Political Party Affiliation of Leaders and Traffic Stop Policing: Evidence from North Carolina

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Abstract

I study how the party affiliation of elected sheriffs affects traffic stop enforcement in North Carolina. Using a difference-in-differences design, I find that Democratic-to-Republican sheriff turnovers, compared to Democratic-to-Democratic transitions, increase the share of Black drivers in traffic stops by 3.8 percentage points (15.7%). The increase is concentrated in moving-violation stops and reflects both broad changes among incumbent officers and personnel reshuffling. Racial disparities in searches widen within moving-violation stops, while unconditional contraband finding rates and vehicle crash counts remain unchanged. Overall, the results underscore the role of leadership in shaping frontline policing.

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1 Introduction

The criminal justice system in the United States is deeply intertwined with and influenced by partisan politics through the political process of personnel selection. Although leaders of local law enforcement agencies are often elected, the impact of their political preferences on frontline policing is not well understood. This paper examines how leaders' political party affiliation affects one of the most common interactions between Americans and law enforcement officers: traffic stops.

I examine the impact of the leader party affiliation on racial disparities in traffic stops. Racial disparities in traffic stops are well-documented. Black drivers are more likely to be stopped than White drivers, especially before sunset; during the stop process, Black drivers are twice as likely to be searched as White drivers, and they are more likely to have speeding citations than White drivers at identical speeds (Aggarwal et al., 2025; Pierson et al., 2020). A vast literature studies the extent to which racial disparities come from racial bias and has established evidence of racial discrimination at the officer level (Knowles et al., 2001; Anwar and Fang, 2006; Grogger and Ridgeway, 2006; Antonovics and Knight, 2009; Horrace and Rohlin, 2016; Goncalves and Mello, 2021). I start from a different point in the hierarchy of law enforcement agencies and ask whether leaders matter in shaping racial disparities in frontline traffic stops.

This paper focuses on Sheriff's Offices in North Carolina. I focus on sheriff's offices rather than police departments because sheriffs are elected in partisan elections. I can thus directly identify the sheriff's party affiliations. By exploiting party turnover among sheriffs induced by elections, I identify the impact of sheriffs' party affiliation on traffic stop practices in their offices. One central challenge in estimating the relationship between party affiliation of local law enforcement leaders and traffic stop practices is that localities with leaders from different parties may have unobserved differences. Such differences may make officers adopt different traffic stop strategies. In addition, time trends that affect local law enforcement practices, such as changes in crime rates and gentrification, may evolve differently across such localities.

I adopt a difference-in-differences research design to overcome these challenges. The control group is counties that experience Democratic-to-Democratic (henceforth D-to-D) sheriff transition that does not necessarily involve a leader turnover; the treatment group is counties that experience Democratic-to-Republican (henceforth D-to-R) sheriff turnover. I analyze turnovers from the 2010, 2014, and 2018 elections. For each election, I examine traffic stops in an election cycle defined as from 3 years before the election to 1 year after the election.

I find that Republican sheriffs' leadership alters the racial composition of stopped drivers.

Republican sheriffs increased the share of Black drivers by 3.8 percentage points, a 15.7% increase compared to the baseline period (one year before the election) in D-to-R counties. The estimates are robust to weighting observations by number of stops, restricting the samples to elections with no changes in the sheriffs' race, and confining the control group to D-to-D counties that will experience D-to-R turnovers in the future (not-yet-treated group). In addition, a placebo test revealed no changes in the racial composition of stops conducted by police officers in the corresponding police departments in the D-to-R counties.

Examining changes in the number of traffic stops, I find suggestive evidence that the change in racial composition is driven by a higher growth rate in the number of stops of Black drivers than non-Black drivers. The D-to-R transitions are associated with a statistically nonsignificant increase of 26 (7) log points in the number of Black (non-Black) stops, while the difference in growth rates, 19 log points, is marginally significant (p-value 0.07).

To investigate mechanisms, I decompose the changes in the Black drivers' share along two dimensions: the initial purpose of the stop and the type of officers.

Law enforcement officers have two goals in conducting traffic stops–maintaining road safety and finding contraband. The two goals motivate the distinction of two types of stops: stops due to moving violations (safety stops) and non-moving violations (investigation stops). The amount of focus a law enforcement agency should put on each type of stop is debated in North Carolina and other states. In 2013, the Fayetteville Police Department Chief proposed focusing mainly on safety stops and minimizing the number of investigation stops. In 2022, the Mecklenburg County Sheriff proposed a similar policy after the Sheriff was presented with information that Black drivers are disproportionately affected by investigatory traffic stops. Outside of North Carolina, similar proposals are seen in Seattle, Los Angeles, and Philadelphia.[1] Fliss et al. (2020) used a synthetic control method and found that the safety-focused traffic stop policy in Fayetteville reduces the share of Black drivers in overall traffic stops. This evidence prompts me to examine how differences in the focus of partisan leaders on stop types may help explain the increase in the share of Black drivers.

I find that Republican sheriffs decrease the share of safety stops by 9.1 percentage points. Such changes can have racially disparate impacts because, in the counties we analyze, Black drivers account for a lower proportion of safety stops than in investigation stops. However, I find that the change in the share of safety stops can only account for 12% of the increase in the share of Black drivers. The compositional changes of the types of stops are not the major contributor. Instead, the Black drivers' share *within* each type plays a more critical

[1] See Jallow (2021), Brown (2021), Z.Diaz (2022), and McCrystal (2023) for coverage of related policy reforms. Beland et al. (2024) finds that a policy aiming at reducing the number of minor violation stops in Los Angeles leads to a larger decrease in the search rate and use-of-force rate for Black drivers than for other drivers within minor violation stops.

role. In particular, the change in Black drivers' share *within* safety stops accounts for 63% of the overall change in Black drivers' share. These results do not imply that the safety-focused policy proposals across the United States would be ineffective in reducing traffic stop racial disparity, but the results highlight a possibility that, among the policy instruments the leaders can apply with their discretion, those that affect traffic stop practices within safety stops might be more influential in shaping racial disparities and hence require more attention.

I consider two channels through which personnel policies could affect traffic stop practices: (i) a change in incumbent officers' stop practices in response to the new leadership; (ii) a reshuffling of officers based on their policy preferences regarding traffic stops. The first channel connects with a general question: how malleable are the officers' law enforcement practices? Answers to this question are fundamental inputs to the effectiveness of a popular policy proposal—officer training (Mello et al., 2025). The reshuffling channel is in a similar spirit to discussions on how the race, gender, and tenure composition of officers affect law enforcement practices (McCrary, 2007; Ba et al., 2021; Rivera, 2025), although I do not observe officer demographics. More broadly, investigating whether reshuffling contributes to changes in law enforcement practices relates to a recent literature documenting how public sector leader party turnovers prompts personnel changes— for example, political appointee turnovers in the U.S. federal government (Spenkuch et al., 2023), and how such personnel turnovers may affect public sector performance, an example being mayor party turnover induced school personnel changes leading to worse student test scores in Brazil (Akhtari et al., 2022).

I find evidence supporting both channels. The incumbent officers, who continued to conduct traffic stops in post-election years in D-to-R counties, increased the share of Black drivers in their stops by 3.7 percentage points compared to incumbent officers in D-to-D counties, representing an 18.8% increase compared to the baseline. Furthermore, I find that the increase in the share of Black drivers among incumbent officers is not driven by a few officers, but rather by many officers having medium-level changes in their tendency to stop Black drivers. New Republican sheriffs also reshuffle the patrolling team, leading to an 18 percentage points (34%) decrease in the share of stops conducted by incumbent officers. Importantly, the officers newly shuffled in are 4.2 percentage points more likely to stop Black drivers compared to those shuffled out. These results rule out that the increase in Black drivers' share is driven by only a few incumbent or new officers, and are consistent with recent evidence that officers' traffic stop practices can systematically change at least in the short term (Mello et al., 2025).

Next, I analyze officers' decisions after stopping a driver, specifically whether to search the vehicle. I examine the impact of the sheriff's party affiliation on overall search rates and within-racial-group search rates. Note that, under the new Republican sheriff regime, relevant characteristics of the stopped driver composition (e.g., suspicion of carrying contraband) are likely to change in the post-election years. I thus interpret the impact on search rates (if any) as coming from a combination of changes on whom to stop *and* whom to search. D-to-R transitions lead to a sizable 1.4 percentage points (18%) but statistically marginally significant (s.e. 0.0086, p-value 0.0935) increase in Black drivers' search rates, while search rates for non-Black drivers do not respond to D-to-R transitions at all. The overall results mask the heterogeneity across safety and investigatory stops. Within safety stops, new Republican sheriffs statistically significantly increase racial disparity by 2.9 percentage points (p-value 0.042), from a base of 0.85 percentage points, which is not statistically different from 0. The increase in disparity is driven by a 3.2 percentage-point rise in search rates for Black drivers. On the other hand, within investigatory stops, search rates for the two racial groups barely respond to leader party turnovers. As at the stop margin, racial disparities in search rates increase only within safety stops, but not in investigatory stops, reinforcing the point that safety-stop-related policies require more attention.

Understanding whether a trade-off between racial disparities in traffic stops and efficiency exists is a central focus in the literature (Feigenberg and Miller, 2022). Motivated by the two goals of traffic stops, finding contraband and keeping roads safe, I measure the efficiency of traffic stops in two ways: the unconditional hit rate, defined as the number of searches with found contraband divided by the total number of stops, and the number of motor vehicle crashes. I find that the D-to-R transitions do not result in statistically significant changes in either efficiency measure. Given the pronounced increase in racial disparity among safety stops, it is important to pay closer attention to the unconditional hit rates for these stops. Indeed, D-to-R transitions increase the unconditional hit rates of safety stops by an economically meaningful magnitude of 1.05 percentage points (with a baseline mean of 1.6 percentage points), but the estimate is imprecise, with a standard error of 0.007 (p-value 0.15).

Finally, I examine the long-term impact by extending the election cycle to four years before and after the elections. I find that the effect of the sheriff's party affiliation on traffic stop disparities is short-lived. Three years after the elections, the gap in the share of Black drivers between D-to-R and D-to-D counties is not significantly different from the baseline year. I argue that such a short-lived impact may not be surprising, given that sheriffs face temporal electoral incentives every four years. In addition, drivers may quickly adjust their driving habits in response to the new traffic stop practices.

Overall, this paper contributes to our understanding of sources of racial disparities in the criminal justice system. Previous literature has found that partisanship influences sentencing:

compared to Democratic-appointed judges, Republican-appointed judges give longer sentences to Black offenders than non-Black offenders with similar crimes (Cohen and Yang, 2019). I provide evidence that the political party affiliation of leaders matter in determining racial disparities in frontline policing, where literature has identified the importance of the racial composition of voters the leaders face (Facchini et al., 2025), the race of the leaders (Bulman, 2019), the party affiliation of the officers (Donahue, 2023), and the racial composition of the police force (McCrary, 2007; Ba et al., 2021). Very recent literature identified heterogeneity in racial bias at the officer level (Goncalves and Mello, 2021) and suggested that officers with different levels of bias exhibit varied traffic stop behaviors in response to Trump rallies during his 2015–2016 campaign (Grosjean et al., 2022).

The impact of partisanship on law enforcement is not without ambiguity *ex ante*. Although survey evidence shows that the general public's party affiliation is correlated with attitudes toward policing policies such as body cams and police force size (Hansen and Navarro, 2021), the political preferences of law enforcement leaders across parties may not be so dissimilar. Thompson (2020) finds no effect of the party affiliation of sheriffs on compliance with federal requests to detain unauthorized immigrants and suggests that the similar compliance rate may be due to sheriffs sharing similar immigration enforcement views across parties.

More broadly, this paper contributes to the literature examining the importance of leaders and managers in the public sector. Fenizia (2022) shows that manager fixed effects can explain 9% of the total variation in the performance of processing government program claims at the office level in Italy. In the context of U.S. law enforcement, Kapustin et al. (2022) finds that some police department leaders dominate others in the sense that under their tenure, the jurisdictions not only experience less violent crime but also witness fewer civilians killed by police. I present a case in which elected leaders from a specific political party do not necessarily achieve higher productivity than those from the other party on one of the agency's tasks, but they do bring about group disparities in task execution.

The rest of the paper is as follows. I describe relevant contexts in section 2 and introduce the data in section 3. I then lay out the empirical methods in section 4. Results are discussed in section 5. I conclude in section 6.

2 Background

2.1 Law-Enforcement Agencies in North Carolina

Sheriff's offices are the principal county law enforcement agencies. They exercise jurisdiction in unincorporated areas and, by contractual arrangement, in some smaller municipalities.

