops. The percentage of Black and Hispanic individuals stopped is calculated by dividing the number of stops involving Black or Hispanic individuals by the total number of individuals stopped. The percentages of stops resulting in a warning, citation, or search are calculated by dividing the number of each outcome by the total number of stops. The contraband discovery rate is defined as the total number of contraband discoveries divided by the total number of searches performed.

Appendix C. Supplemental Analysis

Appendix Figure C.1: Robustness: RDiT Model Specification

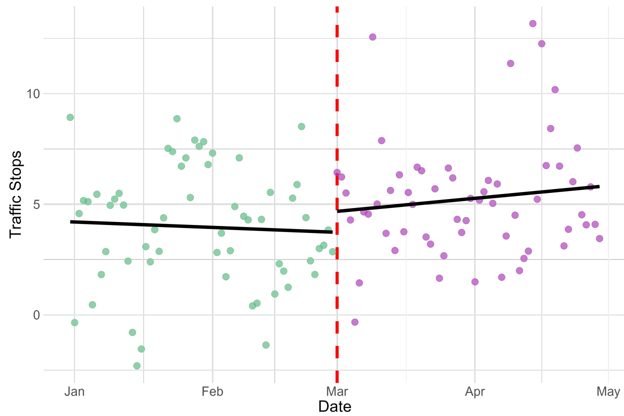


* Opt denotes optimal bandwidth estimated using procedures laid out by Calonico et al. (2015).

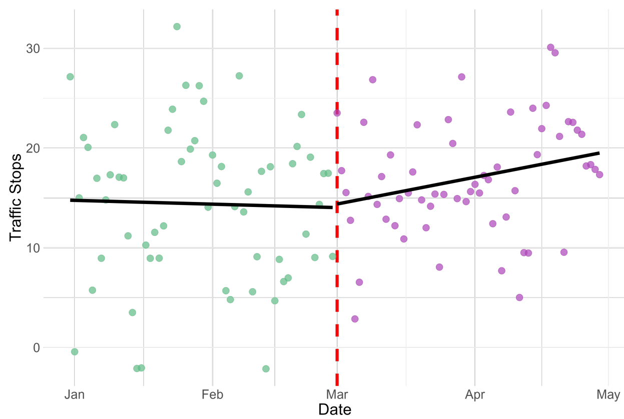
Notes: This figure shows robustness to the use of quadratic polynomial (2nd estimate), varying bandwidth, donut RD where I exclude the sample window of January 30 to March 30 (7th estimate), and aggregating the data to the weekly or monthly level (8th and 9th estimate). The first estimate (very left) shows my preferred estimate for comparison. The black dots represent my estimated treatment effect, and the bar plots represent the 95 percent confidence interval generated using Newey-West standard errors.

Appendix Figure C.2: RDiT Estimate: Police-Initiated Stops Using March 1, 2019 Treatment Date

