Can State Laws Address Discretion-Induced Disparities? Evidence from Traffic Stop Reforms[*]

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Abstract

Racial disparities persist in the US criminal justice system. The role of legal actor discretion in generating these disparities has received considerable attention. We examine how broad institutional changes intended to mitigate these disparities affect outcomes by studying laws in Oregon and Virginia that restricted police officers' ability to make certain discretionary traffic stops. Using both administrative traffic stop records and automobile accident data in a synthetic difference-in-differences design, we find that both policies significantly reduced stop activity, without increasing accidents. Reductions in stops were similar across non-White and White drivers in both states, though in Virginia non-White drivers experienced slightly larger declines. While Virginia agencies with disproportionately high pre-period equipment stops of non-White motorists experienced larger reductions in stop activity, these reductions did not accrue especially to non-White motorists. These findings suggest that broad institutional changes can curtail discretionary enforcement activity, but may fall short of achieving targeted reductions.

Declarations of Interest: None.

1   Introduction

The criminal justice system faces a fundamental challenge: legal actors need discretion to do their jobs well, but that discretion can allow biases to shape outcomes. A growing body of research documents how this discretion — exercised by police officers, prosecutors, and judges — can lead to racial disparities in criminal justice decisions (Arnold et al., 2022; Chen et al., 2023; Cooper and Yuan, 2023; Feigenberg and Miller, 2021; Goncalves and Mello, 2021; Hoekstra and Sloan, 2022; Rehavi and Starr, 2014). In many instances, the discretion of legal actors introduces racial disparities into the applications of laws that otherwise may have had no disparate impact.

While several studies provide evidence that discretion-induced disparities exist, less is known about the effectiveness of policies designed to mitigate them. Prior work provides evidence that targeted interventions can improve outcomes for minority populations (Parker et al., 2024), and that individual agency policy may similarly yield beneficial effects (Makofske, 2024; Matsuzawa, 2024; Naddeo and Pulvino, 2024). However, the consequences of broader institutional reforms that constrain the scope of discretion remain less well understood. Such institutional changes would define the set of actions available to legal actors, ensuring goals of the criminal justice system are still met, while minimizing the potential for bias to enter through discretionary channels. Conceptually, this approach aligns with the U.S. legal doctrine of disparate impact, which prohibits policies that disproportionately harm protected groups when the same policy goals could be achieved through alternative measures that impose less harm on those groups.

In this paper, we study these types of institutional changes in the states of Oregon and Virginia. The policies we consider aimed to reduce racial disparities arising from police officer discretion in traffic stops. Both of these policies limited enforcement discretion by prohibiting officers from making traffic stops for certain non-moving violations. We estimate the effect of each law change separately for Oregon and Virginia police agencies, using data from agencies in untreated states as controls in a synthetic difference-in-differences design.

Our setting has several advantages for studying institutional solutions to discretion-induced disparities. First, traffic stops represent a situation where otherwise race-neutral laws may have a disparate impact due to the discretion of the responsible legal actor. Indeed, discretion in the enforcement of apparently race-neutral laws such as speed limits and licensing requirements has led to disparate outcomes for minorities (Goncalves and Mello, 2021; West, 2024). Second, traffic stops provide a clean setting to study the interaction of policy and discretion. Compared to other criminal justice contexts that involve discretion of legal actors, traffic stops represent relatively isolated enforcement decisions where discretion operates along comparatively narrow dimensions. Third, traffic stops represent the most common police-initiated interaction between civilians and the police. Finally, the policies we study represent explicit attempts to achieve a reduced disparate impact through changing institutional rules, rather than by a targeted intervention or isolated policy change.

The Oregon law change we study became effective in March of 2022, and prohibited officers from making traffic stops based solely on head or tail-light violations. It also required officers to inform drivers of their right to refuse a consent search. The Virginia law change became effective

in March 2021, and prohibited officers from making traffic stops for lights-related violations, defective equipment, and a number of other non-moving infractions. At the time these laws were passed, proponents suggested that decreasing officer discretion would reduce extant racial disparities in policing, while opponents cited concerns for public safety.[1]

We estimate the effects of the Oregon and Virginia law changes by analyzing administrative traffic stop records as well as fatal and non-fatal vehicle accident reports. Using data from untreated agencies and states as controls, we estimate the causal effect of each policy change using a synthetic difference-in-differences control (Arkhangelsky et al., 2021). We estimate the overall effect of each state law change, as well as heterogeneous effects on traffic stops by the race of driver and pre-period stopping behavior of police agencies.

We find that both the Oregon and Virginia law changes significantly reduced traffic stop activity without increasing motor vehicle accidents. Oregon motorists experienced 15 percent declines in overall traffic stops, driven by large reductions in the affected equipment stops. In Virginia, motorists experienced 19 percent declines in stops, again due to reductions in equipment stops, and marginally significant declines in post-stop searches and arrests. There was no increase in either fatal or overall accidents in Oregon or Virginia.

The reductions in traffic stop activity caused by these law changes were fairly similar across non-White and White drivers. In Oregon, non-White and White motorists both experienced a 13-14 percent decline in overall stops, with no significant changes in search or arrest volume. In Virginia, non-White motorists experienced a 22 percent decline in stops, compared to 18 percent for White motorists. Non-White motorists in Virginia also experienced significant reductions in post-stop searches and arrests, while non-White drivers experienced only noisy declines. When we consider agencies that stopped non-White motorists for equipment violations at disproportionately high rates in the pre-period, we find no evidence of shrinking race gaps in stop rates. Taken together, our finding suggest that these broad policies did reduce discretionary enforcement, but did not achieve the intended decrease in racial disparities in stops.

Our study contributes to a growing literature surrounding legal actor discretion and racial disparities. Several studies document instances where police officer discretion leads to disparate outcomes. Settings include police response to 911 calls (Hoekstra and Sloan, 2022), police monitoring and patrol patterns (Chen et al., 2023), pedestrian stops (Fryer, 2019), and traffic stops (Goncalves and Mello, 2021; Pierson et al., 2020; West, 2024). Other studies consider how policy affects legal actor discretion (Bjerk, 2005; Cooper and Yuan, 2023; Kessler and Piehl, 1998; Lacasse and Payne, 1999; Starr and Rehavi, 2013; Yang, 2015).[2]

A small number of recent studies investigate targeted efforts to reduce discretion-induced disparities in traffic stops. Naddeo and Pulvino (2024), Matsuzawa (2024), and Makofske (2024) all study department-initiated efforts to reduce disparities arising from discretionary enforcement of traffic laws, finding that these policies reduced likely-pretextual stops, especially for

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[1]For example, see https://www.opb.org/article/2022/03/03/bill-that-would-limit-minor-traffic-stops-heads-to-oregon-gov-kate-brown/, https://www.wtvr.com/news/local-news/northam-on-bill-that-would-make-it-harder-for-officers-to-pull-you-over-we-have-concerns.