Police departments are in charge of law enforcement in most incorporated areas.[2] The main functions of sheriff's offices include managing jails and detention centers, investigating crime, handling immigration detention, conducting patrols, and issuing documents such as gun permits. In this paper, I focus on traffic stops and searches. Patrol officers account for a fifth of the personnel in North Carolina sheriff's offices, while jailers and detectives/investigators account for 36% and 10%, respectively. Police departments do not manage jails. They assign more personnel to patrol and investigation: 46% to patrol and 14% to investigation.[3] Police officers conduct more stops than deputy sheriffs. Between 2008 and 2019 (my sample period), on average, deputy sheriffs made approximately 108,000 stops per year, while police officers made around 677,000.[4]

Each of North Carolina's 100 counties has one Sheriff's Office. Voters directly elect all sheriffs in North Carolina. The elections are partisan and occur every four years in November. Sheriffs have no term limits. The newly elected sheriffs are sworn in on the first Monday in December. The deputies take their oath on the same day. Since 1998, all elected sheriffs have been affiliated with either the Democratic or the Republican Party. Police chiefs, who lead police departments, are, on the other hand, appointed by the municipal government. This institutional feature—sheriffs elected while police chiefs are appointed within the same counties—allows me to use traffic stops conducted by municipal police departments as a placebo test.[5]

2.2 Traffic Stop

Law enforcement officers stop drivers either for reckless driving, such as speeding, or for non-moving violations. Non-moving violations include equipment failures such as broken taillights, vehicle regulation violations such as expired registration, and suspicion in relation to ongoing investigations. Following Baumgartner et al. (2018), I call the first type a traffic safety stop and the second type an investigatory stop. In practice, officers may use vehicle regulation violations as a pretext to stop drivers in pursuit of potential criminal investigations or drug possession searches.

By law, officers may search a vehicle if they have probable cause to believe a law has been broken, a decision that involves substantial discretionary power. Regardless of whether a

[2] Gaston and Mecklenburg county police departments have county-wide jurisdictions.

[3] The personnel numbers are from the 2016 Law Enforcement Management and Administrative Statistics (LEMAS) Survey. 22 out of 100 sheriff's offices and 72 out of 189 police departments in North Carolina are in the sample. The included agencies are larger. The median personnel size is 51. The percentage of personnel in each category is the weighted average of the shares, with the size of each agency's personnel as the weights.

[4] State highway troopers, on the other hand, conducted on average 650,000 stops yearly.

[5] I do not use traffic stops conducted by state highway troopers as a placebo test, because their districts often span multiple counties and thus cannot be directly mapped to the jurisdiction of individual sheriffs.

search is conducted, a traffic stop results in one of four actions: no action, warning, citation, or arrest. During searches, an officer might find contraband, including drugs, alcohol, or weapons.

3 Data

I combine administrative data on traffic stops and searches, motor vehicle crashes, and sheriff elections in North Carolina to examine how sheriffs' party affiliation affects officers' stop and search practices and road safety, measured by crash outcomes.

3.1 Sheriff Election Records

Sheriff's election results since 2010 are publicly available on the North Carolina State Board of Elections website. We hand-collected the 2006 election data through news articles and county board of elections websites. Party affiliation and the names of the elected sheriffs are used to determine if a county went through sheriff turnovers and/or party turnovers. Vote shares of the winners are used to assess the competitiveness of the elections.

Table 1 reports the sheriff election results from 2010 to 2018. I define the control group as the county-election cycles that experience Democratic-to-Democratic elections. The treatment group includes county-election cycles that experience Democratic-to-Republican elections. I do not focus on comparisons between Republican-to-Democratic and Republican-to-Republican transitions because the relevant sample sizes are small (10 for R-to-R and 3 for R-to-D), and the parallel trend assumption does not seem to hold.[6]

Panel E of Table 1 shows the distribution of the winners' vote share. All D-to-R elections have winners' vote shares below 80%, except the Currituck County in 2018.[7] In contrast, many D-to-D elections were non-competitive, including a substantial number with uncontested races (vote share of 1). Because non-competitive D-to-D counties may not provide valid counterfactual trends for D-to-R counties, I restrict the sample to county-cycles with winners' vote shares below 80%.

The final restriction concerns the number of yearly stops within an election cycle. This restriction serves three purposes. First, I require a balanced panel of the main outcome variable, the share of stopped drivers who are Black. To achieve this, I exclude county-cycles

[6] Panel A in the appendix table A.1 shows that Black drivers' share in R-to-R and R-to-D counties exhibits differential trends before the elections, possibly due to little overlap of urban categories shown in Panel B in the same table. The estimate of the post-election year is small and insignificant.

[7] The incumbent sheriff retired in June 2018. Matthew Beickert won the Republican primary in May, and was appointed by the Board of Commissioners in June. He later won the general election unopposed.

where the number of stops is zero in any year, as shown in Panel C of Table 1. Second, the restriction ensures consistent reporting quality over the years. Although all 100 sheriffs' offices have been legally required to report all traffic stops since 2002, some counties display large fluctuations in reported stops. For example, New Hanover reported only four stops in 2009 but 890 in 2010. Other counties report zero stops in one year and hundreds in adjacent years. Such swings raise doubts about whether the reported numbers reflect a representative sample of all stops in those counties. Third, the number of yearly stops must be sufficiently large to allow meaningful decomposition by stop type (safety vs. investigatory) and by officer type (incumbent deputies vs. others). To balance the loss of sample size against the need for data quality, I set the threshold at 50 stops per year. Panel D of Table 1 presents the resulting number of county-cycles by election type. As Panels C and D show, the restriction on the number of stops primarily excludes county-cycles with zero reported stops in a year. Relatively few additional county-cycles are lost by raising the threshold from zero to fifty. Figure 1 plots the maps of the D-to-D and D-to-R counties included in Panel D.

3.2 County and Sheriff Characteristics

Table 2 reports the county and sheriff characteristics for the county-election cycles in Panel D of Table 1. Counties may appear in multiple columns—for example, a county with a D-to-D election in 2010, a D-to-R election in 2014, and an R-to-R election in 2018 contributes to all three groups.

Panel A of Table 2 compares urban categories and population characteristics across counties with different election turnover outcomes. The difference-in-differences method does not require balance in the characteristics, but a strong overlap of the characteristics increases the credibility of the parallel trend assumption. D-to-D and D-to-R counties overlap across all three urban classifications, though D-to-R counties are more rural (60% nonmetropolitan vs. 46% for D-to-D) and have lower shares of Black and college-educated residents. These moderate differences are consistent with the Republican Party's strength in rural North Carolina, but not so large as to undermine the plausibility of parallel trends. Urban categories follow the 2013 National Center for Health Statistics census-based urban–rural classification scheme, and population characteristics (Black share, college share, poverty rate) are drawn from county-level American Community Survey data for 2010, 2014, and 2018 accessed via NHGIS.

If D-to-R turnovers coincided with changes in sheriffs' race or gender, it would be difficult to separate the effect of party affiliation from other characteristics. Panel B of Table 2 shows this is not a major concern: no D-to-R elections involve gender changes, and only one

D-to-D election does. Race changes are somewhat more common, occurring in 13% of D-to-R elections (Black to White) and 11% of D-to-D elections (White to Black).

3.3 Traffic Stop and Search Records

I obtain the traffic stop and search records from the North Carolina State Bureau of Investigation database. The dataset contains the driver's race, ethnicity, gender, and age. Officers have to report the purpose of each stop. Each stop is associated with one of the twelve stop purposes. Following Baumgartner et al. (2018), I exclude the sample associated with the checkpoints because such stops are recorded only when searches are conducted. I classify stops into two types: safety and investigation. Safety stops include speed limit violations, stop light/sign violations, driving while impaired, and safe movement violations. Investigation stops include those associated with vehicle equipment violations, vehicle regulatory violations, seat belt violations, investigation, and other motor vehicle violations. With the categorization, I construct the share of safety stops among all stops and the share of Black drivers within the two types of stops.

Unique officer IDs are included in the data.[8] The IDs are not linked to other information about officers, such as names, races, or ages. I use the officer IDs to identify two groups of officers: stayers and non-stayers. Stayers are the officers who conduct traffic stops both before and after elections. Non-stayers are the officers who conduct traffic stops only before or after the elections. Based on this categorization, I construct the following variables: the share of stops made by stayers or non-stayers, and the share of Black drivers in stops made by stayers or non-stayers.

The dataset includes the time and location of each stop. The location can be a county, a city/town, a census-designated place (CDP), or a local location name. Around 60% of the stops record location only at the county level, which significantly limits my ability to analyze officers' patrol location decisions.

The dataset contains information on searches and contraband, enabling me to derive two key outcomes: search rate and unconditional hit rate. The search rate is the number of searches divided by the number of stops. The unconditional hit rate is the number of searches with found contraband divided by the number of stops.

Summary Statistics of Traffic Stops and Searches.

Table 3 presents the summary statistics of traffic stops and searches in the D-to-D and D-to-R county-cycles in Panel D, Table 1. I report descriptive shares on race, gender, and traffic stop types. The driver is female in 35% of the stops, Black in 26% of the stops, Hispanic

[8] The officer ID is unique within the specific law enforcement agency. I am unable to track officers across different agencies.

in 7% of the stops, and White in 65% of the stops. Given the small share of Hispanic drivers, I divided drivers into Black and non-Black groups for the analysis.[9] Officers search drivers in 6.7% of stops and find contraband in 2.2% of stops. Black drivers, once stopped, are more likely to be searched than White drivers (7.9% compared to 6.1%).

When stops are divided into safety and investigation types, the driver is 28% Black in investigation stops and 24% in safety stops. Officers are more likely to search in investigation stops than safety stops (8.5% and 5.1%, respectively). The conditional hit rates (number of searches with found contraband divided by the total number of searches) are similar across the two types of stops, around 31%.

3.4 Motor Vehicle Crash Data

I obtain data on all reportable motor vehicle crashes in North Carolina from the Highway Safety Information System.[10] Deputy sheriffs patrol local streets where municipal police coverage is absent. Thus, I restrict the analysis to two types of crashes: (i) crashes on local streets outside municipal boundaries and (ii) crashes on local streets within municipalities that either lack a police department or are not required to report traffic stops. These crashes account for 3.4 percent of all crashes between 2007 and 2019. I construct four outcomes from the crash reports: (i) an indicator for whether at least one driver is Black, (ii) an indicator for whether at least one vehicle was speeding, (iii) an indicator for whether any person was injured or killed, and (iv) the number of persons injured or killed. These are based on each driver's reported race, the speed of each vehicle before impact, the posted speed limit, and the number of injuries and fatalities recorded in the crash.

4 Empirical Methods

I estimate the causal effect of sheriffs' party affiliation on traffic stop practices using a difference-in-differences method. The comparison is between counties with elections resulting in Democratic-to-Democratic transitions and those with Democratic-to-Republican transitions.

[9]The records have one race variable and one ethnicity variable. Ethnicity can be Hispanic or non-Hispanic. I define Hispanic drivers as those whose ethnicity is recorded as Hispanic, regardless of race. Accordingly, Black (White) drivers are Black (White) non-Hispanic drivers. Other races, including Asians, Native Americans, and Other/Unknown, account for around 2% of stops and are included in the non-Black group. This categorization of drivers mitigates concerns that the results are driven by misreporting Hispanic drivers as White drivers, a practice recently documented by Luh (2022).

[10]State law requires every reportable crash to be documented on a standardized DMV-349 form. A reportable motor vehicle traffic crash must include a fatality, injury, property damage of $1,000 or greater, or property damage of any amount to a vehicle seized.

In the main analysis, to capture short-term changes in enforcement practices, I define an election cycle as the period from three years prior to the year following an election.

Main specification. I estimate the average treatment effect on the treated (henceforth ATT) in two steps. First, I estimate an ordinary least squares regression with a difference-in-differences type specification. Specifically, I estimate election-cycle-specific effects from a saturated model of treatment status, calendar years, and election-cycle dummies, with county-cycle and calendar-year fixed effects:

$$\begin{aligned} Y_{cle} = \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \beta_{le} D_{cl}^{D-to-R} \cdot \eta_e \cdot \eta_l + \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot \eta_e + \\ \delta_{le} + \delta_{cl} + \epsilon_{cle}. \end{aligned} \tag{1}$$

Here, Y_{cle} is a variable at county-year level for county c in year e in cycle l. The treatment indicator $D_{cl}^{D\text{-}to\text{-}R}$ equals one if the county experiences a Democratic-to-Republican turnover in cycle l. I analyze three election cycles, 2010, 2014, and 2018 ($l \in 2010, 2014, 2018$). Within each cycle, the year of election is indexed as $e = 0$, with $e = -2$ and $e = -1$ for the two pre-election years, and $e = 1$ for the year following the election. In tables and figures, I denote the year of election as t, with $t-2, t-1$, and $t+1$ referring to other years in a cycle.

Because sheriffs are sworn in on the first Monday in December, I define a year as running from December through the following November. For example, in the 2010 cycle, the year t ($e = 0$) covers December 2009–November 2010. I call a year spanning December 2009–November 2010 as calendar year 2010

. I include county-cycle fixed effects (δ_{cl}) and calendar-year fixed effects (δ_{le}). Note that le uniquely defines each year e in cycle l, so I call δ_{le} calendar-year fixed effects. I use the year before the election ($t-1$) as the omitted base year so that I can observe any significant changes in traffic stop practices in the year of the election.

I analyze at the county level instead of the stop level because I am interested in the causal effect of leadership on law enforcement agencies, which is defined at the agency level.