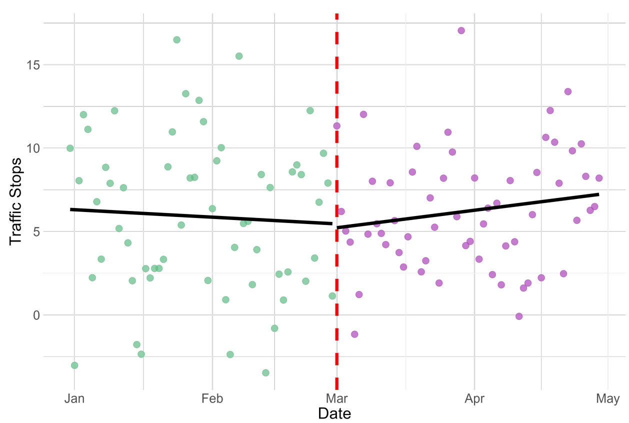
(a) Minor Traffic Violation



(b) All Stops

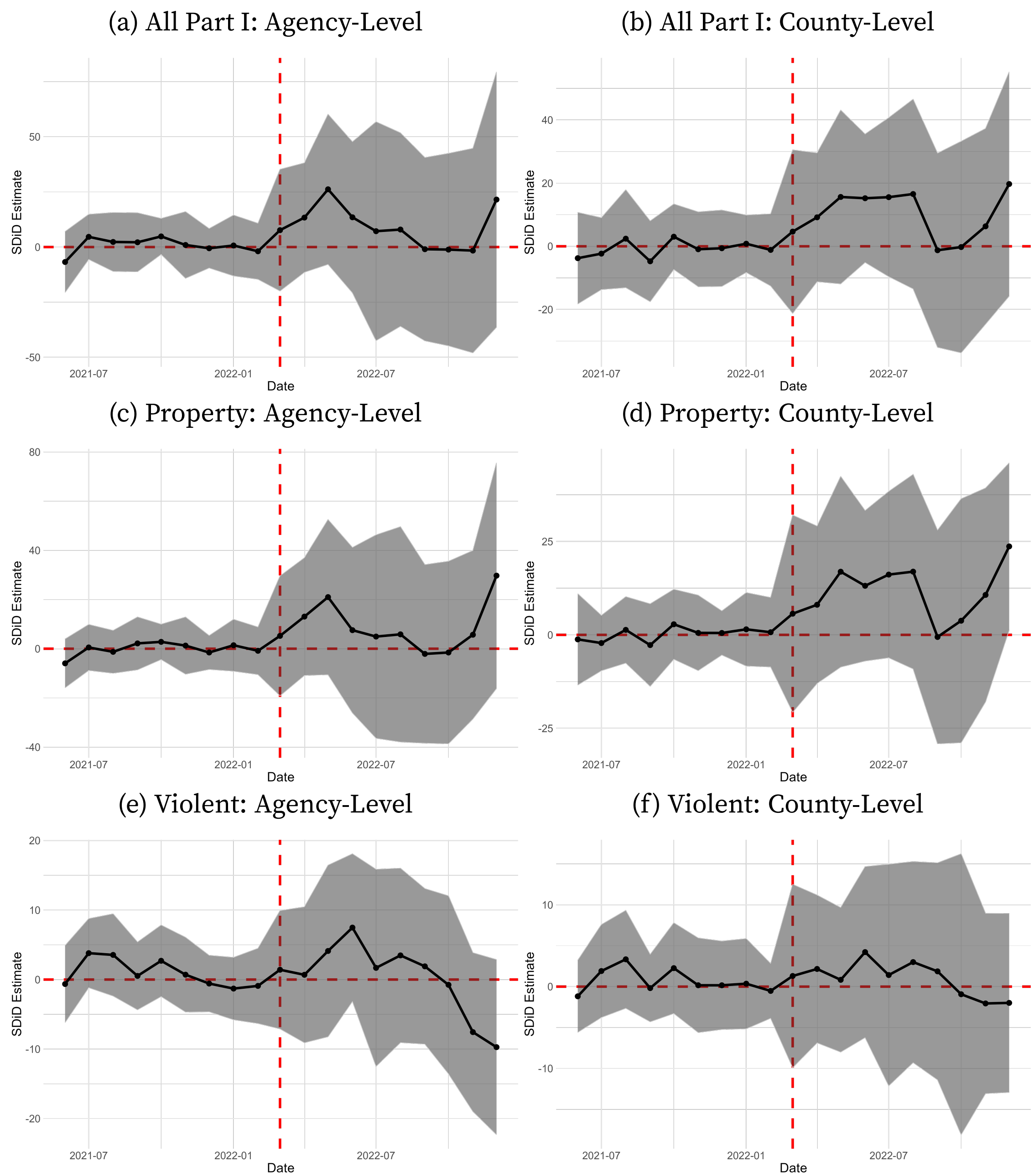


(c) Other Minor Traffic Violation



Notes: The residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022 are shown. For ease of interpretation, I added the residuals with the baseline mean of the outcome. The green color points represent my observation in the pre-treatment period. The purple color points represent my observation in the post-treatment period. The black line represents linear fit for both the pre- and post-treatment periods.