[2]Still others discuss how civilian oversight may serve as a way to check officer discretion (Adams et al., 2025).

non-White motorists. Campbell (2024), Rivera and Ba (2024), Tebes and Fagan (2024) and Devi and Fryer (2020) all study instances of targeted oversight or policy change imposed on individual departments suspected of misconduct. These studies tend to find that oversight reduces disparities/misconduct. To our knowledge, the only other study of a state-wide effort to reduce discretion-induced disparities in traffic stops is the targeted intervention studied in Parker et al. (2024). They analyze a Connecticut program started in 2013 that identifies agencies with large racial disparities in stops, providing training and accountability with the goal of reducing disparities. They find this program reduced traffic stops of minority drivers to the tune of about 24 percent, with no impact on White drivers. Our paper complements these studies by considering the effects of broad, untargeted institutional changes meant to impose limits on officer discretion.
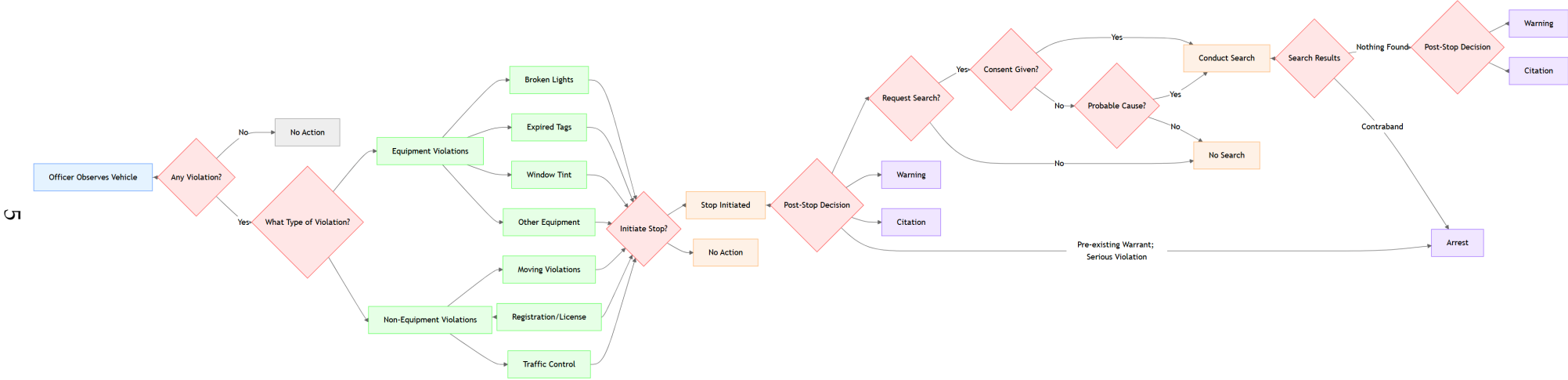
2 Background

2.1 Traffic Stops

A traffic stop is the temporary detention of a motorist by a police officer. These stops are law enforcement activities, where police officers investigate civilians suspected of a traffic law violation. A traffic stop is the most common form of law enforcement experienced by the average civilian: in 2022, an estimated 12,446,800 distinct individuals were stopped at least once by the police while driving in the United States (Tapp and Davis, 2022).

An officer faces many choices when enforcing traffic laws, which can lead to discretion-induced disparities. A visual representation of the traffic stop process is shown in Figure 1.[3] The law violations that justify a stop are numerous; violations of law can include a moving violation such as failure to signal adequately before and during a lane change, or an equipment violation such as a vision-inhibiting object hanging from a rearview mirror. Many have noted that the volume and complexity of traffic-related laws necessarily require officers to engage in selective enforcement. While all traffic laws are meant to promote safety for road users, some, including equipment-related laws, have been challenged as serving primarily as pretextual tools for officers rather than tools to promote public safety. As seen in Figure 1, a traffic stop provides opportunities for police officers to identify pre-existing warrants, visually inspect for contraband such as alcohol drugs or weapons, and to search (by consent or with probable cause) for these contraband. These post-stop opportunities provide some incentive for officers to target drivers based on non-traffic-violation-related characteristics, and to stop them under the pretext of enforcing minor traffic laws.

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[3]Specific stop procedures vary across jurisdictions in the United States. But in general, modern-day US traffic stops follow this pattern. While not yet adopted, some jurisdictions are considering removing the in-person interaction when possible and instead shifting to a contactless stop. https://www.kxan.com/news/texas/new-policing-tool-could-eliminate-the-need-for-some-traffic-stops/

Figure 1: Traffic Stop Process

Notes: An example traffic stop process. While specific stop procedures vary across jurisdictions in the United States, most modern-day US traffic stops follow a similar pattern.

2.2 The Oregon and Virginia Law Changes

The policies enacted in Oregon and Virginia emerged amid widespread public concern over pretextual and selective policing. This concern intensified following several high-profile incidents of police violence against minority individuals, including the killing of Breonna Taylor in March 2020 and the murder of George Floyd in May 2020. Although these events sparked nationwide protests, apprehension about racial disparities in policing had been mounting for years. One notable case was the 2015 death of Sandra Bland, who died by suicide after being arrested for noncompliance during a routine traffic stop. In response to such incidents and the resulting public outcry, numerous jurisdictions pursued reforms aimed at reducing racial inequities in criminal justice. The reforms in Oregon and Virginia represent some of the most comprehensive state-wide efforts to date to address discriminatory practices by limiting the discretion of law enforcement.

Oregon's reform was passed under Senate Bill 1510, which became effective on March 23, 2022.[4] [5] Two sections of the Oregon bill affected traffic stops. First, officers were prohibited from pulling over drivers due to a violation for a broken headlight, taillights, or license plate light. Second, officers were required to inform drivers of their right to refuse a consent search of their vehicle, before obtaining this consent. While language in the bill does not explicitly mention race or pretextual policing, the law was put forth with the specific intention of addressing racial disparities arising from discretionary stops. [6]

Virginia's reform was passed under House Bill 5058/Senate Bill 5029, and became effective on March 1, 2021.[7] The bill introduced restrictions on police officers' ability to stop a motor vehicle, prohibiting officers from stopping a driver for defective/unsafe equipment, improper tail/brake lights, unapproved window tints or stickers, suspended objects obstructing view of the driver (e.g., air fresheners), expired vehicle inspection (within 4 months of expiry), odor of marijuana, and loud exhaust.[8] Similar to the Oregon law change, discussion around this bill centered on racial disparities in policing.[9]

In both Oregon and Virginia, these changes were officer-oriented. While these non-moving violations remained illegal, the law changes specified that officers were now prohibited from

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[4]The full text of the bill can be found here: https://olis.oregonlegislature.gov/liz/2022R1/Downloads/Measure-Document/SB1510.

[5]The sections of the bill affecting traffic stops have a listed operational date of January 1, 2023. However, coverage of the bill seemed to emphasize the effective date of the bill, which was March 23, 2022 (see https://www.opb.org/article/2022/03/03/bill-that-would-limit-minor-traffic-stops-heads-to-oregon-gov-kate-brown/. As will be evident in our analysis, SB 1510's effective date in March appears to have led to immediate changes in policing behavior. Thus, we use the March 23, 2022 date as the treatment date in Oregon.

[6]See, for example, https://www.aclu-or.org/en/press-releases/legislatures-passage-sb-1510-transforming-justice-omnibus-bill-address, https://www.statesmanjournal.com/story/news/2022/03/04/oregon-bill-to-keep-police-from-stopping-drivers-for-minor-violations-passes/65063351007/.

[7]The full text of the bill can be found here: https://legacylis.virginia.gov/cgi-bin/legp604.exe?202+ful+CHAP0051.

[8]The provision for loud exhaust was repealed in July 2022. See
https://law.lis.virginia.gov/vacode/title46.2/chapter10/section46.2-1049/.

[9]See, for example, https://virginiamercury.com/2020/10/02/virginia-lawmakers-pass-bill-banning-pretextual-traffic-stops-and-searches-based-on-the-smell-of-marijuana/.

making a traffic stop based solely on the occurrence of the affected non-moving violations. Officers could still stop people for violations not addressed in the revised state code, and at these stops could issue citations for the affected non-moving violations.

3 Data

We use administrative traffic stop records from Oregon, Virginia, California, Connecticut and North Carolina. The Oregon data were provided by the Research Division at the Oregon Criminal Justice Commission. The Virginia data are made publicly available by the Virginia Community Policing Act.[10] The California data are collected per the Racial and Identity Profiling Act and are made available through the state's Open Justice data portal.[11] The Connecticut data are made publicly available by the Connecticut Racial Profiling Prohibition Advisory Board.[12] The North Carolina data were provided by the North Carolina State Bureau of Investigations.