In the second step of estimating ATT, I take a weighted average of the cycle-specific estimates. Weights are the observed frequency of D-to-R turnovers across cycles. Specifically, I report the following estimates:

$$\begin{aligned} \beta_e^* = \frac{\omega_{2010}^{D-to-R}}{\sum_{l=2010}^{2018} \omega_l^{D-to-R}} \cdot \beta_e + \sum_{l=2014}^{2018} \{ \frac{\omega_l^{D-to-R}}{\sum_{l=2010}^{2018} \omega_l^{D-to-R}} (\beta_e + \beta_{le}) \}, \\ e = -2, 0, 1 \end{aligned} \tag{2}$$

where ω_l^{D-to-R} is the number of D-to-R county-cycles in election cycle l. The weights for the 2010, 2014, and 2018 election cycles are respectively $\frac{4}{15}$, $\frac{6}{15}$, $\frac{5}{15}$ (see the empirical distribution of cycles in Panel D, Table 1). [11] I estimate standard errors clustered at the county level throughout the paper.

Parallel trend assumptions. The difference-in-differences method relies on the parallel trend assumption. In this paper's context, the parallel trend assumption is that the outcome variables in the D-to-R counties would exhibit the same time trend as those in the D-to-D counties after elections if the Republican candidates had not won. To probe the plausibility of the parallel trend assumptions, in section 5, I examined the trend in the main outcome, the Black driver's share, between D-to-D and D-to-R groups in a graph (Figure 2) and in regression estimations.

Concern on small sample size. The main analysis includes 62 county-cycles. With four years in an election cycle, I have 248 county-year observations. Standard errors may be large given the limited sample size, especially for outcomes such as search and unconditional hit rates, where a median county-year has only about 47 searches and 15 contraband finds (with median stops, search rate, and unconditional hit rate of 696, 0.067, and 0.022, respectively). A common strategy to reduce standard errors is to weight the group-level (county-year) observations by the population (number of stops). Whether such a strategy would empirically reduce the standard errors depends on the error term structure. Decomposing the county-year level error term (ϵ_{cle} in equation 1) into a county-level and a stop-level component, whether weighting by the number of stops would reduce the standard error depends on the size of variance of the stop-level component relative to that of the county-level component (Solon et al., 2015).

In the context of this paper, the number of stops during the election campaign and post-election may be affected by the type of election. I thus weight each county-year by the average annual number of stops in the two pre-election years ($t-2$ and $t-1$). The logic is to assign more weight to counties with more stops, hoping that the variance of the stop-level component of the error term is much larger than that of the county-level component. This weighting scheme also ensures equal weights across county-years within a county in the same election cycle.

Analyzing group differences. Section 5.1 compares the change in the number of stops and search rates across racial groups. Appendix Table A.3 reports whether the type of party transitions of sheriffs has a different impact on the number of stops in the year of elections

[11] Note that β_e^* would not be equal to the estimates from a typical two-way fixed effect specification (TWFE) because the weights in a TWFE specification are generally different from the weights derived from the empirical distribution of the treatment timings within the treatment group. For more details of the weights in a TWFE specification, see the Appendix in Gardner et al. (2024)

when the elections are close and when the incumbent candidates participate in the elections. In these instances, I estimate the following regression, a saturated model of treatment status, calendar years, groups, and election cycles dummy variables, with county cycle and calendar year fixed effects:

$$\begin{aligned} Y_{cleg} = & \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \gamma_{el}^{1} D_{cl}^{D-to-R} \cdot \eta_e \cdot G_g \cdot \eta_l + \sum_{e=-2}^{1} \gamma_{e}^{1} D_{cl}^{D-to-R} \cdot \eta_e \cdot G_g + \\ & \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \gamma_{le}^{0} D_{cl}^{D-to-R} \cdot \eta_e \cdot \eta_l + \sum_{e=-2}^{1} \gamma_{e}^{0} D_{cl}^{D-to-R} \cdot \eta_e + \\ & D_{cl}^{D-to-R} \cdot G_g + \eta_l \cdot G_g + \eta_e \cdot G_g + G_g + \delta_{le} + \delta_{cl} + \epsilon_{cle}, \end{aligned} \tag{3}$$

where G denotes groups (Black or non-Black drivers, close or non-close elections, incumbent participated elections or not, with the latter ones as the reference group). Other notations are defined as in equation 1. I report the ATT estimates for the baseline group (γ_e^{0*}) and the difference between the two groups (γ_e^{1*}):

$$\begin{aligned} \gamma_e^{0*} = & \frac{\omega_{2010}^{D-to-R}}{\sum_{l=2010}^{2018} \omega_l^{D-to-R}} \cdot \gamma_e^0 + \sum_{l=2014}^{2018} \{ \frac{\omega_l^{D-to-R}}{\sum_{l=2010}^{2018} \omega_l^{D-to-R}} (\gamma_e^0 + \gamma_{le}^0) \}, \\ \gamma_e^{1*} = & \frac{\omega_{2010}^{D-to-R}}{\sum_{l=2010}^{2018} \omega_l^{D-to-R}} \cdot \gamma_e^1 + \sum_{l=2014}^{2018} \{ \frac{\omega_l^{D-to-R}}{\sum_{l=2010}^{2018} \omega_l^{D-to-R}} (\gamma_e^1 + \gamma_{le}^1) \}, \\ e = & -2, 0, 1. \end{aligned} \tag{4}$$

5 Results

In section 5.1, I begin by documenting racial disparities in the share of Black drivers among traffic stops. Sections 5.2 and 5.3 decompose these disparities along two dimensions: the type of stop (safety vs. investigation) and the type of the officer (stayers vs. non-stayers). Section 5.5 turns to the second stage of the stop—the decision to search—while section 5.6 examines whether shifts in efficiency, measured by unconditional hit rates and the number of motor vehicle crashes, are accompanied by changes in racial disparities (as seen in section 5.1) and shifts in the share of safety stops (as seen in section 5.2). Section 5.7 considers the longer-term impacts of party affiliation of sheriffs and offers cautions for interpreting the disparities documented in section 5.1.

5.1 The Share of Black Drivers

Graphical evidence.

Figure 2 plots the raw data to show the variation captured by the difference-in-differences specification. I compute the Black drivers' share among all stops at the county-year level. I then take the simple averages across counties and election cycles to aggregate the data into D-to-D, D-to-R, and R-to-R groups. D-to-D county-cycles have higher Black driver shares than D-to-R and R-to-R county-cycles since D-to-D counties have a higher percentage of Black population, as seen in Table 2. Before the election, the gap in the share of Black drivers between the three groups remained roughly constant over the years within an election cycle. However, one year after the election, the share of Black drivers in D-to-R counties increased, while the shares in D-to-D and R-to-R counties remained largely unchanged.

Regression estimation results.

Column 1 in Table 4 presents the ATT estimates of β^*_e. These are calculated as in equation 2, based on regression estimates from equation 1, with the share of Black drivers as the outcome variable. Before the elections ($t-2$ and t), the interaction term estimates are small and non-significant, providing confidence that the parallel trend assumption is likely satisfied in this setting. Immediately after the election, the share of Black drivers increases by 3.8 percentage points in D-to-R counties compared to D-to-D counties. Given that the dependent variable mean in D-to-R counties in the year before the elections is 0.24, this amounts to a 15.7% increase in the share of Black drivers. I plot these estimates in Figure 3 to visualize their magnitudes.

I assess the robustness of the estimated impact of sheriff party affiliation on the share of Black drivers by (i) applying weights, (ii) restricting to close elections, and (iii) examining a placebo scenario. Column 2 reports ATT estimates from a regression where the county-cycle observations are weighted by each county's average annual number of stops in the pre-election years in that cycle ($t-2$ and $t-1$). If the causal impacts do not vary systematically with county size, the weighted estimates should be similar to those in Column 1. Weighting can also reduce standard errors when some county-cycles have very few stops and most of the variance in the error term comes from the stop-level component (Solon et al., 2015). Empirically, the weighted regression in Column 2 produces a somewhat smaller post-election year point estimate (3.1 percentage points), still a 15.7% increase in terms of percent changes. The standard errors, however, decrease minimally.

Column 3 restricts the sample to county-cycles where the winner's vote share was below 60 percent, a close-election setting where the parallel trends assumption is more credible. The estimated effect is smaller than in Column 1 but remains sizable (3.1 p.p.). Standard errors increase substantially, rendering the estimate statistically insignificant.

Columns 4–5 present a placebo test using traffic stops conducted by police officers in D-to-D and D-to-R county-cycles. Although deputy sheriffs and police officers patrol different neighborhoods, this test should control for the county-specific time trends, such as changes in driver population, if any. To construct the sample, I restrict to county-cycles that meet the inclusion criteria in Column 1 and additionally require that within each county-cycle, at least one police department records more than 50 stops every year in the cycle. This yields 30 D-to-D and 12 D-to-R county-cycles. Column 4 shows that ATT estimates for sheriff stops in this restricted sample are similar to Column 1. Column 5 shows that estimates using police stops in the same county-cycles are more than ten times smaller, indicating that the increase in the share of Black drivers in D-to-R counties reflects changes in deputy sheriff practices rather than shifts in the underlying trend.

Robustness: sample and control group selection criteria.

Appendix Table A.2 shows that the increase in Black drivers' share is robust to alternative samples and to control group definitions that make parallel trends more credible. Column 1 relaxes the sample criterion from ">=50 stops every year" to "> 0 stops every year" (Panel C in Table 1). The post-election party turnover effect is unchanged.

Parallel trend assumptions are likely to be more credible in more homogeneous counties and in elections that are more similar. I restrict the sample to homogeneous non-metropolitan counties, elections with no changes in sheriff race, D-to-D counties which will experience D-to-R turnovers in the future (not-yet-treated group), and D-to-D counties with sheriff turnovers. Appendix Table A.2 columns 2-5 show that the magnitudes of the post-election ATT estimates with these sample restrictions stay the same with statistical significance, except when the control group is D-to-D counties with sheriff turnovers (Column 5). Inspecting the county characteristics, the smaller magnitude in Column 5 is likely due to a worse overlap in urban categories between D-to-D and D-to-R, especially in the 2018 election cycle. Indeed, when I add urban group multiplied by year fixed effects in the specification, Panel B in Appendix Table A.2 shows that the magnitude of the ATT estimate in turnover elections is similar to other estimates.

Changes in levels.

Table 4 documents changes in Black drivers' shares; Table 5 turns to levels, asking whether more Black drivers are stopped. Columns 1–2 report ATT estimates on the number of stops by race. The post-election D-to-R coefficient is large in magnitude, about 88% of the pre-election mean for Black stops and 29% for non-Black stops. But both estimates are imprecise. We cannot reject no change at the 10% level for either group.

Columns 3–5 report ATT estimates on the natural log of the number of stops. Using natural log places Black and non-Black stops on a common scale (percent changes for moderate magnitudes) and facilitates comparison when baseline levels differ. Note that the number of stops for either group across all county-years is all positive. In Columns 3 and 4, post-election estimates are again statistically insignificant. Column 5 compares the estimated impact across groups, reporting the ATT estimates of γ_e^{0*} and γ_e^{1*} in equation 4, aggregated from a regression as in equation 5. Column 5 shows that the causal impact on Black stops is about 0.19 log points larger than for non-Black stops, and this difference is marginally significant (p-value 0.0697). Taken together, these results suggest that the rise in the share of Black drivers in Table 4 is driven by an increase in Black stops rather than a decline in non-Black stops.

Columns 1-4 in Table 5 also show a notable pattern: a decrease in the number of stops in the year of the election in D-to-R counties. I hypothesize that such a decrease may result from sheriff candidates allocating effort to campaign activities or from incumbents exerting less effort on traffic duties due to retirement (Losak and Makowsky, 2024). I test the hypotheses by estimating a triple differences specification (equation 3), with the third difference being (i) whether the elections were close (winner's vote share below 60%) and (ii) whether the incumbents participated in the elections. Appendix Table A.3 shows that the decrease in the number of stops is likely driven by *non-close* elections, and not by incumbents' participation in elections. These patterns suggest that Democratic sheriffs' offices may reduce their patrol efforts when they are more likely to lose upcoming elections.

In this section, I establish evidence that Republican sheriffs increase the number of traffic stops for Black drivers, increasing the share of Black drivers. In subsequent sections, I examine whether changes in the focus of specific types of traffic stops, the personnel, and the patrolling locations and times can explain the observed increase in the share of Black drivers.

5.2 Initial Purpose of Traffic Stops

Deputy sheriffs conduct traffic stops to improve traffic safety (safety stops) and find crimes (investigatory stops). I examine whether Republican sheriffs assign weights to the two goals differently from Democratic sheriffs, and whether such differences result in an increase in the share of Black drivers. Table 6 Column 2 shows that Republican sheriffs focus more on investigatory stops. D-to-R counties experience a 9.1 percentage-point decrease in the share of safety stops after elections, a 17% decrease relative to the D-to-R counties' mean in the year before elections.

Changes in the focus on safety and investigatory stops can have a racially disparate impact. The share of Black drivers is, on average, higher in safety stops than in investigation stops (see Table 3). Assuming that the share of Black drivers within the safety and investigation stops stay constant after the election in each county, the mere change in the share of safety stops can generate changes in the overall Black drivers' share. On the other hand, sheriffs may adopt policies that induce officers to change their practices of conducting specific types of stops, resulting in a change in Black drivers' share *within* the safety and investigation stops. Following this logic, I decompose the changes in the Black driver's share over the years into four parts: (i) the part contributed by the changes in the share of safety stops (while holding the Black drivers' share within two types of stops constant), (ii) the part contributed by the changes within the safety stops, (iii) the part contributed by the changes within the investigation stops, and (iv) the left-over second order changes. The derivation of the decomposition is in Appendix B.