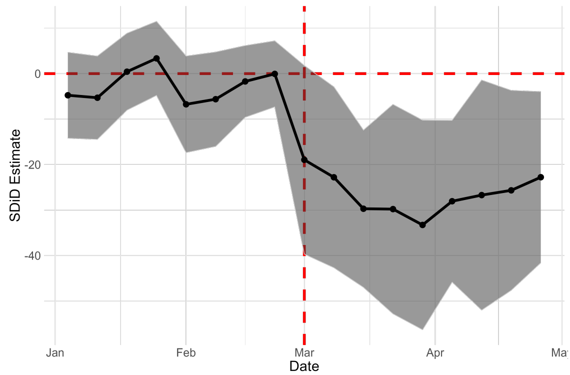
Appendix Figure C.3: SDiD Estimate: Crime Comparing Los Angeles to the Rest of California



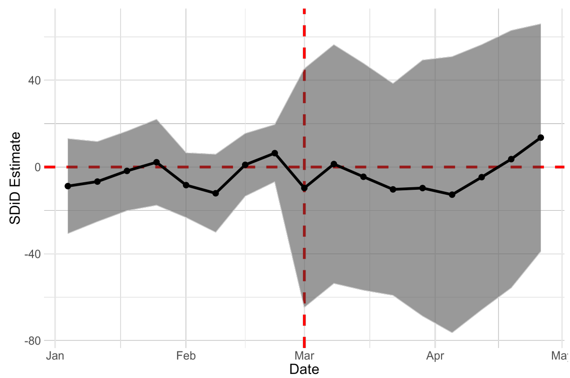
Notes: The synthetic difference-in-differences model is estimated. The left side of the panel uses agency-level data and compares LAPD (treated) to other large police agencies not in Los Angeles County. The right side of the panel uses county-level data and compares the county of Los Angeles (treated) to other large counties. The gray area represents 95 percent confidence intervals generated using placebo-based standard errors. The sample is restricted to June 2021 to December 2022.

Appendix Figure C.4: SDiD Estimate: Stops per 100,000

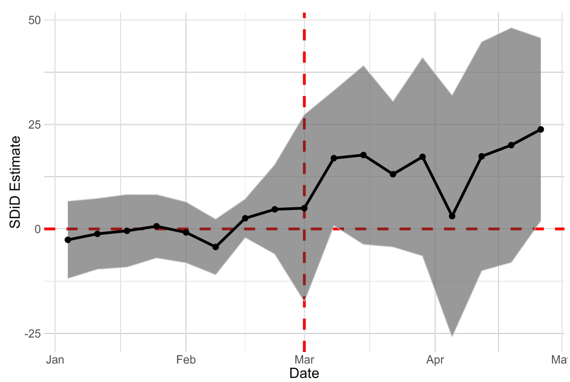
(a) Minor Traffic



(b) All Stops



(c) Other Minor Traffic Violations



Notes: The synthetic difference-in-differences model is estimated using weekly data from January 1 to April 30, 2022. The gray shaded area represents 95 percent confidence intervals generated using placebo-based standard errors.

Appendix Table C.1: RDiT: Heterogenous Effects on Infractions Across Time & Space

	Night v. Day		Non-Minor Traffic Stops		Traffic Accidents	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.158	1.474*	1.999	0.784	2.231	0.414
	(0.188)	(0.858)	(1.237)	(0.984)	(1.441)	(0.752)
Mean of DV	2.928	9.878	13.297	12.424	17.435	8.248
Sample	Night	Day	$\geq$ Median	$<$ Median	$\geq$ Median	$<$ Median

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis. Columns (1) and (2) focus on nighttime stops defined as 9 pm to 6 am and daytime stops defined as 6 am to 9 pm, respectively. Columns (3) and (4) split the sample by whether the zip code had pre-treatment (July 2018 to February 2022) total minor moving stops per capita that were above or below the median. Columns (5) and (6) follow the same logic as columns (3) and (4) but split the sample by traffic accidents per capita. Zip code level traffic accident data is obtained from the city of Los Angeles open data portal.