The stops datasets contain three types of information, all at the level of an individual traffic stop. First, they contain demographic information about the driver of the stopped vehicle, including race and gender. Second, they contain information about the agency of the officer making the stop. Third, they contain information about the stop and its outcomes. The justification for the stop is included in every state, with differing degrees of detail. All states report whether or not a stop was based on an equipment violation, and some states provide additional detail of the type of equipment violation (e.g., broken tail light). The month and location of each stop is reported in the data from each of the five states, and the time of day is reported by California, Connecticut, North Carolina, and Oregon. The stop outcomes reported by all states include whether a search was conducted, whether an arrest was made, and whether a citation was given. The data from California, Connecticut, North Carolina, and Oregon also report whether each search was consensual or the result of the officer having probable cause, as well as whether each search yielded contraband. Additionally, the stop data from California, North Carolina and Oregon contain information on the type of any contraband discovered at search.

In our analyses of the Oregon and Virginia law changes, we consider balanced agency-month panels of traffic stops made by local police departments. Local police departments are those serving a specific city, town, or metropolitan area.[13] We form two agency-month panels for separate analyses of the Oregon and Virginia law changes. The first panel, with Oregon agencies as treated, runs from March 2021 - December 2022 and includes agencies from California, Connecticut and North Carolina as potential controls. The second panel, with Virginia agencies as treated, runs from July 2020 - December 2021 and includes agencies from California, Connecticut, North Carolina, and Oregon as controls (Oregon is untreated during this window). In each of these panels, agencies are only included if they exhibit balanced traffic stop outcome reporting over

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[10] https://data.virginia.gov/dataset/community-policing-data

[11] https://openjustice.doj.ca.gov/data.

[12] https://data.ct.gov/Public-Safety/Traffic-Stops-Racial-Profiling-Prohibition-Project/nahi-zqrt/about_data

[13] In this analysis, we do not include agencies from State Police, Sheriff's, or small entity-specific agencies such as airport or university police.

the entire analysis window. To achieve this balance, we require that agencies report at least one traffic stop in each month of the analysis window, and that traffic stop outcomes are reported without discontinuous dropoffs. Furthermore, in our main estimates we exclude agencies that serve jurisdictions with less than 10,000 residents.[14] Our final analysis sample for the Oregon law change consists of 876,844 traffic stops made by 161 agencies, and our analysis sample for the Virginia law change consists of 1,138,876 stops made by 206 agencies. Appendix Table A1 reports the sample reduction from each of these restrictions.

Table 1 presents descriptive statistics for traffic stops made in the months leading up to the state-wide law changes in Oregon and Virginia. There are a few differences across the states' agencies, including the size of populations served, the pre-period propensity to make equipment stops, and the racial makeup of drivers stopped by police varies across these states, for example with fewer Black drivers in Oregon.

We have two sources of motor vehicle accident data which we use for estimation. The first is the Fatality Analysis Reporting System (FARS), which is a nationwide census providing incident-level information about fatal injuries suffered in motor vehicle traffic crashes.[15] The second dataset contains administrative traffic accident records from Oregon, Virginia, and Connecticut, obtained from Oregon and Virginia's departments of transportation and from the Connecticut Crash Data Repository. While these data are only available from this smaller subset of states, they contain all vehicle accidents reported to the police. As the large majority of motor vehicle crashes are non-fatal, this dataset is useful to provide a deeper picture of the potential roadway safety effects of the OR and VA law changes. As with the stops analysis, we form separate panels for Oregon and Virginia. We use the fatal and all-accidents data to form agency-month panels, including the same agencies as in the traffic stops panels. Additionally, we form state-month panels which collapse data from all jurisdictions in each state.

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[14]The choice to omit small agencies is motivated by conversations with data providers, who expressed concern about the consistency of reporting in smaller agencies. This choice does not meaningfully affect our main results.

[15]See https://www.nhtsa.gov/research-data/fatality-analysis-reporting-system-fars.

Table 1: Descriptive Statistics

	Treated	Control			Treated	Control			
	Oregon LPDs	California LPDs	Connecticut LPDs	North Carolina LPDs	Virginia LPDs	California LPDs	Connecticut LPDs	North Carolina LPDs	Oregon LPDs
Population Served	235,056	1,816,033	62,411	93,289	191,247	2,195,340	61,902	105,150	238,425
N (agency-months)	504	192	960	468	256	72	640	360	344
All Drivers									
Stops per 100k residents	598	386	541	811	662	430	384	837	554
Equipment Stops	0.11	0.41	0.13	0.39	0.21	0.43	0.10	0.32	0.13
Black Driver	0.08	0.20	0.22	0.47	0.44	0.23	0.22	0.46	0.08
Hispanic Driver	0.15	0.44	0.21	0.10	0.07	0.44	0.21	0.09	0.15
Other Minority Driver	0.05	0.13	0.04	0.02	0.02	0.12	0.02	0.02	0.05
Female Driver	0.34	0.29	0.38	0.40	0.39	0.27	0.36	0.40	0.33
Arrest	0.03	0.05	0.02	0.03	0.03	0.05	0.02	0.03	0.03
Search	0.03	0.11	0.04	0.06	0.06	0.14	0.05	0.06	0.03
Consent Search	0.01	0.04	0.00	0.01					
Successful Search	0.01	0.04	0.01	0.03					
Nighttime Stops	0.49	0.38	0.38	0.47					
Nonwhite Drivers									
Stops per 100k residents	148	302	229	448	343	343	165	456	134
Equipment Stops	0.11	0.42	0.16	0.41	0.23	0.44	0.12	0.34	0.12
Arrest	0.04	0.05	0.03	0.03	0.04	0.06	0.03	0.03	0.03
Search	0.04	0.13	0.05	0.07	0.07	0.15	0.06	0.07	0.03
Consent Search	0.01	0.05	0.00	0.01					
Successful Search	0.02	0.04	0.01	0.03					
Nighttime Stops	0.55	0.40	0.44	0.50					
White Drivers									
Stops per 100k residents	450	84	313	360	319	87	220	382	421
Equipment Stops	0.11	0.38	0.09	0.34	0.19	0.38	0.08	0.27	0.13
Arrest	0.03	0.04	0.02	0.02	0.02	0.05	0.02	0.02	0.03
Search	0.03	0.06	0.02	0.04	0.04	0.08	0.03	0.04	0.03
Consent Search	0.01	0.02	0.00	0.01					
Successful Search	0.01	0.03	0.01	0.02					
Nighttime Stops	0.47	0.31	0.33	0.42					

Notes: All data are from local police departments with balanced reporting over the analysis window. The data reported in these descriptive statistics come only from the pre-period. For the left panel, where Oregon is the treated unit, the preperiod is March 2021 - February 2022. For the right panel, where Virginia is the treated unit, the preperiod is July 2020 - February 2021. Data are reported at the agency-month level. Some variables are not available in all states; these instances are indicated by blank cells.

4 Empirical Strategy

We are interested in the causal effects of the Oregon and Virginia law changes. To uncover these effects, we use the balanced agency-month panels described above in a synthetic difference-in-differences design (Arkhangelsky et al., 2021). We perform our analysis separately for Oregon and Virginia, to investigate the different effects of the distinct law changes. In all regressions, we consider outcomes that are normalized to be counts per 100,000 residents served.[16]

The SDiD approach combines elements of both the synthetic control and traditional difference-in-differences approaches to policy analysis. In our setting, we use SDiD as a data-driven approach to determine the appropriate counterfactual, while maintaining the ability to conduct valid inference. For each treated state, we utilize data from all untreated agencies as potential controls for the treated agencies. Following the methods outlined in Arkhangelsky et al. (2021), we calculate optimal unit weights $\hat{\omega}_s$ to align pre-policy trends in outcomes across treated and untreated units, and time weights $\hat{\lambda}_t$ to decrease the role of pre-policy time periods that are very different from post-policy ones. We then apply these weights in a basic two-way fixed effects regression:

$$Y_{ist} = \alpha_i + \lambda_t + \gamma \cdot \text{Treat}_s \times \text{Post}_t + \varepsilon_{ist} \tag{1}$$

where Y_{ist} represents the outcome of interest (e.g., stops, searches, crashes) for agency i in state s at time t. Treat_s is equal to 1 for agencies in the treated state and 0 otherwise, and Post_t equals 1 for months after the relevant law change and 0 for months before. Agency and month fixed effects are included to account for time-invariant agency differences, and agency-invariant time differences in the panel. The coefficient of interest, γ, captures the average treatment effect of the law change. When performing statistical inference on our estimated treatment effects, we utilize the bootstrap inference method outlined in Arkhangelsky et al. (2021).