Panel A in Table 6 displays the decomposition results. Note that coefficients in Columns 3-6 add up to the coefficients in Column 1. Column 3 shows that, although statistically significant, the increase in the share of investigatory stops contributes very little (12.6% of the overall change) to the increase in Black drivers' share. Changes within safety stops are the main contributor, accounting for 63% of the total changes (Column 4). The contribution from changes within investigatory stops (Column 5) is smaller in magnitude than the contribution from within safety stops (Column 4) and is statistically insignificant. Panel B more directly examines changes in racial composition across safety and investigatory stops by using the share of Black drivers in each type of stop as the outcomes. Columns 1 and 3 show that within safety stops, D-to-R transitions lead to a significant increase in Black drivers' share by 4.7 percentage points (21%), whereas no significant change is observed within investigatory stops. The results are similar when I weight the observations by the number of stops (Columns 2 and 4), suggesting that the results are not driven by small sheriff's offices.

The decomposition results show that the rise in the share of Black drivers in D-to-R counties is driven primarily by changes within safety stops, rather than by the shift from safety to investigatory stops per se. This implies that efforts to address racial disparities should target not only investigatory-stop practices but also the design of safety-stop enforcement, including neighborhood deployment priorities and speeding thresholds. Relatedly, I examine whether stop location and time can explain the increase in the share of Black drivers in section 5.4.

5.3 Personnel Policies

Officers play essential roles in shaping racial disparities in traffic stops. Literature, however, knows little about how officers respond to leadership and whether leaders assign patrol tasks based on officers' traffic stop styles, which may be related to the proportion of Black drivers in their stops. I test two mechanisms that may lead to a change in the share of Black drivers. First, the same group of officers responds to the new sheriff's leadership by changing their traffic stop practices. Second, the new sheriffs reshuffle the patrol personnel, and the differences between the officers shuffled in and out in the tendency to stop Black drivers result in a higher share of Black drivers in traffic stops.

To test the two mechanisms, I decompose the difference in the share of Black drivers at the agency level across years into four parts, as in Section 5.2. Here, the stops are categorized by who conducted them: stayers or non-stayers. The four parts in the decomposition are: (i) the part contributed by the changes in the share of stayer stops (while holding the Black drivers' share within two stayer and non-stayer stops constant), (ii) the part contributed by the changes within the stayer stops, (iii) the part contributed by the changes within the non-stayer stops, and (iv) the leftover second-order changes. The derivation of the decomposition is in Appendix B.

Panel A in Table 7 reports the results of the decomposition. D-to-R transitions lead to a sizable reshuffling, a 19 percentage-point decrease in the share of stayer stops, or a 34% drop compared to the D-to-R county's pre-election mean. Measuring reshuffling by the share of non-stayers among all patrol members over time reveals the same pattern. D-to-R transitions result in a 15.3 percentage points increase (25% compared to pre-election mean) in the share of non-stayers (Column 2, Panel C). [12]

Patrol officers who left the team, however, are similar to stayers in their tendency to stop Black drivers, as the decrease in the share of stayer stops does not lead to a change in the share of Black drivers (Column 2, Panel A). Officers who join the patrol team after elections differ significantly from those who left the team. Such differences contribute to 43% of the increase in the Black driver's share (Column 5, Panel A). On the other hand, officers who stay on the patrol team after the elections respond to the new leadership by changing their traffic stop practices, contributing to 70% of the increase in the share of Black drivers (Column 4, Panel A).[13]

To more directly compare behavior changes between stayers and non-stayers, I use the share of Black drivers among stayers and non-stayers as outcome variables and present the results in Panel B of Table 7. The variation in these outcomes over the years may be driven by a few stops in small counties, so Columns 2 and 4 in Panel B report results that weight observations by the average number of stops by officer type in pre-election years ($t-2$ and

[12] I define new officers as those whose first traffic stop in that agency, in the whole sample period (2007-2019), is recorded in that year. New officers in post-election years are a subset of non-stayers, while new officers in pre-election years can be either stayers or non-stayers. Column 4 in Panel C shows that D-to-R transitions increase the share of new officers by 18 percentage points (47%).

[13] The second order change is negative, so the sum of the other three parts exceeds 100%.

$t-1$). It turns out that the magnitude of the behavior changes within stayers is similar to the difference between non-stayers before the elections (officers shuffled out of the team) and after elections (officers shuffled into the team), a sizable increase between 3.7 and 4.2 p.p or 18.8-21.8% compared to the pre-election mean.

To shed further light on the nature of the behavior changes among stayers, I investigate whether the observed shift is widespread or concentrated among a few outliers. I calculate, for each officer, the before–and–after election difference in the tendency to stop Black drivers, measured as the average residual from regressing a Black driver indicator on stop location and time. To avoid noise from officers with few stops, I restrict the sample to those with at least 19 pre-election and 9 post-election stops—thresholds corresponding to the median in the pre- and post-election periods. The included officers account for 90–98% of all stops across D-to-D and D-to-R election cycles. Figure 4 plots the cumulative distributions of the officer-level tendency change in D-to-D and D-to-R groups. Compared to D-to-D counties, the D-to-R counties exhibit a rightward shift in both the right-tail and the middle of the distribution. This pattern indicates that the behavioral adjustment among stayers is broad-based, rather than limited to a few individuals.

I provide two takeaways from the officer analysis. First, leadership affects officers' traffic stop behaviors. A large number of stayers in D-to-R counties appear to be changing their traffic stop practices in response to the new Republican sheriffs. Second, new Republican sheriffs reshuffle the patrolling team. Those who are shuffled in had different traffic stop practices from those who are shuffled out. Both channels contribute to the increase in the share of Black drivers.

5.4 Patrol Policies

New Republican sheriffs can potentially affect the share of Black drivers by reallocating patrolling teams' resources across neighborhoods and times of the day. Table 8 presents evidence on the extent to which predictions based on the location and time of an event where a stopped driver is Black (hereafter, a Black stop), using pre-lection data, can explain the increase in the share of Black drivers in D-to-R counties.

The exercise consists of two steps. First, using stop data before the elections (2007 to the year of the election in that cycle), I regress a dummy variable indicating whether the stopped driver is Black on stop location or stop time fixed effects. I then use the OLS coefficients on the stop time and location dummy variables (unique to each county) to predict the probability of a stop with a Black driver for all observed pre- and post-election stops. Second, I compute the averages of the predicted probabilities at the county-year level and estimate ATTs in equation 2, aggregated from estimates from regressions specified as in 1 with the predicted probability averages as the outcome variable.

Table 8 reports the estimation results. Across columns, I find that the predicted probabilities of a Black stop based on time or location do not significantly change in the D-to-R county over the years. This holds true for both safety (Column 3-4) and investigation stops (Column 5-6). The estimates suggest that the overall changes in the share of Black drivers, and the changes within safety stops (as presented in section 5.2) in D-to-R counties are not explained by the patrolling neighborhoods or times observed in the data. We conclude with a cautionary note. In the estimation sample, around 60% of the stops record only the county of the stop, but not the neighborhoods. Such granularity limits my ability to detect changes in patrol focus at the sub-county level. Better data on stop locations is needed to understand the mechanisms by which law enforcement leaders influence traffic stop practices.

5.5 Search

Thus far, I have examined whether sheriffs' party affiliations affect whom to stop. I now ask who is searched. I first report the changes in overall and race-specific search rates. I then investigate heterogeneity along the dimensions of stop purpose and stop officer type. Changes in search rates should be interpreted as the *combined* impacts of the changes in both stop and search practices associated with the new Republican sheriff. The thought exercise holds the at-risk population of being stopped the same right before and after elections, instead of the stopped driver population.

Table 9 shows that overall and non-Black search rates do not respond to changes in the party affiliation of the sheriffs, while the Black search rates display a suggestive increase of economically meaningful magnitude, but with statistically marginal significance. The estimate of the Black search rate in the post-election year is large relative to the pre-election mean (0.032, or 29%), but the standard error is so large (0.023) that I cannot reject the null of zero impact (Panel A, Column 3). Weighting the county-year observations in Panel B by the average annual number of stops in the pre-election years ($t-2$ and $t-1$) maintains the magnitude and improves precision. As a result, the increase in the Black search rate is marginally significant (p-value 0.0976). However, the magnitude is sensitive to outliers. Column 4 in Panel B shows that after removing the two outliers (in terms of changes in search rates for Black drivers), the magnitude halves to 0.0145, or 18% relative to the pre-election mean (p-value 0.0835). In contrast to the meaningful magnitude of the Black search rates, the non-Black search rates change little (Panel B, Column 6). Formally testing the difference between changes in Black (Column 4) and non-Black (Column 6) groups, Column 8 finds that the increase in the search rate disparity across racial groups is statistically nonsignificant. However, the magnitude is economically meaningful (0.014) compared to the racial search rate differences in D-to-R counties one year before the election (0.023). Given the small annual search counts, I focus on results from number-of-stops-weighted, outliers-excluded sample regressions for the following discussion of search rates and unconditional hit rates.

Similar to changes in stop behavior, changes in search behavior are concentrated in safety stops. Table 10 reports the stop purpose-specific regression estimates where observations are weighted by the average annual number of stops in pre-election years. Before the elections, the search rate racial disparity appears in investigation stops (3.2 p.p. higher in Black stops with p-value 0.017), but not in safety stops (0.85 p.p. with p-value 0.15), as seen in Baseline diff., Column 8, Panel A and Panel B. The D-to-R transition increases the search rate disparity in safety stops by 2.9 p.p. (Column 8, Panel A), while leaving the search rate disparity in investigation stops unchanged from pre-election levels (Column 8, Panel B).

I find suggestive evidence that stayers increase their search rate racial disparity in response to the D-to-R transition, a 1.4 p.p marginally significant increase (p-value 0.090) with a non-significant baseline racial disparity of 1.2 p.p (p-value 0.150). Non-stayers, on the other hand, exhibit a similar magnitude of 1.1 p.p increase, but with much less precision (p-value 0.692).

Overall, the search behavior analysis along the stop purpose dimension aligns with previous findings on changes in stop behavior. Changes in search rate racial disparities stem from within safety stops. Along the officer type dimension, I find suggestive evidence that stayer officers increase the Black search rate while keeping the non-Black search rate constant in response to the D-to-R transition.

5.6 Efficiency

Deputy sheriffs stop vehicles to maintain road safety and find contraband. I examine whether changes in the efficiency of traffic stops, measured by the unconditional hit rate (the number of searches with found contraband divided by the number of stops) and the number of traffic accidents, are accompanied by an increase in the stop and search racial disparity, and a decrease in safety stop shares.

Table 11 finds no significant changes in efficiency in terms of unconditional hit rates accompanied by the D-to-R transitions. Focusing on results from number-of-stops weighted regressions with outliers-excluded sample (even number columns in Panel B), both Black and non-Black unconditional hit rates exhibit minimal changes after D-to-R transitions.[14]

[14] I focus on number-of-weighted regressions due to the small number of stops with contraband finds. Recall that a median county-year has 696 stops, and about 47 searches and 15 contraband finds.

The unconditional hit rates might exhibit heterogeneity along the stop purpose dimension, given the large increase in racial disparities in search rates among safety stops. Columns 2 and 4, Panel A in Table 12 shows that, for safety stop, although the magnitude of the changes in the overall and Black unconditional hit rates are economically meaningful, respectively 1.0 and 1.6 p.p increase compared to the baseline mean of 0.016, the standard errors are also so large that I cannot reject the null that the unconditional hit rates did not change (p-value 0.156 and 0.167 for overall and Black search rates).

Heterogeneity may also appear among different officer types. The increase in the share of new officers (presumably less experienced) in D-to-R counties in post-election years may lower unconditional hit rates if experience matters. Contrary to this prior, Column 2, Panel D shows that non-stayer-officers in post-election years do not exhibit worse unconditional hit rates in the overall stops. In particular, Column 4, Panel D shows that the newly shuffled-in officers are much better at finding contraband in Black stops compared to the officers shuffled out. Their unconditional hit rates are, on average, 2.3 percentage points higher (87%). The stayer-officers' unconditional hit rate barely changes.

Turning to motor vehicle crashes, Table 13 finds that D-to-R transitions are not associated with changes in the corresponding jurisdictions, in the number of motor vehicle accidents, the number of people injured or killed in accidents, the share of accidents involving Black drivers, the share of accidents involving speeding, and the share of accidents resulting in injury or death.

Taken together, the results in Table 4, 10, 12 and 13 imply that newly elected Republican sheriffs enact policies that induce greater racial disparities in traffic stops and searches within safety stops, without a discernible increase in efficiency.

5.7 Long(er)-term Impacts

In previous sections, I focused on the short-term impacts of partisan leadership, comparing traffic stop practices immediately after the elections with those before. A natural request is to examine the long-term impact permitted by the research design restrictions. For this purpose, I extend the event window to four years before and after the election, re-estimate equation 1 with $e \in \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$ (0 is the year of election), and aggregate to ATTs via equation 1. Sample rules match the short-term analysis (winner's vote share $<$ 80%; $>=$50 stops in each year of the cycle). The longer election cycle requires more than 50 stops in each county in all eight years, so the sample shrinks from 62 to 47 county–cycles.