Appendix Table C.2: RDiT: Heterogeneous Effects by Zip Code

	Income		Minority Population	
	(1)	(2)	(3)	(4)
	Panel I: Minor Traffic Violation			
Post	-0.552**	-2.217***	-2.094***	-0.665**
	(0.243)	(0.634)	(0.617)	(0.304)
Mean of DV	2.409	7.727	7.334	2.897
	Panel II: All Other Violation			
Post	1.112	1.429	1.911*	0.360
	(0.992)	(1.192)	(1.101)	(1.011)
Mean of DV	9.494	14.974	14.209	10.524
Sample	$\geq$ Median	$<$ Median	$\geq$ Median	$<$ Median

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis. Columns (1) and (4) split the sample by whether the zip code's 2022 median household income is above or below the median. Columns (3) and (4) follow the same logic as columns (1) and (2) but split the sample by 2022 share of Hispanic and Black population. Both median household income and population are collected from the 2020 census.

Appendix Table C.3: RDiT: Number of People Stopped by Race, Other Minor Moving Traffic Stops

	Black	Hispanic	White
	(1)	(2)	(3)
	Panel I: RDiT		
Post	1.152	1.258	1.118
	(2.380)	(0.892)	(1.049)
	Panel II: Differences in Discontinuties		
Post*LAPD	0.416	0.686	0.504
	(2.438)	(0.932)	(1.079)
Mean of DV	21.948	8.977	6.738

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Appendix Table C.4: RDiT: Heterogeneous Treatment Effects on All Stops By Pre-Treatment Stops Involving Racial Minorities

	All		Black		Hispanic		White	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-1.453	0.783	-15.302***	0.165	-3.122	0.409	0.533	0.850
	(1.655)	(1.075)	(5.836)	(2.969)	(1.985)	(1.159)	(1.775)	(1.453)
Mean of DV	26.771	11.486	82.657	32.399	31.753	15.634	16.598	8.580
Sample	$\geq$ Median	$<$ Median	$\geq$ Median	$<$ Median	$\geq$ Median	$<$ Median	$\geq$ Median	$<$ Median

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis. The sample is split by whether each zip code's pre-treatment (July 2018 to February 2022) sum of racial minorities stopped per 100,000 is above or below the median.

Appendix Table C.5: RDiT: Contraband Found by Types

	Any	Firearms	Drugs	Other Contraband
	(1)	(2)	(3)	(4)
	Panel I: RDiT			
Post	-0.308**	-0.031	-0.185	-0.091
	(0.147)	(0.077)	(0.129)	(0.105)
	Panel II: Differences in Discontinuties			
Post*LAPD	-0.319**	-0.011	-0.219*	-0.089
	(0.148)	(0.078)	(0.130)	(0.107)
Mean of DV	1.907	0.451	1.053	0.402

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Appendix Table C.6: RDiT: Traffic Stop Outcomes, by Type of Stops

	Warning	Citation	Searched	Found Some-thing	Seized Some-thing	Discovery Rate	Seizure Rate	Avg. Stop Time	Use-of-Force	Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post	-1.801***	-0.188	-0.814***	-0.237***	-0.012	-2.751	0.134	1.071	-0.137	-0.076***
	(0.480)	(0.188)	(0.183)	(0.064)	(0.012)	(3.290)	(0.953)	(1.770)	(0.669)	(0.025)
Mean of DV	5.351	1.452	1.954	0.453	0.032	22.999	1.607	32.631	2.083	0.180

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Appendix Table C.7: RDiT: Traffic Stop Outcomes by Race

	Warning	Citation	Searched	Found Something	Seized Something	Discovery Rate	Seizure Rate	Avg. Stop Time	Use-of-Force	Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel I: Black									
Post	-12.034***	1.601	-5.445**	-1.487*	0.423*	1.657	2.038***	-0.255	1.094	-0.274
	(3.295)	(2.043)	(2.386)	(0.860)	(0.228)	(2.462)	(0.729)	(2.279)	(0.944)	(0.652)
Mean of DV	41.420	16.976	29.021	8.808	0.766	30.384	2.788	48.264	5.433	9.231
	Panel II: Hispanic									
Post	-2.681***	1.553*	-1.608***	-0.459***	-0.006	-0.470	0.497	0.658	-2.432**	-0.190
	(0.803)	(0.875)	(0.374)	(0.170)	(0.051)	(1.924)	(0.617)	(1.448)	(1.199)	(0.161)
Mean of DV	11.861	8.161	8.054	2.047	0.242	25.293	2.985	41.086	10.617	2.986
	Panel III: White									
Post	-0.841*	1.466	-0.003	0.149	0.039	4.334	0.527	-1.723	-0.058	-0.006
	(0.479)	(1.041)	(0.161)	(0.116)	(0.035)	(4.237)	(1.346)	(3.541)	(0.543)	(0.098)
Mean of DV	4.087	6.328	2.267	0.517	0.124	22.784	5.449	37.358	1.767	1.219