The validity of our synthetic difference-in-differences design rests on a relaxed assumption of parallel trends, and an assumption of no anticipation of treatment. Regarding the former, our two-way fixed effects model accounts for the time-invariant differences between our treated and control states, as well as commonly-experienced differences over time. Additionally, any units and time periods that are dissimilar from treated units and time periods receive less or no weight in the regression. The plots of raw data alongside the SDiD control in Figure 2 provide evidence of parallel trends in the pre-period; however, the fundamentally untestable assumption needed in order to interpret our results as causal is that the relationship between traffic stops in the treated agencies and traffic stops in the untreated agencies that receive positive weight in the SDiD control would have remained the same, had the treated state not passed its law change. For treated agencies, the change affecting traffic stops came from a state law revision, which reduces concerns about endogenous policy adoption at the agency level. For untreated agencies, we are aware of no state-wide changes in traffic stop policy enacted in control states during the analysis window. Regarding the assumption of no anticipation, this holds if agencies do not adjust their

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[16]We construct population estimates for agency jurisdictions by mapping Census population data to agency service areas using a large language model (Claude). See Appendix section A for details.

traffic stop behavior until the time of the law change. This assumption could be violated in two ways, which each would introduce a different kind of bias into our estimation. Our results would be biased away from zero if, between the announcement and the effective dates of the law change, agencies increased the use of the to-be-outlawed stops in anticipation of not being allowed to make these stops at the effective date. On the other hand, our results would be biased towards zero if agencies comply with the law change before the effective date. The plots in Figure 2 suggest some premature compliance with the law change, which would indicate our estimates for stop outcomes are potentially biased towards zero. The traffic accidents data do not display any changes near the time of policy change (see Appendix Figure A1).

5   Results

We first report results from our synthetic difference-in-differences (SDiD) estimates of the causal effects of the state law changes on overall stop outcomes. This includes volumes of stops, equipment stops, non-equipment stops, searches, arrests, and citations. In this section, we estimate changes in these outcomes for all stops, regardless of the race of the driver.

We begin with the effect of these state-wide laws on the volume of traffic stops. Plots of raw data alongside the synthetic difference-in-differences controls are reported in Figure 2. Relative to their controls, agencies in both states experience immediate and persistent reductions in equipment stops after their respective law changes. Non-equipment stops do not appear to have a sharp change.

Table 2 reports the SDiD estimates of the average treatment effects for treated agencies. As seen in the first panel, Oregon local police departments experienced an average decrease of 165 stops per 100,000 residents after the law change, or about 15 percent relative to the post-period counterfactual mean of 1074 stops per 100,000 residents. This decrease is driven by a 44 percent decline in equipment stops. We find no statistically significant change in non-equipment stops in Oregon, though the result is consistent with a noisy reduction in these stops. The second panel reports analogous estimates for Virginia local police departments, which experienced a 19 percent decrease in overall stops; stops made for equipment violations decrease by 67 percent, and non-equipment stops experienced a noisy reduction. These estimates suggest that these state-wide policies effectively reduced the incidence of traffic stops, especially those made under the justification of equipment violations. The fact that stops decreased overall, rather than remaining the same due to a compensatory increase in non-equipment violations, suggest that police officers did not simply re-code justifications for stops after the law change, but did indeed reduce the use of these discretionary stops.

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Figure 2: Traffic stops per 100k residents and SDiD control
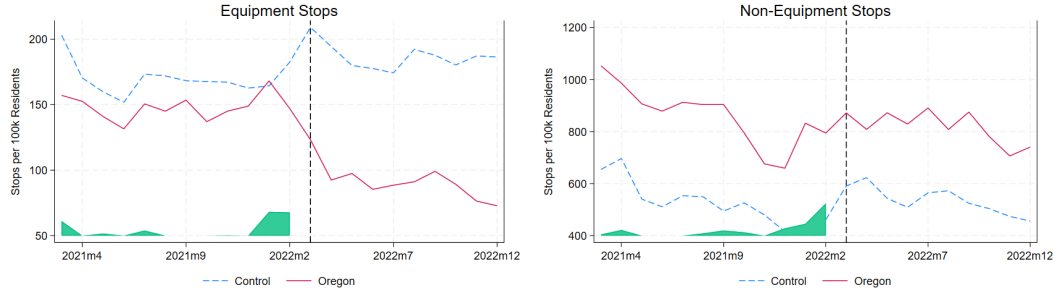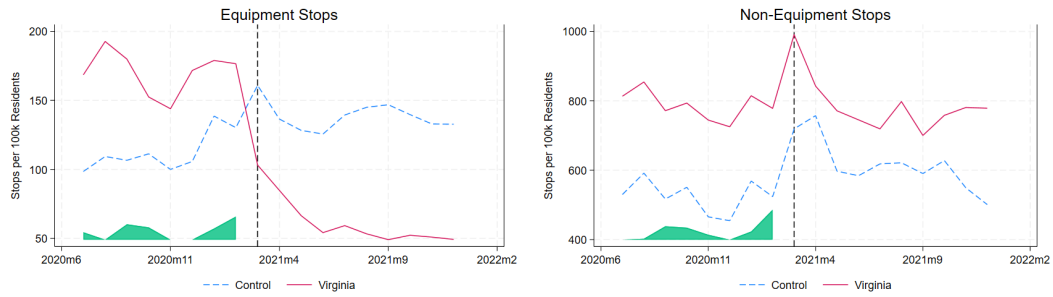
(a) Oregon

(b) Virginia

Notes: Raw data for Oregon and Virginia alongside their respective synthetic difference-in-differences control. The solid red line indicates the average number of stops per 100,000 residents made by a local police agency in the treated state. The dashed blue line indicates the synthetic counterfactual for the treated state, allowing for a difference in intercept. The shaded green area at the bottom of each plot represents the time weights $\hat{\lambda}_t$ derived in the SDiD estimation.

Table 2: Effects on traffic stops per 100k residents

	(1) all stops	(2) non-eq stops	(3) eq stops
OR × post	-164.7***	-53.35	-72.64***
	(58.06)	(46.56)	(18.08)
Post counterfactual mean	*1074.47*	*871.50*	*164.21*
Percent change (95% CI)	(-26%, -5%)	(-17%, 4%)	(-66%, -23%)
N (agency-months)	3872	3872	3872
VA × post	-203.9**	-81.39	-129.4***
	(85.32)	(66.73)	(29.05)
Post counterfactual mean	*1054.70*	*869.90*	*191.74*
Percent change (95% CI)	(-35%, -3%)	(-24%, 6%)	(-97%, -38%)
N (agency-months)	3762	3762	3762

Notes: Separate SDiD estimates for Oregon and Virginia policies. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.

 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We next report estimates of the effects of these laws on post-stop outcomes in Table 3. In both Oregon and Virginia, citations issued at traffic stops noisily decline, with point estimates proportional to the observed changes in overall stop volume reported in Table 2. Estimates for changes in post-stop searches and arrests are somewhat imprecise, due to the relative infrequency of these outcomes. However, the patterns in Oregon and Virginia appear to differ. In Oregon, estimates of changes in post-stop search and arrest volume are not statistically distinguishable from zero.[17] In Virginia, post-stop searches significantly decrease by 37 percent, though the confidence interval is wide. The point estimate for post-stop arrests in Virginia suggests a 57 percent reduction, but this estimate attains only marginal significance.