Two caveats should be kept in mind in the longer-term analysis. First, in the longer term, drivers may respond to the new traffic stop practices initiated by the new sheriffs. One would then be unable to estimate the causal impact of partisan leadership on the racial composition of traffic stops, holding the at-risk driver population constant. Second, the newly elected sheriffs in the D-to-D and D-to-R counties may face different pressures for their next election. Among the counties in the estimation sample, 60% of D-to-D counties have the winner's vote share smaller than 0.8 in the next election, while 40% of D-to-R counties fall into such category. The parallel trend assumption may thus fail as the counties progress toward the next elections.

Figure 5 plots the share of Black drivers in the long cycles. The gap between D-to-D and D-to-R groups shrinks right after the elections, the same pattern as in the short election cycles in Figure 2. As we approach the end of the election cycle, the gap widens to a level similar to that in pre-election periods.

Table 14, Column 1 confirms the pattern in Figure 5. Black drivers' share increases by 3.2 percentage points in D-to-R counties one year after the elections, compared to D-to-D counties. The magnitude of the estimate remains similar for the subsequent year, but the standard errors increase. Three to four years after the election (or one to two years before the next election), the difference in the share of Black drivers between D-to-D and D-to-R counties becomes much smaller and is not statistically significantly different from the baseline-year differences ($t-1$). Weighting the observations by the average annual number of stops increases the magnitude of the coefficients for t+3 and t+4 (Column 2), suggesting that some small agencies (in terms of number of stops) may drive the decrease in the magnitudes in Column 1. The decrease in magnitudes in Column 1 can not be explained by sheriff's offices responding to police department policy changes. Column 4 shows that the share of Black drivers in stops conducted by police officers in D-to-D and D-to-R counties exhibits a similar trend throughout the electoral cycle.

Overall, the long(er)-term results provide a caution to the interpretation of the results in section 5.1. The impact of sheriffs' party affiliations on racial disparities in traffic stops may be short-lived. The short-lived impact is perhaps unsurprising: law-enforcement leaders' policy choices may be influenced by temporal incentives such as pressure from the upcoming elections. Drivers may also respond to the new traffic stop policies in a short period of time. I conclude the long(er)-term discussion by cautioning that identifying the long-term impact of leaders on traffic stops may be more challenging than other law-enforcement practices.

6 Conclusion

While literature in the past three decades provides evidence that law enforcement officers exhibit racial bias in traffic stops and searches, we know little about the role of leaders in shaping traffic stop racial disparities. In this paper, I present evidence that leaders' party affiliation affects traffic stop practices. In North Carolina, a Democratic-to-Republican sheriff turnover, compared to a Democratic-to-Democratic transition, increases the share of Black drivers among all stops by 3.8 percentage points (15.7% compared to baseline mean).

Among proposals to reduce racial disparities in traffic stops, one strand focuses on reducing the number of investigatory stops. I find that the increase in the Black drivers' share is *not* driven by changes in the composition of safety and investigation stops. Instead, it is driven by changes *within* safety stops. Another line of policy proposals focuses on officer training (Mello et al., 2025). A critical dimension determining the effectiveness of training is the malleability of officers' practices. I find that the changes in the Black drivers' share associated with Republican sheriffs are driven by broad, moderate shifts across many officers, rather than by drastic changes among a few. This pattern suggests that individual officer practice is malleable and can be shaped by leadership.

Turning to trade-offs between group disparity and overall traffic stop efficiency, I find that the increase in racial disparities does *not* coincide with changes in efficiency, as measured by unconditional hit rates or the number of motor vehicle crashes, despite that the Republican sheriffs put more emphasis on crime investigation than on traffic safety.

I end with a suggestion for future research. An obvious policy choice for sheriffs is the allocation of patrol resources across neighborhoods. Although I find no evidence that the increase in the share of Black drivers stems from Republican sheriffs reallocating patrols across neighborhoods, this conclusion is limited by the lack of detailed geographic data. One fruitful direction is to use more granular patrol information (such as patrol assignment data used in Ba et al. (2021) or smartphone location data used in Chen et al. (2025)) to shed light on how much of the within-stop purpose and within-officer changes in traffic stops can be explained by patrol locations.

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Figures

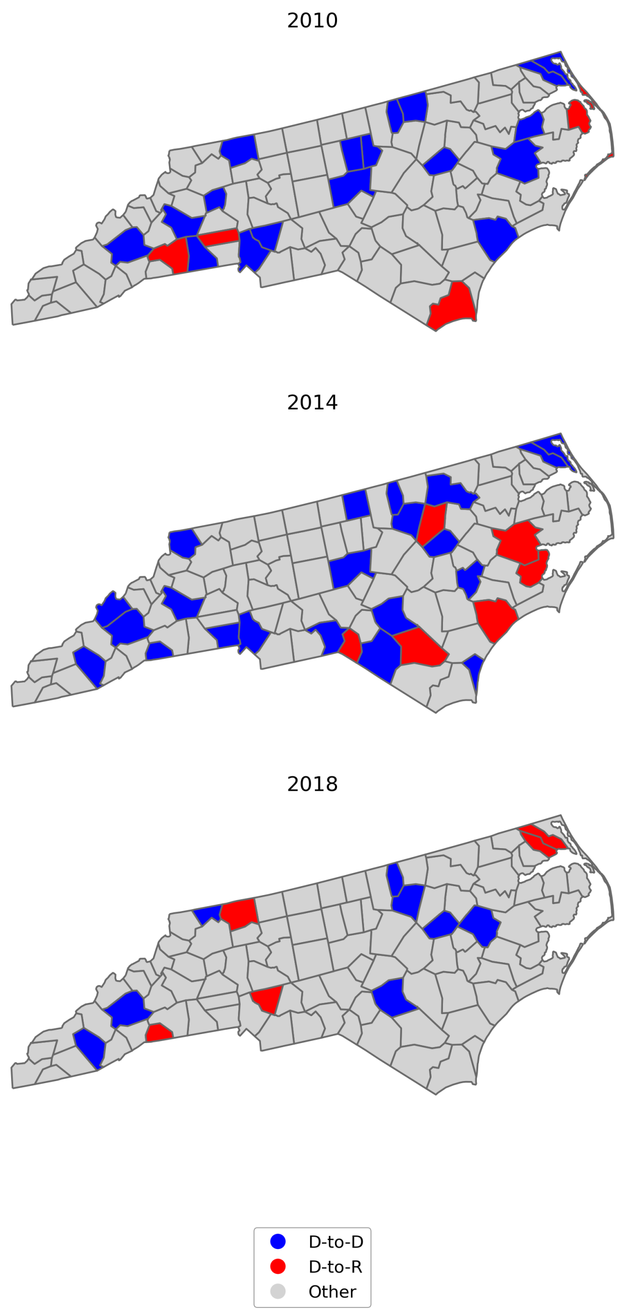

Figure 1: D-to-D and D-to-R County Maps

Notes: This figure plots the map of counties that experienced Democratic-to-Democratic and Democratic-to-Republican transitions in 2010, 2014, and 2018, and satisfy the sample inclusion criteria for the analysis. The set of counties is the same as the ones included in Panel D, Table 1.

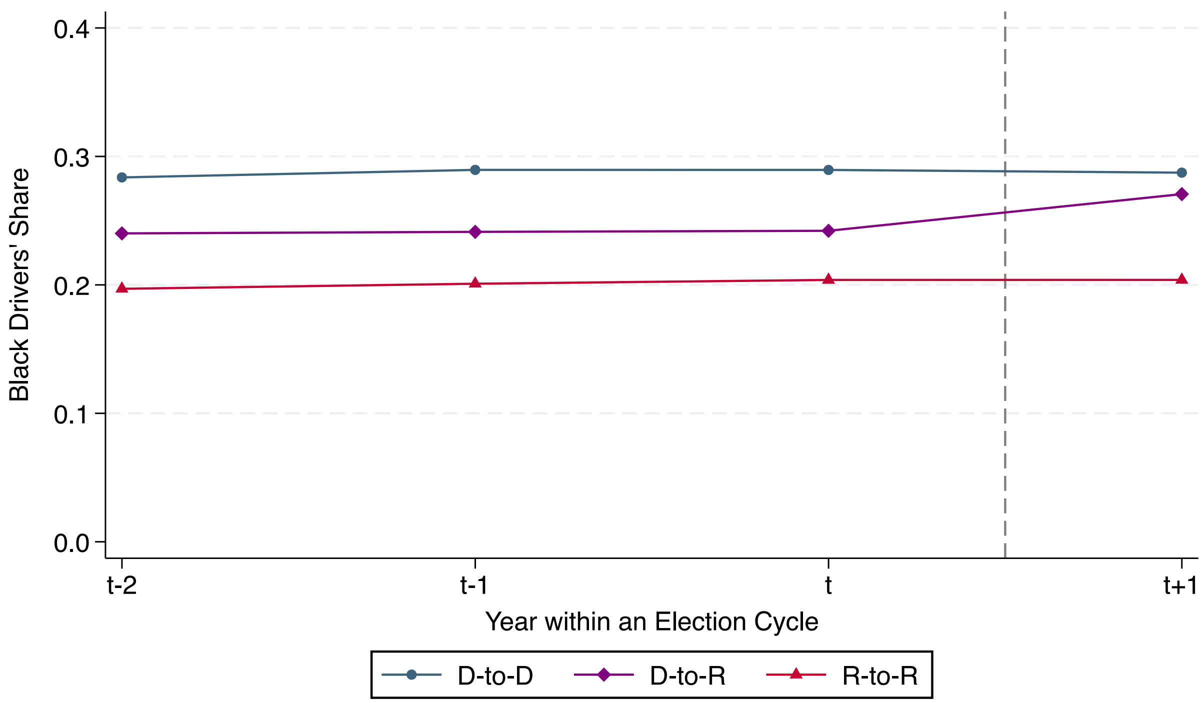

Figure 2: The Share of Black Drivers Among All Stops

Notes: This figure plots the raw data pattern. I first compute the share of Black drivers at the county-year level. I then compute the simple average of the share of Black drivers within D-to-D/D-to-R/R-to-R groups, stacking up the three election cycles. Each dot thus contains samples from three years.

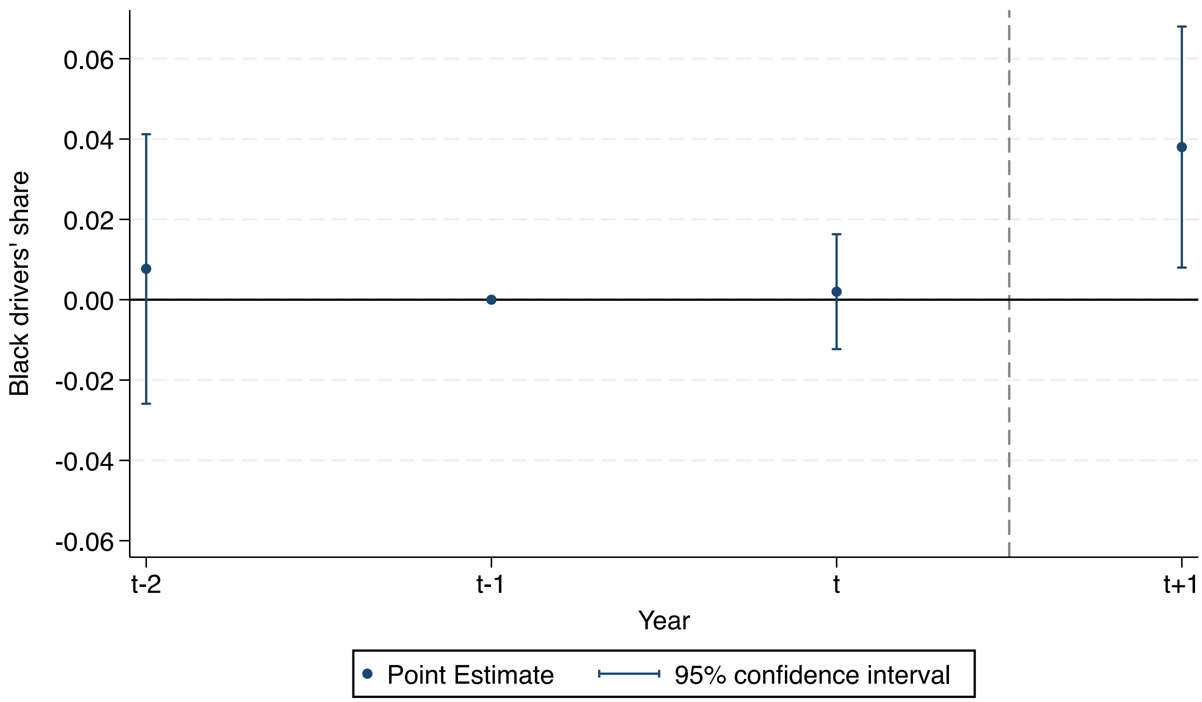

Figure 3: Impact of Sheriff Party Affiliation on the Share of Black Drivers

Notes: This figure plots the point estimate and 95% confidence intervals of β_e^* in equation 2, which are average treatment effect on the treated estimates of the impact of a D-to-R sheriff turnover (compared to a D-to-D sheriff transition) with the Black drivers' share as the outcome variable. t denotes the year when the election happened.