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: Regression discontinuity in time model is estimated using daily data from January 1 to April 30, 2022. The outcome used is the residuals after regressing the outcome on daily temperature, precipitation, vehicle miles traveled, and day of the week, month, and year fixed effects using data from July 2018 to December 2022. All estimates include controls for the linear function of the running variable, where the slope varies between the pre-treatment and post-treatment periods. Newey-West standard errors are reported inside the parenthesis.

Appendix Table C.8: TWFE: Reported Crime

	LAPD vs. Other Agencies			LA vs. Other Counties		
	All	Property	Violent	All	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LA	12.157	12.207	-0.050	6.271	4.871	1.401
	(22.151)	(22.494)	(3.358)	(21.825)	(20.855)	(2.770)
Rank Based P-Value	{0.441}	{0.412}	{0.962}	{0.668}	{0.729}	{0.534}
Mean of DV	276.529	207.754	68.776	244.913	195.107	49.805

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: The difference-in-differences model is estimated using monthly data. Columns 1 to 3 use an agency-by-month panel and compare the LAPD (treated) to other large police agencies not in Los Angeles County. Columns 4 to 6 use a county-by-month panel and compare the county of Los Angeles (treated) to other large counties. The standard errors estimated using placebo-based tests are reported inside the parenthesis. Rank-based p-value where the p-value is computed using a relative ranking of the average treatment effect is shown inside a curly bracket. The sample is restricted to June 2021 to December 2022, and the counterfactual includes the rest of the U.S.

Appendix Table C.9: SDiD: Crime Clearance

	LAPD vs. Other Agencies			LA vs. Other Counties		
	All	Property	Violent	All	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LA	1.081	-0.055	0.981	-2.599	-0.505	-1.011
	(5.210)	(3.347)	(2.561)	(6.632)	(2.826)	(3.020)
Rank Based P-Value	{0.842}	{0.991}	{0.652}	{0.556}	{0.882}	{0.583}
Mean of DV	38.770	12.185	26.585	35.935	14.618	21.317

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: The difference-in-differences model is estimated using monthly data. Columns 1 to 3 use an agency-by-month panel and compare the LAPD (treated) to other large police agencies not in Los Angeles County. Columns 4 to 6 use a county-by-month panel and compare the county of Los Angeles (treated) to other large counties. The standard errors estimated using placebo-based tests are reported inside the parenthesis. Rank-based p-value where the p-value is computed using a relative ranking of the average treatment effect is shown inside a curly bracket. The sample is restricted to June 2021 to December 2022, and the counterfactual includes the rest of the U.S.

Appendix Table C.10: TWFE: Reported Traffic Accidents

	LAPD vs. Other Agencies			LA vs. Other Counties		
	All	Speeding	Other Violation	All	Speeding	Other Violation
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LA	-3.608	-1.000	-1.664	-2.886	-1.544	-0.817
	(2.986)	(1.389)	(2.398)	(11.169)	(3.533)	(6.464)
Rank Based P-Value	{0.203}	{0.211}	{0.259}	{0.691}	{0.574}	{0.815}
Mean of DV	28.483	7.215	12.943	89.045	27.586	43.774

* P-val $<$ 0.1; ** P-val $<$ 0.05; *** P-val $<$ 0.01

Notes: The difference-in-differences model is estimated using monthly data. Columns 1 to 3 use an agency-by-month panel and compare the LAPD (treated) to other large police agencies not in Los Angeles County. Columns 4 to 6 use a county-by-month panel and compare the county of Los Angeles (treated) to other large counties. The standard errors estimated using placebo-based tests are reported inside the parenthesis. Rank-based p-value where the p-value is computed using a relative ranking of the average treatment effect is shown inside a curly bracket. The sample is restricted to June 2021 to December 2023, and the counterfactual includes the rest of California.