Table 3: Effects on traffic stop outcomes per 100k residents

	(1) citations	(2) searches	(3) arrests
OR × post	-38.75*	0.286	2.008
	(22.62)	(1.631)	(2.464)
Post counterfactual mean	*269.21*	*10.38*	*17.15*
Percent change (95% CI)	*(-31%, 2%)*	*(-28%, 34%)*	*(-16%, 40%)*
N (agency-months)	3872	3872	3872
VA × post	-70.18	-19.02**	-27.77*
	(61.85)	(9.095)	(15.05)
Post counterfactual mean	*519.48*	*51.56*	*48.39*
Percent change (95% CI)	*(-37%, 10%)*	*(-71%, -2%)*	*(-118%, 4%)*
N (agency-months)	3762	3762	3762

Notes: Separate SDiD estimates for Oregon and Virginia policies. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.1 Heterogeneity by race of driver

We next consider the effects of these state law changes on stop outcomes separately by the race of the stopped vehicle's driver. To provide similar estimates across the two demographically different states, we separate drivers in both states into two groups: non-White and White. We note that as reported in Table 1, non-White and White drivers stopped by police in the pre-period were equally likely to have the justification for the stop be for an equipment-related violation.

Table 4 reports results by race of driver. In Oregon, our estimates suggest the law change had similar effects on non-White and White motorists. White motorists experienced a sharp 45 percent reduction in equipment stops, compared to a 37 percent decline for non-White motorists. While the ordering of point estimates and confidence interval endpoints are suggestive of White

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[17]Appendix B further analyzes the changes in Oregon searches, using detailed data not available in Virginia. This analysis suggests that the Oregon policy did not have sharp effects on searches, though drug discovery did decline.

Table 4: Policy effect heterogeneity by race of driver

	(1) all stops	(2) non-eq stops	(3) eq stops	(4) citations	(5) searches	(6) arrests
Oregon						
Nonwhite Drivers						
OR × post	-33.80**	-13.03	-11.63**	-10.48	-0.210	-0.208
	(14.79)	(12.52)	(4.911)	(7.959)	(0.892)	(0.888)
Post counterfactual mean	*235.69*	*194.77*	*31.79*	*65.73*	*2.83*	*4.67*
Percent change (95% CI)	*(-27%, -2%)*	*(-19%, 6%)*	*(-67%, -6%)*	*(-40%, 8%)*	*(-69%, 54%)*	*(-42%, 33%)*
N (agency-months)	3872	3872	3872	3872	3872	3872
White Drivers						
OR × post	-110.2**	-44.34	-58.63***	-24.83	0.394	2.488
	(45.66)	(37.92)	(15.28)	(15.38)	(1.044)	(1.878)
Post counterfactual mean	*818.08*	*680.76*	*130.05*	*200.04*	*7.65*	*12.21*
Percent change (95% CI)	*(-24%, -3%)*	*(-17%, 4%)*	*(-68%, -22%)*	*(-27%, 3%)*	*(-22%, 32%)*	*(-10%, 51%)*
N (agency-months)	3872	3872	3872	3872	3872	3872
Virginia						
Nonwhite Drivers						
VA × post	-110.4**	-40.35	-70.11***	-51.87	-16.01***	-18.66**
	(47.71)	(36.27)	(17.00)	(32.37)	(5.401)	(8.805)
Post counterfactual mean	*469.07*	*373.20*	*95.88*	*242.57*	*30.51*	*28.18*
Percent change (95% CI)	*(-43%, -4%)*	*(-30%, 8%)*	*(-108%, -38%)*	*(-48%, 5%)*	*(-87%, -18%)*	*(-127%, -5%)*
N (agency-months)	3762	3762	3762	3762	3762	3762
White Drivers						
VA × post	-106.6**	-47.23	-61.19***	-16.31	-4.094	-12.59
	(42.90)	(37.62)	(14.21)	(34.08)	(4.145)	(8.815)
Post counterfactual mean	*598.79*	*502.90*	*97.72*	*274.91*	*22.14*	*23.68*
Percent change (95% CI)	*(-32%, -4%)*	*(-24%, 5%)*	*(-91%, -34%)*	*(-30%, 18%)*	*(-55%, 18%)*	*(-126%, 20%)*
N (agency-months)	3762	3762	3762	3762	3762	3762

Notes: Separate SDiD estimates for Oregon and Virginia policies, by race of driver. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

motorists experiencing a larger scaling back of equipment stops, this apparent difference could be due to noise, as the base rate for non-White motorists is smaller. When considering other outcomes, estimates imply that non-White and White OR drivers were similarly affected by the law change, with marginally significant decreases of about 6 percent in the volume of non-equipment stops after the law change, noisy but insignificant declines in citations, and no significant changes in searches or arrests.

The results for Virginia suggest non-White motorists experienced slightly larger declines in stop activity. Non-White motorists experience a 73 percent reduction in equipment stops after the law change, compared to 63 percent for White motorists, with both groups having similar base rates. Non-White drivers experience significant declines in both searches (52 percent reduction) and arrests (66 percent reduction). Estimates for searches and arrests of White motorists are insignificant, but are consistent with noisy declines.

5.2 Heterogeneity by Agency

We next consider whether the effects of these law changes differ across different types of agencies in Oregon and Virginia. To explore this, we partition agencies in each state based on whether they stopped non-White motorists for equipment violations at disproportionately higher rates than White motorists in the pre-period, relative to the population living in their jurisdiction. We then separately estimate the effect of the ban for these two groups of agencies in each state. Figure 3 displays this partitioning as well as key descriptive facts about the effect of the policies on equipment stop volume and racial disparities in these stops.

Figure 3: Equipment stops of nonwhite drivers, relative to nonwhite population

(a) Oregon
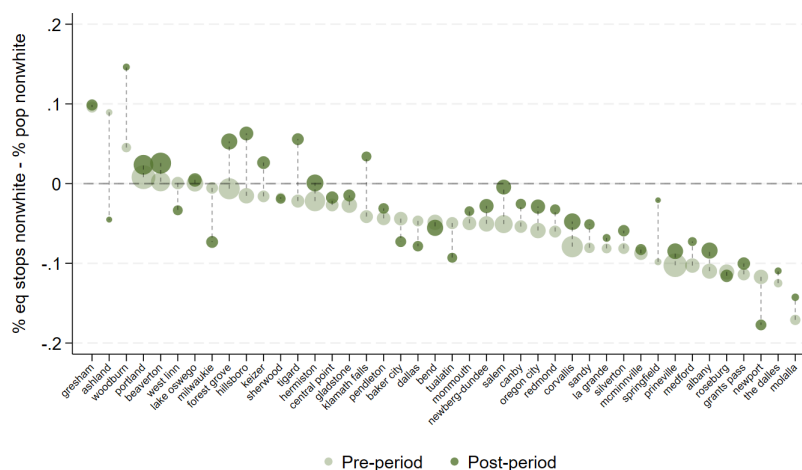
(b) Virginia
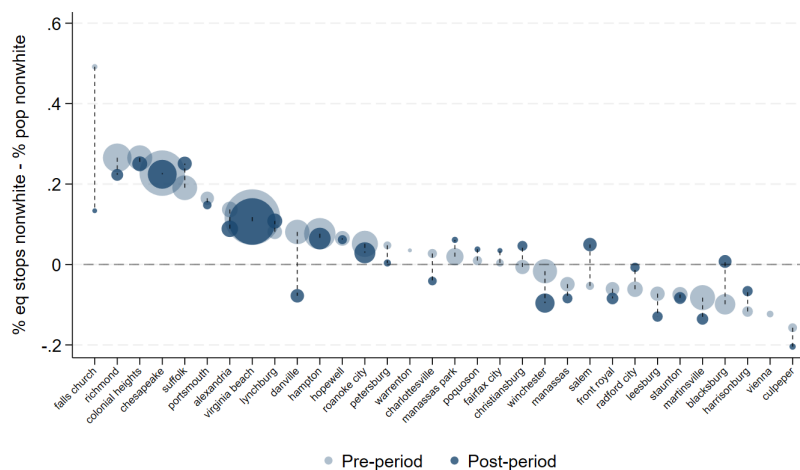
Notes: Each circle represents an agency. There are two markers for every agency, one each for the pre and post-period. The vertical axis reports the disparity in each agency's use of equipment stops against non-White motorists, relative to the population served by the agency. The size of the markers indicate the relative volume of monthly equipment stops. Agencies are ordered with those on the left that in the pre-period made the most disproportionate number of equipment stops of non-White motorists.