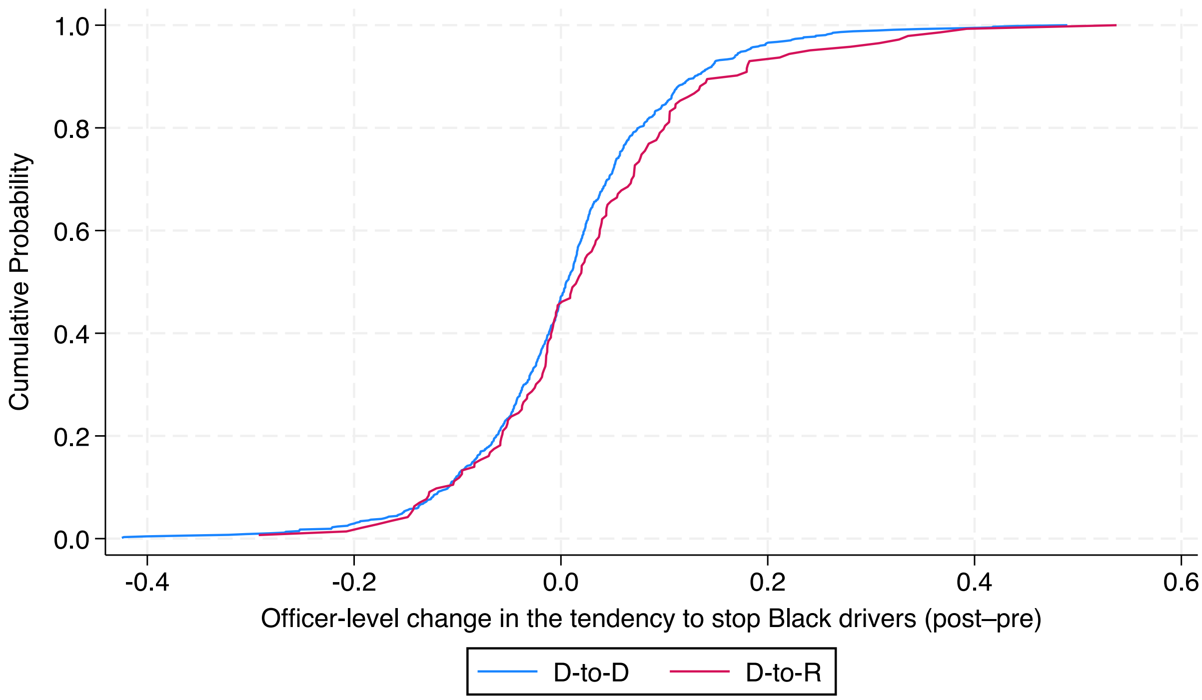

Figure 4: Cumulative Distributions of the Differences in the Tendency of Stopping Black Drivers before and after Elections among Stayers

Notes: This figure plots two cumulative distribution functions of the difference in the tendency to stop Black drivers before and after elections at the officer level, one for the stayer officers in the D-to-D counties and one for the stayer officers in the D-to-R counties. The tendency to stop Black drivers is derived from two steps. First, I regress Black stop (1 if the stopped driver is Black, 0 otherwise) on stop location and stop time fixed effects, and obtain the residuals. Stop locations are the finest geography level recorded for the stop. They can be counties, cities, census-designated places (CDPs), or intersections. I divide a day into four time periods by three time points: 6 am, 12 pm, and 6 pm. Stop time is quarter (four quarters in a year) $\times$ time period. Second, I calculate the average residual for each officer, both before and after the elections. I restrict the sample to officers with ≥ 19 pre-election stops and ≥ 9 post-election stops. These thresholds correspond to the median stop counts of officers in the pre- and post-election periods, respectively. Officers meeting this criterion account for 90–98% of all stops across D-to-D and D-to-R pre- and post-election cycles.

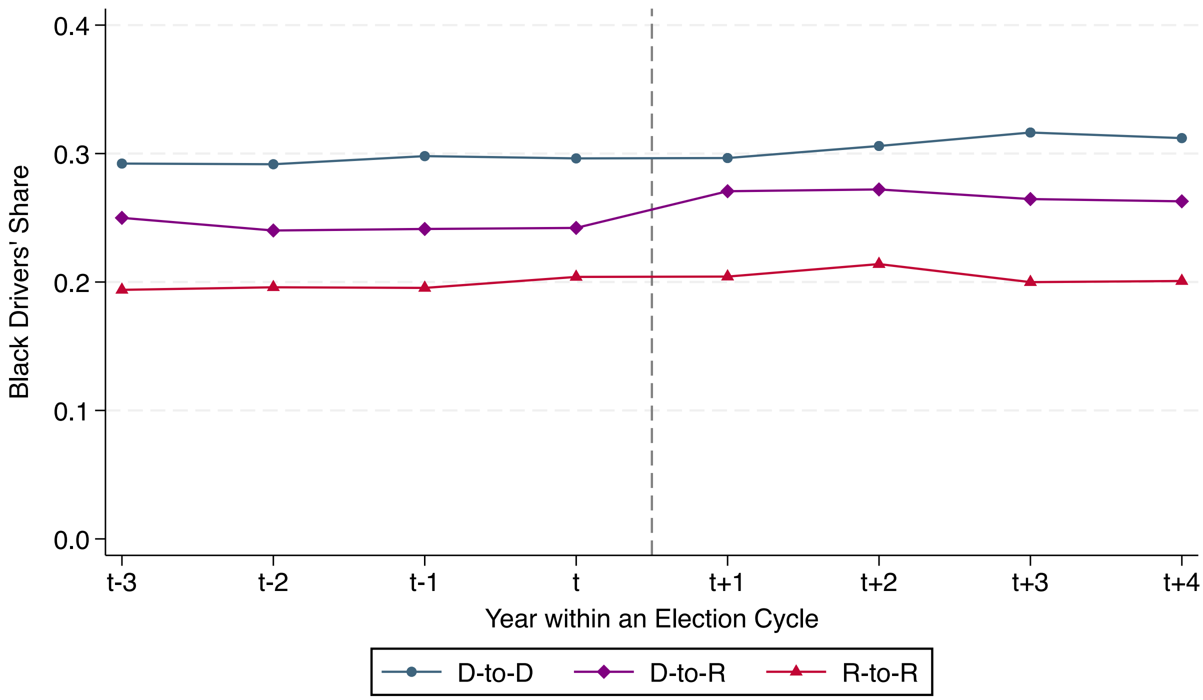

Figure 5: The Share of Black Drivers Among All Stops in Long Election Cycles

Notes: This figure plots the raw data pattern. I first compute the Black driver's share at county-year level. I then compute the simple average of the Black driver's share within D-to-D/D-to-R/R-to-R groups, stacking up the three election cycles. Each election cycle is eight years, four years before and after the elections. Each dot contains samples from three years. The number of county-cycles is less than the one in Figure 2 because we require the number of stops to be more than 50 in each county for a longer election cycle.

Tables

Table 1: Sheriff Election Results in North Carolina

Year of Election	R to R Turnover	R to R No Turnover	R to D	D to D Turnover	D to D No Turnover	D to R
Panel A: All sheriffs' offices						
2010	8	25	1	14	46	6
2014	5	33	1	14	37	10
2018	13	32	3	15	28	9
Panel B: Offices with winners' vote share < 80%						
2010	7	17	1	12	26	6
2014	3	16	1	8	21	10
2018	5	12	3	6	8	8
Panel C: Offices with winners' vote share < 80% and number of stops > 0 every year						
2010	3	14	0	8	15	4
2014	3	13	0	7	16	8
2018	3	9	3	4	5	5
Panel D: Offices with winners' vote share < 80% and number of stops > 50 every year						
2010	3	14	0	4	14	4
2014	3	12	0	6	15	6
2018	3	7	3	4	4	5
Panel E: Winners' vote share distribution in all D-to-D and D-to-R elections						
	2010		2014		2018	
Winner's vote share	D-to-D	D-to-R	D-to-D	D-to -R	D-to-D	D-to-R
<=0.6	12	4	11	8	5	7
0.6 – 0.7	15	1	8	1	7	0
0.7 – 0.8	11	1	10	1	2	1
>= 0.8& < 1	4	0	4	0	6	0
1	18	0	18	0	23	1

Notes: D refers to the Democratic Party and R to the Republican Party. North Carolina has 100 sheriff's offices, one per county. Panel A reports party turnover outcomes in all elections from 2010 to 2018. Panel B restricts to elections where the winner's vote share is below 80%, to match the competitiveness of D-to-R elections. Panel C further drops elections with at least one year of zero reported traffic stops in the four-year cycle (three years before and one year after the election). Panel D additionally excludes counties with fewer than 50 reported stops in any year of the cycle; this is the main analysis sample. Panel E shows the winners' vote share distribution in all D-to-D (turnover and no-turnover) and D-to-R elections. A vote share of one indicates an uncontested election.

Table 2: Summary Statistics of County and Sheriff Characteristics

	R to R	R to D	D to D	D to R
Panel A: County characteristics				
Urban Category				
Large Metro	7	1	8	2
Small and Medium Metro	17	2	17	4
Nonmetropolitan	18	0	22	9
Pop. Char. (share)				
Black	0.20	0.25	0.25	0.18
College	0.40	0.47	0.40	0.36
Poor (household)	0.13	0.12	0.15	0.15
Panel B: Sheriff characteristics				
Gender				
Female to Female	0	0	2	0
Female to Male	0	0	0	0
Male to Male	42	3	44	15
Male to Female	0	0	1	0
Race				
Black to Black	0	0	8	0
Black to White	0	0	0	2
White to White	42	0	34	13
White to Black	0	3	5	0
# of county-cycles	42	3	47	15

Notes: This table presents summary statistics for all county-cycles in Panel D, Table 1. Urban categories are from the National Center for Health Statistics 2013 census-based urban-rural classification scheme. Large metro includes both "central" and "fringe" counties of MSAs with a population of 1 million or more. Small and medium metros include counties with MSAs of 50,000 to 999,999 population. Nonmetropolitan includes the other counties. The population characteristics of the counties are population-weighted averages derived from county-level data from 2010, 2014, and 2018 American Community Surveys accessed via NHGIS. The row "College" reports the share of people with at least some college education. The row "Poor" reports the share of households whose income in the past 12 months is below the poverty level determined by the U.S. Census Bureau. The poverty level considers the household size, the number of people in the household who are children, and the age of the householder (under/over age 65).

Table 3: Summary Statistics of Traffic Stops and Searches

	Stops by Motorists' Group				Stops by Types		All
	Black	Hispanic	Other races	White	Safety	Investigatory	
Share Black	1.000	0.000	0.000	0.000	0.239	0.280	0.259
Share Hispanic	0.000	1.000	0.000	0.000	0.068	0.070	0.069
Share Other races	0.000	0.000	1.000	0.000	0.025	0.018	0.022
Share White	0.000	0.000	0.000	1.000	0.667	0.632	0.650
Share Female	0.362	0.240	0.318	0.359	0.357	0.344	0.351
Share Safety Stops	0.478	0.510	0.599	0.530	1.000	0.000	0.517
Share Investigatory Stops	0.522	0.490	0.401	0.470	0.000	1.000	0.483
Search Rate	0.079	0.086	0.055	0.061	0.051	0.084	0.067
Unconditional Hit Rate	0.024	0.016	0.016	0.021	0.016	0.027	0.022
Observations	85,607	22,764	7,219	214,956	170,814	159,732	330,546

Notes: This table presents summary statistics of stops in all D-to-D and D-to-R county-cycles included in Panel D in Table 1. All stops can be categorized into safety or investigatory stops. Safety stops include stops due to Speed Limit Violation, Stop Light/Sign Violation, Driving While Impaired, and Safe Movement Violation. Investigatory stops include stops due to Vehicle Equipment Violation, Vehicle Regulatory Violation, Seat Belt Violation, Investigation, and Other Motor Vehicle Violation. The traffic stop records have one race variable and one ethnicity variable. Ethnicity can be Hispanic or non-Hispanic. I define Hispanic drivers as those whose ethnicity is recorded as Hispanic, regardless of race. Accordingly, Black (White) drivers are Black (White) non-Hispanic drivers. Other races include Asians, Native Americans, and Other/Unknown. The search rate is defined as the number of stops with searches divided by the total number of stops. The unconditional hit rate is defined as the number of stops with found contraband divided by the total number of stops.