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A few things stand out in Figure 3. First, the volume of equipment stops appears to decrease across most agencies, as evidenced by the reduction in size of circles from pre to post. This is consistent with the estimates in Table 2. Second, the pre-period disparities observed in Oregon are different from those in Virginia, with more VA agencies having positive disparities in non-White equipment stops. Third, there are not dramatic changes in disparate use of equipment stops for non-White motorists when comparing pre and post markers for each agency, though in Virginia there are several agencies with modest reductions.

Table 5: Policy effect heterogeneity by agency pre-period non-White equipment stop intensity

	(1) all stops	(2) non-eq stops	(3) eq stops	(4) citations	(5) searches	(6) arrests
Oregon						
High-NW-EQ Agencies						
OR × post	-132.4***	-62.84*	-30.42**	-50.18	1.576	0.450
	(46.40)	(36.98)	(12.76)	(30.99)	(1.725)	(1.395)
Post counterfactual mean	*680.74*	*570.90*	*70.67*	*261.21*	*5.55*	*12.23*
Percent change (95% CI)	*(-33%, -6%)*	*(-24%, 2%)*	*(-78%, -8%)*	*(-42%, 4%)*	*(-33%, 89%)*	*(-19%, 26%)*
N (agency-months)	3102	3102	3102	3102	3102	3102
Low-NW-EQ Agencies						
OR × post	-171.1**	-52.62	-81.11***	-37.03	0.0129	2.325
	(72.76)	(58.34)	(21.48)	(22.97)	(2.032)	(2.863)
Post counterfactual mean	*1153.16*	*932.79*	*182.94*	*271.37*	*11.37*	*18.13*
Percent change (95% CI)	*(-27%, -2%)*	*(-18%, 7%)*	*(-67%, -21%)*	*(-30%, 3%)*	*(-35%, 35%)*	*(-18%, 44%)*
N (agency-months)	3718	3718	3718	3718	3718	3718
Virginia						
High-NW-EQ Agencies						
VA × post	-313.4**	-194.5**	-122.7***	-163.7*	-30.82**	-27.29*
	(122.2)	(95.31)	(33.79)	(86.36)	(12.87)	(15.93)
Post counterfactual mean	*975.91*	*808.72*	*170.95*	*506.19*	*53.39*	*41.59*
Percent change (95% CI)	*(-57%, -8%)*	*(-47%, -1%)*	*(-111%, -33%)*	*(-66%, 1%)*	*(-105%, -10%)*	*(-141%, 9%)*
N (agency-months)	3528	3528	3528	3528	3528	3528
Low-NW-EQ Agencies						
VA × post	-48.27	82.49	-133.7**	68.17	-1.002	-28.67
	(123.7)	(94.30)	(60.36)	(80.02)	(9.618)	(22.90)
Post counterfactual mean	*1174.32*	*960.73*	*216.51*	*537.20*	*48.13*	*58.51*
Percent change (95% CI)	*(-25%, 17%)*	*(-11%, 28%)*	*(-116%, -7%)*	*(-17%, 42%)*	*(-41%, 37%)*	*(-126%, 28%)*
N (agency-months)	3420	3420	3420	3420	3420	3420

Notes: Separate SDiD estimates for Oregon and Virginia policies, partitioning agencies by their use of equipment stops for non-White motorists in the pre-period. High-NW-EQ (Low-NW-EQ) agencies are those who made a disproportionately high (low) number of equipment stops of non-White motorists in the pre-period, relative to the population served by the agency. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Formal SDiD estimates of policy effects for these two groups of agencies are reported in Table 5. Within Oregon, estimates are quite similar across the low and high-non-White-equipment-

stopping agencies. Within Virginia, we observe larger and sharper reductions in equipment stops in agencies that made a disproportionate number of equipment stops of non-White motorists in the pre-period. We also observe a notable 24 percent reduction in non-equipment stops, indicating a general scaling back of stop activity for these agencies. Searches also fall notably for these agencies, with arrests and citations declining noisily as well.

Table 6: Policy effect heterogeneity by race of driver, for agencies with high pre-period non-White equipment stop intensity

	(1) all stops	(2) non-eq stops	(3) eq stops	(4) citations	(5) searches	(6) arrests
Virginia High-NW-EQ Agencies						
Nonwhite Drivers						
VA × post	-161.3**	-85.02	-78.82***	-84.27	-23.25***	-21.18*
	(81.94)	(64.67)	(22.56)	(54.06)	(8.191)	(12.33)
Post counterfactual mean	*525.80*	*421.76*	*106.62*	*274.37*	*37.47*	*29.88*
Percent change (95% CI)	*(-61%, -0%)*	*(-50%, 10%)*	*(-115%, -32%)*	*(-69%, 8%)*	*(-105%, -19%)*	*(-152%, 10%)*
N (agency-months)	3528	3528	3528	3528	3528	3528
White Drivers						
VA × post	-154.3***	-111.3***	-46.21***	-74.24**	-7.398	-7.373
	(48.84)	(39.19)	(14.90)	(35.10)	(4.582)	(4.618)
Post counterfactual mean	*452.21*	*388.82*	*66.66*	*226.65*	*15.74*	*12.97*
Percent change (95% CI)	*(-55%, -13%)*	*(-48%, -9%)*	*(-113%, -26%)*	*(-63%, -2%)*	*(-104%, 10%)*	*(-127%, 13%)*
N (agency-months)	3528	3528	3528	3528	3528	3528

Notes: Separate SDiD estimates for non-White and White drivers, for Virginia High-NW-EQ agencies. High-NW-EQ are those agencies that made a disproportionately high number of equipment stops of non-White motorists in the pre-period, relative to the population served by the agency. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results indicate the VA law led to especially pronounced reductions in stop activity for agencies with disparately high pre-period equipment stop rates of non-White motorists. To explore more about the reductions in these agencies, we repeat the heterogeneity analysis by race of vehicle driver for these agencies. These estimates are reported in Table 6. Equipment stops by these agencies reduced by about 70 percent for both non-White and White motorists, with reductions in searches and arrests that are slightly larger and sharper for non-White motorists. In all, these results suggest that while these high-disparity agencies did scale back their stop activity more than other agencies, the reductions did not accrue especially to non-White motorists.

5.3 Traffic Accidents

We next examine how these state law changes affected road safety as measured by motor vehicle accidents. The results above indicate that both the Oregon and Virginia laws led to significant and sustained decreases in equipment stops. This could affect roadway safety if these equipment stops were productive in term of removing damaged vehicles from the road, or by

uncovering other dangerous driving behavior. In Table 7, we report SDiD estimates using both FARS and DOT accident data. There are no increases in accidents, fatal or overall, with fairly tight confidence intervals for all crashes. Interestingly, we observe a small but significant decrease in overall crashes in Virginia. Appendix Figure A1 and Table A3 report further estimation results which all indicate there was no increase in accidents, even for the agencies that experienced the largest reductions in equipment stops in the post period.

Table 7: Effects on traffic accidents per 100k residents

	(1) Fatal Accidents (FARS)	(2) All Crashes (DOT)
OR × post	-0.129	1.323
	(0.146)	(2.700)
Post counterfactual mean	0.62	74.91
Percent change (95% CI)	(-67%, 25%)	(-5%, 9%)
N (agency-months)	2288	2662
VA × post	-0.147	-9.627**
	(0.262)	(4.281)
Post counterfactual mean	0.87	138.22
Percent change (95% CI)	(-76%, 42%)	(-13%, -1%)
N (agency/state-months)	2376	2772

Notes: Separate SDiD estimates for Oregon and Virginia policies. Fatal Accidents (FARS) are fatal motor vehicle accidents from the Fatality Analysis Reporting System. All Crashes (DOT) are all non-fatal and fatal accidents as reported by state departments of transportation. The FARS estimation panels contain only agencies in the main stop analysis panel. The DOT panels contain only agencies from the main stop panels from Oregon, Virginia, and Connecticut due to data availability constraints. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6 Discussion

This study investigates the effects of broad institutional changes meant to reduce discretion-induced disparities in policing. We study law changes Oregon and Virginia that limited the ability of police officers to make certain discretionary traffic stops. The Oregon law change became effective in March of 2022, and prohibited officers from making traffic stops based solely on head or tail-light violations, as well as requiring officers to inform drivers of their right to refuse a consent search. The Virginia law change became effective in March 2021, and prohibited officers from making traffic stops based solely on equipment violations of any kind. We estimate the effects of these laws separately for Oregon and Virginia, using a synthetic difference-in-differences design with data from agencies in untreated states as controls.

In both Oregon and Virginia, the implementation of the state's law change significantly decreased the volume of traffic stops without increasing motor vehicle accidents. Oregon motorists experienced 15 percent declines in overall traffic stops, driven by large reductions in the affected equipment stops. In Virginia, motorists experienced 19 percent declines in stops, again due to reductions in equipment stops, and marginally significant declines in post-stop searches and arrests. There was no increase in either fatal or overall accidents in Oregon or Virginia.

The reductions in traffic stop activity caused by these law changes accrued similarly to non-White and White drivers in each state. In Oregon, non-White and White motorists both experienced a 13-14 percent decline in overall stops, with no significant changes in search or arrest volume in Oregon. In Virginia, non-White motorists experienced a 22 percent decline in stops, compared to 18 percent for White motorists; non-White motorists in Virginia also experienced significant reductions in post-stop searches and arrests, while non-White drivers did not. In Virginia, there were many agencies that stopped non-White motorists for equipment violations at disproportionately high rates in the pre-period. These high-disparity Virginia agencies reduced their stop activity more than other agencies in the state, but the reductions were accrued similarly to non-White and White motorists proportionally to their population size, with only slightly larger reductions in searches and arrests. In Oregon, there were fewer agencies with high disparities in equipment stop use against non-White motorists, and these agencies responded to the law change much as other OR agencies.

Our findings indicate that broad institutional changes can reduce discretionary enforcement, but only bluntly. Both the Oregon and Virginia law changes achieved reductions in stops, without simply shifting justifications for stops to other violations, and without increasing motor vehicle accidents. In Virginia reductions were especially large for agencies with disparately high pre-period equipment stop rates of non-White motorists, although the reductions in these agencies did not accrue especially to non-White motorists. These results differ from studies of both department-initiated and targeted oversight policies, which have been found to decrease discretionary activity for non-White civilians, with only limited effects for White civilians. Our results suggest that broad institutional changes may be less suited to attain such targeted reductions, but can still be used to curtail discretionary enforcement activity.

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A Population estimates for police jurisdictions

We construct population estimates for police jurisdictions by mapping Census population data to agency service areas using a large language model (Claude). Our primary data source is the Census Bureau's Incorporated Places and Minor Civil Divisions Datasets: Subcounty Resident Population Estimates (SUB-EST2022).[18]

The mapping process involves two steps. First, we use Claude to determine each agency's geographic jurisdiction by searching official government sources. Second, we map these jurisdictions to Census population data. For jurisdictions not directly corresponding to Census places, Claude provides estimates from alternative credible sources. We validate the approach through manual verification of jurisdiction sources and population estimates for a random subset of agencies, finding very high accuracy. The high accuracy partially reflects the fact that most police jurisdictions align directly with Census places. This is due to the granular nature of Census' SUB-EST2022 data, which includes population estimates even for small administrative units like Connecticut townships. We also verified estimates for a subset of agencies with jurisdictions that did not map clearly to Census places, finding that estimates from Claude were consistent with other online sources.

B Oregon Searches

In Oregon, we are also able to explore the effect of the law change on search outcomes including search justification, search success, and types of contraband found in successful searches.[19] These outcomes are especially of interest in Oregon, as the state's law change included a new requirement for officers to inform stopped drivers of their right to refuse a consent search. Changes in search outcomes are reported in Table A2. We observe no significant changes in the volume of consent or non-consent searches in Oregon (columns 1-2), though confidence intervals are wide. While these noisy null results are consistent with the result from Table 3, it is notable that the law change did not affect consent searches, as it introduced a new requirement for officers to inform drivers of their right to refuse a consent search. Interestingly, in Table A2 we see a significant 16 percent decline in the volume of drug discovery (column 4), but no other significant changes in discovery including overall successful searches (column 3). Taken together, these results suggest that the Oregon policy did not have a sharp effect on overall traffic stop searches, though drug discovery did decline.

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[18]See https://www.census.gov/data/tables/time-series/demo/popest/2020s-total-cities-and-towns.html.

[19]These data are not available in Virginia.

C Supplementary Figures and Tables

Table A1: Data cleaning and sample size

	Treated	Control			Treated	Control			
	Oregon LPDs	California LPDs	Connecticut LPDs	North Carolina LPDs	Virginia LPDs	California LPDs	Connecticut LPDs	North Carolina LPDs	Oregon LPDs
As-Is from Data Provider									
Number of Agencies	106	404	94	164	209	42	94	156	105
Number of Traffic Stops	400,947	1,828,500	381,017	959,795	707,921	792,637	264,284	758,179	327,788
1+ Stop in Each Month									
Number of Agencies	76	16	87	101	127	9	87	102	74
Number of Traffic Stops	364,821	841,459	373,888	557,951	616,036	662,328	261,907	490,684	302,097
No Dropoffs in Outcome Reporting									
Number of Agencies	74	16	87	82	123	9	87	83	72
Number of Traffic Stops	338,444	841,459	373,888	287,522	566,888	662,328	261,907	266,140	278,866
Population Served > 10,000									
Number of Agencies	42	16	80	39	32	9	80	45	43
Number of Traffic Stops	286,420	841,459	363,979	226,445	249,246	662,328	256,638	221,664	235,670

Notes: Analysis sample size as different restrictions are added. The top panel reports all data, as it came from the data providers, with no cleaning. Moving down the table, we sequentially add restrictions on the data, so that lower panels include all restrictions from the upper panels. For the left side of the table, where Oregon is the treated unit, the analysis window is March 2021 - December 2022. For the right side, where Virginia is the treated unit, the analysis window is July 2020 - December 2021.

Table A2: Effects on Oregon traffic stops searches, per 100k residents

	(1) consent searches	(2) non-consent seachers	(3) successful searches	(4) alcohol discovery	(5) drug discovery	(6) weapon discovery
Oregon						
All Drivers						
OR × post	0.0801	0.242	-0.807	-0.0530	-1.102**	-0.00442
	(0.629)	(1.225)	(0.778)	(0.337)	(0.441)	(0.180)
Post counterfactual mean	4.12	6.23	5.08	1.43	2.71	0.44
Percent change (95% CI)	(-28%, 32%)	(-35%, 42%)	(-46%, 14%)	(-50%, 42%)	(-73%, -9%)	(-82%, 79%)
N (agency-months)	3872	3872	3872	3872	3872	3872
Nonwhite Drivers						
OR × post	-0.267	-0.150	-0.466	0.0557	-0.333**	-0.0543
	(0.376)	(0.759)	(0.434)	(0.181)	(0.162)	(0.103)
Post counterfactual mean	1.07	1.97	1.52	0.50	0.57	0.15
Percent change (95% CI)	(-94%, 44%)	(-83%, 68%)	(-86%, 25%)	(-59%, 82%)	(-115%, -3%)	(-166%, 95%)
N (agency-months)	3872	3872	3872	3872	3872	3872
White Drivers						
OR × post	0.256	0.331	-0.525	-0.208	-0.936*	-0.108
	(0.483)	(0.773)	(0.556)	(0.262)	(0.518)	(0.175)
Post counterfactual mean	3.14	4.32	3.74	1.03	2.31	0.44
Percent change (95% CI)	(-22%, 38%)	(-27%, 43%)	(-43%, 15%)	(-70%, 30%)	(-85%, 3%)	(-102%, 53%)
N (agency-months)	3872	3872	3872	3872	3872	3872