Table 4: Impact of Sheriff Party Affiliation on Black Drivers' Share: Regression Estimates and a Placebo Test

	$\frac{\text{\# of Black stops}}{\text{\# of all stops}}$				
	Sheriff's offices				Police departments
	(1)	(2)	(3)	(4)	(5)
t-2 x D-to-R	0.0077	0.0114	-0.0243	0.0080	-0.0104
	(0.0166)	(0.0126)	(0.0254)	(0.0210)	(0.0102)
	[0.6467]	[0.3691]	[0.3508]	[0.7073]	[0.3165]
t x D-to-R	0.0020	0.0020	-0.0043	-0.0004	-0.0028
	(0.0071)	(0.0068)	(0.0109)	(0.0077)	(0.0107)
	[0.7776]	[0.7640]	[0.6992]	[0.9560]	[0.7927]
t+1 x D-to-R	0.0380	0.0310	0.0314	0.0399	0.0029
	(0.0149)	(0.0145)	(0.0225)	(0.0166)	(0.0104)
	[0.0144]	[0.0393]	[0.1766]	[0.0234]	[0.7850]
Dep. mean	0.2413	0.1972	0.2425	0.2288	0.2706
# of control	47	47	8	30	30
# of treatment	15	15	9	12	12
# of cluster	42	42	22	30	30
N	248	248	104	168	168
Sample	All	All	Close election	Police Dept. with good data	
County-cycle fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Weight	Agency	# of stops	Agency	Agency	Agency

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. This table reports the ATT estimates of D-to-R sheriff turnovers relative to D-to-D transitions (β_e^* in equation 2). Columns 1-4: sheriff's office stops. Column 5: police department stops. Columns 1-2: all county-cycles in Panel D, Table 1. Column 2 weights the observations by the average number of stops in pre-election years $t-2$ and $t-1$. Column 3 includes elections where the winner's vote share is less than 60%. Column 4 restricts to stops conducted by deputy sheriffs in county-cycles where at least one police department records more than 50 stops in every year of the election cycle. Column 5 limits the sample to stops from those police departments only. All regressions include county-cycle and calendar-year fixed effects. Dep. means are computed from D-to-R counties in year $t-1$. The number of controls and treatments is in the unit of county-cycles. The number of clusters is the number of unique counties, i.e., the number of groups in the estimation of cluster-robust s.e..

Table 5: Impact of Sheriff Party Affiliation on the Number of Stops by Race

	# of stops		Natural log of number of stops		
	Black	Non-Black	Black	Non-Black	Both Race
	(1)	(2)	(3)	(4)	(5)
t-2 x D-to-R	-11.6052	63.4390	-0.2217	-0.1998	-0.1998
	(60.4546)	(158.5531)	(0.1653)	(0.1366)	(0.1478)
	[0.8487]	[0.6912]	[0.1873]	[0.1512]	[0.1839]
t x D-to-R	-69.2479	-281.7628	-0.3238	-0.3977	-0.3977
	(44.0377)	(111.1032)	(0.1959)	(0.1917)	(0.2074)
	[0.1235]	[0.0151]	[0.1059]	[0.0443]	[0.0621]
t+1 x D-to-R	237.2769	302.9636	0.2628	0.0714	0.0714
	(274.3942)	(474.6905)	(0.3200)	(0.3061)	(0.3312)
	[0.3922]	[0.5269]	[0.4163]	[0.8166]	[0.8303]
t-2 x D-to-R x Black					-0.0219
					(0.1005)
					[0.8286]
t x D-to-R x Black					0.0739
					(0.0703)
					[0.2995]
t+1 x D-to-R x Black					0.1913
					(0.1027)
					[0.0697]
Average # of stops	269	1,042			
N	248	248	248	248	496
County-Cycle	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. The outcome variables in Columns 1-5 are, respectively, the number of Black stops, the number of non-Black stops, the natural log of the number of Black stops, the natural log of the number of non-Black stops, and the natural log of the number of stops in the group (Black or non-Black). Columns 1-4 report β_e^* in equation 2. Column 5 report $\gamma_e^{0*}, \gamma_e^{1*}$ in equation 4. $\beta_1^*, \gamma_1^{0*}, \gamma_1^{1*}$ are the estimates of the average treatment effect on the treated of D-to-R sheriff turnovers relative to D-to-D transitions. All regression specifications include county-cycle and calendar-year fixed effects. The average number of stops is computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Table 6: Heterogeneity in Changes in the Black Drivers' Share Across Stop Purpose

Panel A: Decomposition of the changes in the share of Black drivers

	$\frac{\text{All Black Stops}}{\text{All Stops}}$	$\frac{\text{All Safety Stops}}{\text{All Stops}}$	$\Delta S_{i,(-1,t)}(B_{1i,-1} - B_{2i,-1})$ Changes in the share of safety stops	$S_{i,-1}\Delta B_{1i,(-1,t)}$ Changes within safety stops	$(1 - S_{i,-1})\Delta B_{2i,(-1,t)}$ Changes within investigation stops	$\Delta S_{i,(-1,t)}(\Delta B_{1i,(-1,t)} - \Delta B_{2i,(-1,t)})$ Second order changes
	(1)	(2)	(3)	(4)	(5)	(6)
t-2 x D-to-R	0.0077	-0.0113	-0.0026	0.0060	0.0037	0.0005
	(0.0166)	(0.0280)	(0.0028)	(0.0074)	(0.0087)	(0.0013)
	[0.6467]	[0.6893]	[0.3561]	[0.4203]	[0.6698]	[0.7089]
t x D-to-R	0.0020	-0.0266	0.0026	0.0041	-0.0039	-0.0007
	(0.0071)	(0.0194)	(0.0014)	(0.0051)	(0.0055)	(0.0016)
	[0.7776]	[0.1781]	[0.0670]	[0.4280]	[0.4856]	[0.6375]
t+1 x D-to-R	0.0380	-0.0917	0.0048	0.0240	0.0104	-0.0012
	(0.0149)	(0.0238)	(0.0022)	(0.0099)	(0.0104)	(0.0030)
	[0.0144]	[0.0004]	[0.0336]	[0.0200]	[0.3238]	[0.6852]
Dep. mean	0.2413	0.5281	0	0	0	0
N	248	248	186	186	186	186
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Changes in the share of Black drivers within stop type

	$\frac{\text{Black Stops within Safety Stops}}{\text{All Safety Stops}}$		$\frac{\text{Black Stops within Investigatory Stops}}{\text{All Investigatory Stops}}$	
t-2 x D-to-R	0.0141	0.0139	0.0063	0.0077
	(0.0138)	(0.0124)	(0.0173)	(0.0141)
	[0.3110]	[0.2664]	[0.7178]	[0.5886]
t x D-to-R	0.0123	0.0057	-0.0023	-0.0002
	(0.0095)	(0.0081)	(0.0109)	(0.0096)
	[0.2001]	[0.4847]	[0.8326]	[0.9831]
t+1 x D-to-R	0.0472	0.0413	0.0223	0.0181
	(0.0181)	(0.0187)	(0.0208)	(0.0202)
	[0.0128]	[0.0325]	[0.2905]	[0.3766]
Dep. mean	0.2198	0.1847	0.2640	0.2108
N	248	248	248	248
Weight	Agency	# of safety stops	Agency	# of investigatory stops
County-Cycle	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. Safety stops are stops due to moving violations, while investigatory stops are those due to non-moving violations. Panel A reports the decomposition of changes in the share of Black drivers across two stop purposes. Columns 1-2 in Panel A report ATT estimates (β_e^*) from an OLS regression with specification as in equation 1, and aggregated as in equation 2. Column 3-6 in Panel A reports ATT estimates from an OLS regression with specification as in equation 5 in the Appendix, and aggregated as in equation 2. Results in Columns 3-6 are the decomposition of the results in Column 1. Summing up coefficients from Columns 3-6 would equal the coefficient in Column 1. I denote B_{1it} and B_{2it} as the share of Black drivers in safety and investigation stops for county i in year t. There are four time periods, $t = -2, -1, 0, 1$. I set $t = -1$ as the baseline period. S_{it} $(1 - S_{it})$ is the share of safety (investigation) stops of all stops. I denote $\Delta S_{i,(-1,t}$ as the difference of the share of safety stops for county i between period -1 and t. Column 3 represents the contribution to the changes in the share of Black drivers from changes in the share of safety stops of all stops (while keeping the Black drivers' share within each type of stop constant). Columns 4 and 5 represent the contribution from changes in the share of Black drivers *within* safety and investigation stops. Column 6 is the leftover second-order changes (contribution from deviation from both the share of safety stops and Black drivers' share in safety and investigation stops). See the Appendix for the derivation of the decomposition. Panel B reports the ATT estimates where the outcome variables are the share of Black drivers within safety and within investigatory stops. Columns 2 and 4 weight the observations by the average number of stops in pre-election years $t - 2$ and $t - 1$. Dep. mean is computed from D-to-R counties in year $t - 1$.

Table 7: Heterogeneity in Changes in the Black Drivers' Share Across Two Types of Officers

Panel A: Decomposition of the changes in the share of Black drivers

	$\frac{\text{All Black Stops}}{\text{All Stops}}$	$\frac{\text{All Stayer Stops}}{\text{All Stops}}$	$\Delta S_{i,(-1,t)}(B_{1i,-1} - B_{2i,-1})$ Changes in stayer share	$S_{i,-1}\,\Delta B_{1i,(-1,t)}$ Changes within stayers	$(1 - S_{i,-1})\,\Delta B_{2i,(-1,t)}$ Changes within non-stayers	$\Delta S_{i,(-1,t)}(\Delta B_{1i,(-1,t)} - \Delta B_{2i,(-1,t)})$ Second-order changes
	(1)	(2)	(3)	(4)	(5)	(6)
$t-2 \times$ D-to-R	0.0077	-0.0326	-0.0014	0.0209	-0.0057	-0.0062
	(0.0166)	(0.0335)	(0.0031)	(0.0130)	(0.0080)	(0.0059)
	[0.6467]	[0.3358]	[0.6567]	[0.1146]	[0.4831]	[0.3001]
$t \times$ D-to-R	0.0020	0.0054	0.0002	0.0038	-0.0084	0.0065
	(0.0071)	(0.0711)	(0.0084)	(0.0052)	(0.0087)	(0.0128)
	[0.7776]	[0.9399]	[0.9852]	[0.4649]	[0.3392]	[0.6148]
$t+1 \times$ D-to-R	0.0380	-0.1874	0.0093	0.0267	0.0167	-0.0147
	(0.0149)	(0.0788)	(0.0094)	(0.0139)	(0.0102)	(0.0103)
	[0.0144]	[0.0222]	[0.3291]	[0.0628]	[0.1093]	[0.1623]
Dep. mean	0.2413	0.5520	0	0	0	0
N	248	248	186	186	186	186
County-Cycle FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Behavior changes of stayers and non-stayers

	$\frac{\text{Black Stops by Stayers}}{\text{All Stops by Stayers}}$		$\frac{\text{Black Stops by Non-Stayers}}{\text{All Stops by Non-Stayers}}$	
	(1)	(2)	(3)	(4)
$t-2 \times$ D-to-R	0.0525	0.0253	-0.0344	-0.0030
	(0.0308)	(0.0150)	(0.0360)	(0.0158)
	[0.0959]	[0.0981]	[0.3452]	[0.8481]
$t \times$ D-to-R	0.0104	-0.0001	-0.0310	-0.0076
	(0.0107)	(0.0081)	(0.0287)	(0.0123)
	[0.3383]	[0.9891]	[0.2851]	[0.5421]
$t+1 \times$ D-to-R	0.0419	0.0372	0.0041	0.0420
	(0.0188)	(0.0163)	(0.0299)	(0.0192)
	[0.0315]	[0.0276]	[0.8921]	[0.0348]
Dep. mean	0.2294	0.1975	0.2661	0.1924
N	248	248	248	248
Weight	Agency	# of stops by stayers	Agency	# of stops by non-stayers
County-Cycle FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Panel C: Changes in the share of non-stayers and new officers

	$\frac{\text{\# of non-stayers}}{\text{\# of all officers}}$		$\frac{\text{\# of new officers}}{\text{\# of all officers}}$	
	(1)	(2)	(3)	(4)
t-2 x D-to-R	-0.0025	0.0151	-0.0120	0.0253
	(0.0313)	(0.0255)	(0.0483)	(0.0440)
	[0.9354]	[0.5569]	[0.8043]	[0.5683]
t x D-to-R	0.0335	-0.0284	0.0303	0.0319
	(0.0418)	(0.0445)	(0.0546)	(0.0612)
	[0.4274]	[0.5262]	[0.5826]	[0.6049]
t+1 x D-to-R	0.1696	0.1536	0.1889	0.1809
	(0.0574)	(0.0720)	(0.0641)	(0.0502)
	[0.0052]	[0.0389]	[0.0053]	[0.0008]
Dep. mean	0.6125	0.6164	0.3658	0.3812
N	248	248	248	248
Weight	Agency	# of officers	Agency	# of officers
County-Cycle	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. Stayers are officers who conducted traffic stops both before and after elections. Non-stayers are those who conducted traffic stops only before or after the elections. New officers are those whose first traffic stop in that agency during the whole sample period (2007-2019) is recorded in that year. Panel A reports the decomposition of changes in the share of Black drivers across two types of officers. Columns 1-2 report ATT estimates (β_e^*) from an OLS regression with specification as in equation 1, and aggregated as in equation 2. Column 3-6 report ATT estimates from an OLS regression with specification as in equation 5 in the Appendix, and aggregated as in equation 2. The estimation results in Columns 3-6 are the decomposition of the results in Column 1. Adding up coefficients from Columns 3-6 would equal the coefficient in Column 1. I denote B_{1it} and B_{2it} as the share of Black drivers of all stops done by stayers and non-stayers, respectively, for county i in year t. There are four time periods, $t = -2, -1, 0, 1$. I set $t = -1$ as the baseline period. I denote S_{it} as the share of stops done by stayers. Then $1 - S_{it}$ is the share of stops done by non-stayers. I denote $\Delta S_{i,(-1,t}$ as the difference of the shares of stops done by stayers in county i between period -1 and t. Column 3 represents the contribution to the changes in the Black driver's share from changes in the share of stops done by stayers. Columns 4 and 5 represent the contribution from changes in the share of Black drivers within stops done by stayers and non-stayers. Column 6 is the leftover second-order changes. See the Appendix for the derivation of the decomposition. Panel B reports the ATT estimates where the outcome variables are the share of Black drivers within stops conducted by stayers and non-stayers. Panel C reports the ATT estimates where the outcome variables are the share of non-stayers and new officers among all officers. Columns 2 and 4 in Panels B and C weight the observations by the average number of stops or officers in pre-election years $t-2$ and $t-1$. Dep. mean is computed from D-to-R counties in year $t-1$.