Notes: SDiD estimates for search-related outcomes in Oregon (these data are unavailable in VA). Outcomes are in rates per 100,000 residents served by the agency. A search is defined as successful if it yields discovery of alcohol, drugs, weapons, or other evidence. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported. In columns (3) - (5), the number of observations is lower due to lack of data availability in the control state of Connecticut.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table A3:** Effects on traffic accidents per 100k residents, robustness

	(1) FARS - Agencies	(2) FARS - States	(3) All Crashes, Agencies	(4) All crashes, High Equipment Stop Reduction Agencies
OR × post	-0.129	-0.0913	1.323	2.832
	(0.146)	(0.110)	(2.806)	(3.270)
Post counterfactual mean	*0.62*	*1.30*	*74.91*	*66.34*
Percent change (95% CI)	*(-67%, 25%)*	*(-24%, 10%)*	*(-6%, 9%)*	*(-5%, 14%)*
N (agency/state-months)	2288	1100	2662	1980
VA × post	-0.147	0.0312	-9.627**	-14.19***
	(0.262)	(0.114)	(4.424)	(4.886)
Post counterfactual mean	*0.87*	*0.97*	*138.22*	*142.79*
Percent change (95% CI)	*(-76%, 42%)*	*(-20%, 26%)*	*(-13%, -1%)*	*(-17%, -3%)*
N (agency/state-months)	2376	1122	2772	2214

Notes: Separate SDiD estimates for Oregon and Virginia policies. FARS - Agencies are fatal motor vehicle accidents from the Fatality Analysis Reporting System, collapsed to an agency-month panel mirroring the main stops analysis panel. FARS - States utilizes all FARS data, and collapses to the state level. All Crashes - Agencies are all non-fatal and fatal accidents as reported by state departments of transportation, collapsed to an agency-month panel mirroring the main stops panel. All Crashes, High Eq Reduction Agencies is a panel that retains treated units only if they were in the top quartile of agencies in terms of the reduction in equipment stops after the policy change. Outcomes are in rates per 100,000 residents served by the agency. Agency-level population measures are included as covariates in estimation. Post-period counterfactual means are reported below each row of estimates/standard errors. 95 percent confidence intervals, in terms of percent changes from the post-period counterfactual mean, are also reported.
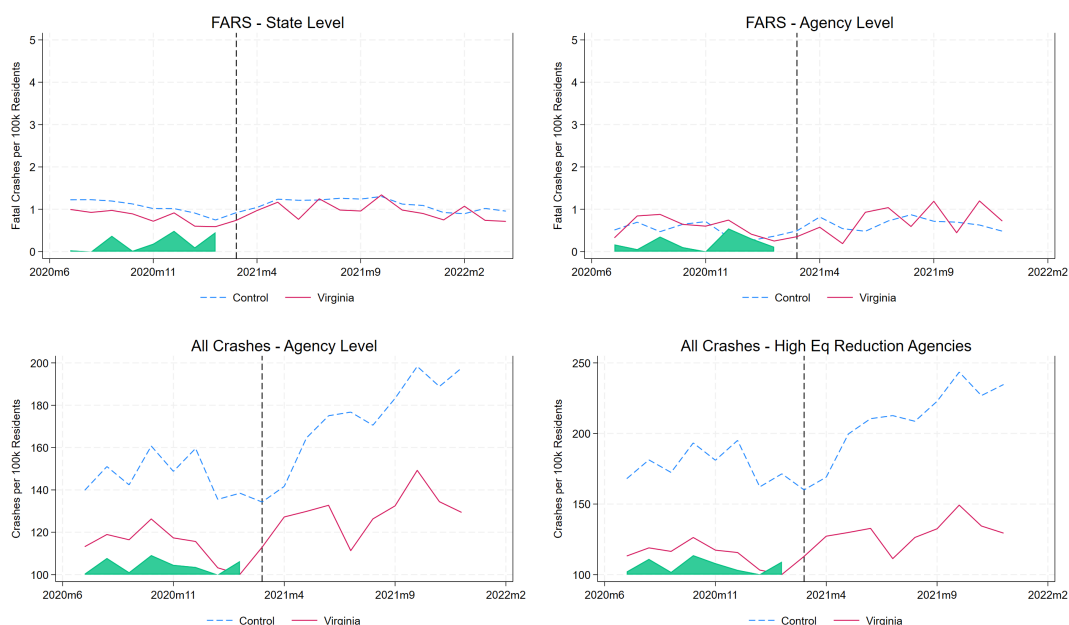
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A1: Car accidents per 100k residents and SDiD control
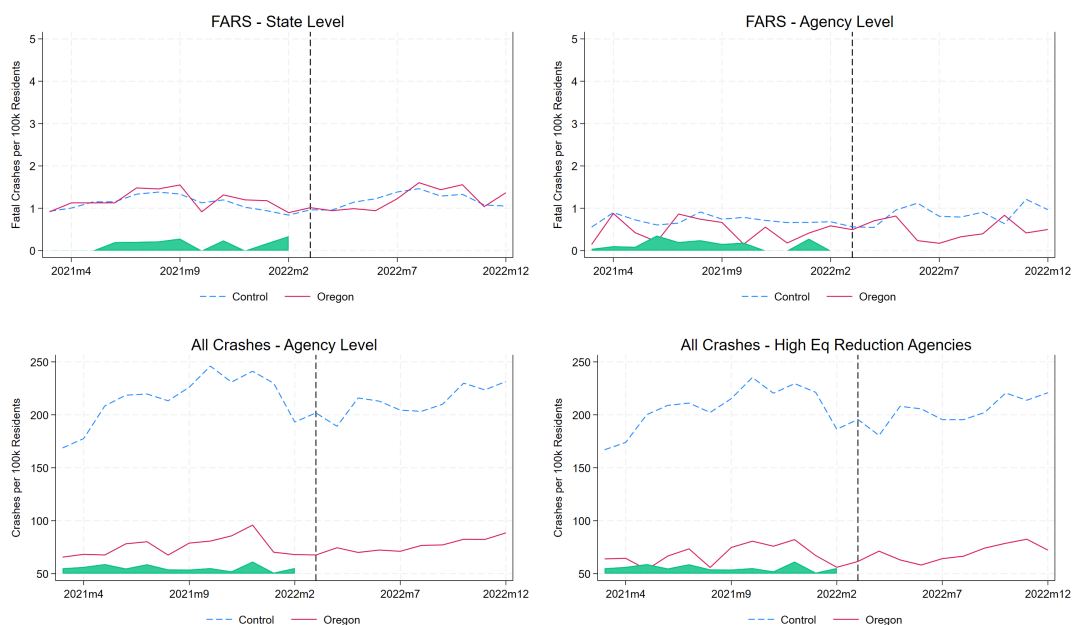
(a) Oregon

(b) Virginia

Notes: Raw data for Oregon and Virginia car accidents alongside their respective synthetic difference-in-differences control. FARS - State Level represents data from state-month panels of the Fatality Analysis Reporting System, which includes all fatal motor vehicle accidents in each state. FARS - Agency Level represents agency-month panels that mirror the composition of the main stops analysis panels, and contains only fatal accidents occurring from those jurisdictions. All Crashes - Agency Level represents data from agency-month panels that mirror the composition of the main stops analysis panels, and contains all motor vehicle accidents as provided by states' departments of transportation. The solid red line indicates the average number of stops per 100,000 residents made by a local police agency in the treated state. The dashed blue line indicates the synthetic counterfactual for the treated state, allowing for a difference in intercept. The shaded green area at the bottom of each plot represents the time weights $\hat{\lambda}_t$ derived in the SDiD estimation.