Table 8: Patrol Location and Time Policy

	$\frac{\text{Predicted Black stops}}{\text{Stops}}$					
	All		Safety Stops		Investigatory Stops	
	Location	Time	Location	Time	Location	Time
	(1)	(2)	(3)	(4)	(5)	(6)
t-2 x D-to-R	0.0053	0.0023	0.0035	0.0004	0.0072	0.0079
	(0.0045)	(0.0038)	(0.0032)	(0.0036)	(0.0056)	(0.0041)
	[0.2453]	[0.5466]	[0.2813]	[0.9134]	[0.2060]	[0.0610]
t x D-to-R	0.0019	-0.0054	0.0025	-0.0043	0.0009	-0.0057
	(0.0033)	(0.0036)	(0.0036)	(0.0042)	(0.0038)	(0.0041)
	[0.5719]	[0.1462]	[0.4872]	[0.3046]	[0.8212]	[0.1682]
t+1 x D-to-R	0.0037	-0.0031	0.0055	-0.0030	0.0011	-0.0033
	(0.0044)	(0.0035)	(0.0048)	(0.0039)	(0.0046)	(0.0036)
	[0.4076]	[0.3850]	[0.2631]	[0.4488]	[0.8092]	[0.3661]
Dep. mean	0.2428	0.2403	0.2410	0.2376	0.2450	0.2443
N	248	248	248	248	248	248
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. All estimates are ATT estimates (β_e^* in equation 2) from an OLS regression with specification as in equation 1, and aggregated as in equation 2. All regression specifications include county-cycle and calendar-year fixed effects. For Columns 1, 3, and 5, I predict whether the stop is associated with a Black driver (Black stop) based on the share of Black stops in each location cell before (including) the election year. For Columns 2, 4, and 6, I predict whether the stop is a Black stop based on the share of Black stops before (including) the election year in each time group x county cell. A day is divided into four time groups by four points: 6 am, noon, 6 pm, midnight. Dep. mean is computed from D-to-R counties in year $t-1$.

Table 9: Impact of Sheriff Party Affiliation on Search Rates

	All searches All stops		Black searches Black stops		Non-Black searches Non-Black stops		Black - Non-Black Search rate diff.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Observations unweighted								
t-2 x D-to-R	0.0085	0.0065	-0.0124	-0.0129	0.0145	0.0125	-0.0269	-0.0254
	(0.0113)	(0.0117)	(0.0170)	(0.0182)	(0.0113)	(0.0117)	(0.0145)	(0.0158)
	[0.4553]	[0.5803]	[0.4693]	[0.4843]	[0.2075]	[0.2902]	[0.0713]	[0.1153]
t x D-to-R	-0.0043	-0.0047	-0.0252	-0.0252	0.0020	0.0019	-0.0272	-0.0271
	(0.0087)	(0.0091)	(0.0237)	(0.0254)	(0.0096)	(0.0101)	(0.0260)	(0.0277)
	[0.6283]	[0.6130]	[0.2939]	[0.3284]	[0.8343]	[0.8491]	[0.3003]	[0.3331]
t+1 x D-to-R	0.0130	0.0057	0.0319	0.0214	0.0101	0.0035	0.0218	0.0180
	(0.0159)	(0.0150)	(0.0231)	(0.0193)	(0.0167)	(0.0168)	(0.0211)	(0.0231)
	[0.4160]	[0.7083]	[0.1744]	[0.2740]	[0.5479]	[0.8377]	[0.3084]	[0.4410]
Dep. mean	0.0832	0.0863	0.1102	0.1138	0.0768	0.0796		
Baseline Diff.							0.0334	0.0342
							(0.0104)	(0.0111)
							[0.0027]	[0.0038]
Panel B: Observations weighted by number of stops								
t-2 x D-to-R	0.0033	0.0025	-0.0028	-0.0018	0.0068	0.0059	-0.0096	-0.0077
	(0.0065)	(0.0069)	(0.0127)	(0.0136)	(0.0059)	(0.0061)	(0.0106)	(0.0108)
	[0.6105]	[0.7194]	[0.8273]	[0.8934]	[0.2515]	[0.3413]	[0.3695]	[0.4805]
t x D-to-R	0.0003	-0.0004	-0.0037	-0.0016	0.0000	-0.0012	-0.0037	-0.0005
	(0.0067)	(0.0073)	(0.0123)	(0.0130)	(0.0060)	(0.0065)	(0.0119)	(0.0126)
	[0.9630]	[0.9521]	[0.7639]	[0.8997]	[0.9981]	[0.8587]	[0.7553]	[0.9698]
t+1 x D-to-R	0.0151	0.0055	0.0323	0.0147	0.0090	0.0005	0.0233	0.0142
	(0.0106)	(0.0075)	(0.0189)	(0.0086)	(0.0099)	(0.0081)	(0.0134)	(0.0094)
	[0.1618]	[0.4651]	[0.0956]	[0.0935]	[0.3685]	[0.9538]	[0.0898]	[0.1379]
Dep. mean	0.0633	0.0672	0.0769	0.0781	0.0599	0.0642		
Baseline Diff.							0.0224	0.0234
							(0.0078)	(0.0089)
							[0.0064]	[0.0125]
N	248	240	248	240	248	240	496	480
Sample	All	No outlier	All	No outlier	All	No outlier	All	No outlier
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. Column 1-6 reports β_e^* in equation 2. Columns 7-8 report γ_e^{1*} in equation 4. All reported estimates represent the average treatment effect on the treated of the impact of a D-to-R sheriff turnover compared to a D-to-D transition. Baseline Diff. is the estimated search rate difference across racial groups (Black − Non-Black) in D-to-R counties in $t-1$, derived from the same regression in that column. In panel B, county-year observations are weighted by the average number of stops, the number of Black stops, and the number of Non-Black stops in the county in $t-2$ and $t-1$. Columns 2, 4, and 6 report estimates from samples that exclude two outliers, Lincoln County in the 2010 cycle (D-to-R) and Jackson County in the 2014 cycle (D-to-D). The two counties experience the largest change in search rates of Black drivers between $t-2$ and $t+1$, 0.23 for Lincoln County and 0.12 for Polk County. Dep. means are computed from D-to-R counties in year $t-1$. The Dep. mean is weighted by the number of stops in Panel B. All regression specifications include county-cycle and calendar-year fixed effects.

Table 10: Impact of Sheriff Party Affiliation on Search Rates by Stop Type and Officer Type

	All searches		Black searches		Non-Black searches		Black - Non-Black	
	All stops		Black stops		Non-Black stops		Search rate diff.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Safety stops								
t-2 x D-to-R	0.0017	0.0013	0.0196	0.0231	-0.0041	-0.0051	0.0236	0.0282
	(0.0086)	(0.0099)	(0.0209)	(0.0226)	(0.0071)	(0.0081)	(0.0170)	(0.0177)
	[0.8480]	[0.8947]	[0.3549]	[0.3129]	[0.5677]	[0.5271]	[0.1712]	[0.1189]
t x D-to-R	-0.0031	-0.0044	-0.0080	-0.0068	-0.0047	-0.0064	-0.0034	-0.0004
	(0.0059)	(0.0064)	(0.0122)	(0.0130)	(0.0052)	(0.0056)	(0.0114)	(0.0120)
	[0.6054]	[0.4979]	[0.5153]	[0.6044]	[0.3711]	[0.2558]	[0.7687]	[0.9735]
t+1 x D-to-R	0.0220	0.0133	0.0521	0.0323	0.0117	0.0033	0.0404	0.0290
	(0.0135)	(0.0116)	(0.0244)	(0.0163)	(0.0122)	(0.0107)	(0.0173)	(0.0138)
	[0.1110]	[0.2588]	[0.0390]	[0.0553]	[0.3406]	[0.7632]	[0.0250]	[0.0424]
Dep. mean	0.0448	0.0487	0.0501	0.0505	0.0437	0.0484		
Baseline Diff.							0.0094	0.0085
							(0.0050)	(0.0058)
							[0.0704]	[0.1551]
Panel B: Investigation stops								
t-2 x D-to-R	0.0015	0.0013	-0.0247	-0.0237	0.0150	0.0151	-0.0397	-0.0388
	(0.0065)	(0.0067)	(0.0132)	(0.0138)	(0.0072)	(0.0074)	(0.0130)	(0.0130)
	[0.8138]	[0.8423]	[0.0696]	[0.0938]	[0.0447]	[0.0485]	[0.0041]	[0.0051]
t x D-to-R	0.0038	0.0041	-0.0016	0.0013	0.0063	0.0062	-0.0080	-0.0048
	(0.0093)	(0.0102)	(0.0184)	(0.0186)	(0.0095)	(0.0106)	(0.0204)	(0.0214)
	[0.6835]	[0.6899]	[0.9296]	[0.9432]	[0.5103]	[0.5643]	[0.6981]	[0.8224]
t+1 x D-to-R	0.0100	0.0014	0.0138	-0.0006	0.0093	0.0021	0.0044	-0.0027
	(0.0110)	(0.0093)	(0.0194)	(0.0143)	(0.0106)	(0.0099)	(0.0165)	(0.0154)
	[0.3683]	[0.8792]	[0.4822]	[0.9661]	[0.3820]	[0.8321]	[0.7903]	[0.8605]
Dep. mean	0.0897	0.0922	0.1085	0.1097	0.0846	0.0869		
Baseline Diff.							0.0317	0.0324
							(0.0118)	(0.0129)
							[0.0102]	[0.0170]
N	236	228	236	228	236	228	472	456
Panel C: Stayers								
t-2 x D-to-R	0.0006	0.0003	0.0011	0.0011	0.0021	0.0017	-0.0011	-0.0006
	(0.0095)	(0.0095)	(0.0131)	(0.0131)	(0.0098)	(0.0098)	(0.0123)	(0.0124)
	[0.9469]	[0.9753]	[0.9342]	[0.9350]	[0.8283]	[0.8624]	[0.9326]	[0.9597]
t x D-to-R	-0.0012	-0.0012	0.0017	0.0017	-0.0051	-0.0051	0.0068	0.0068
	(0.0067)	(0.0067)	(0.0093)	(0.0093)	(0.0058)	(0.0058)	(0.0089)	(0.0089)
	[0.8562]	[0.8564]	[0.8547]	[0.8552]	[0.3921]	[0.3923]	[0.4537]	[0.4544]
t+1 x D-to-R	0.0041	0.0041	0.0126	0.0128	-0.0015	-0.0017	0.0141	0.0145
	(0.0079)	(0.0080)	(0.0080)	(0.0080)	(0.0088)	(0.0089)	(0.0083)	(0.0083)
	[0.6097]	[0.6140]	[0.1268]	[0.1196]	[0.8648]	[0.8520]	[0.0983]	[0.0901]
Dep. mean	0.0659	0.0659	0.0737	0.1068	0.0641	0.0641		
Baseline Diff.							0.0121	0.0121
							(0.0082)	(0.0082)
							[0.1496]	[0.1505]
Panel D: Non-stayers								
t-2 x D-to-R	0.0064	0.0058	0.0133	0.0133	0.0081	0.0072	0.0051	0.0060
	(0.0119)	(0.0119)	(0.0158)	(0.0158)	(0.0138)	(0.0138)	(0.0211)	(0.0212)
	[0.5933]	[0.6297]	[0.4069]	[0.4083]	[0.5583]	[0.6021]	[0.8092]	[0.7779]
t x D-to-R	0.0003	0.0005	-0.0072	-0.0071	0.0027	0.0029	-0.0099	-0.0100
	(0.0191)	(0.0192)	(0.0368)	(0.0369)	(0.0166)	(0.0167)	(0.0325)	(0.0326)
	[0.9875]	[0.9806]	[0.8470]	[0.8479]	[0.8708]	[0.8635]	[0.7633]	[0.7604]
t+1 x D-to-R	0.0085	0.0096	0.0170	0.0171	0.0045	0.0061	0.0126	0.0110
	(0.0170)	(0.0172)	(0.0228)	(0.0229)	(0.0201)	(0.0204)	(0.0275)	(0.0276)
	[0.6216]	[0.5806]	[0.4608]	[0.4602]	[0.8253]	[0.7676]	[0.6508]	[0.6926]
Dep. mean	0.0517	0.0517	0.0670	0.0670	0.0464	0.0464		
Baseline Diff.							0.0278	0.0278
							(0.0194)	(0.0194)
							[0.1596]	[0.1605]
N	212	208	212	208	212	208	424	416
Sample	All	No outlier	All	No outlier	All	No outlier	All	No outlier
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. All county-year observations are weighted by the average number of stops, the number of Black stops, and the number of Non-Black stops in the corresponding type of stops in the county in $t-2$ and $t-1$. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. Column 1-6 reports β_c^* in equation 2. Columns 7-8 report γ_c^{1*} in equation 4. All reported estimates represent the average treatment effect on the treated of the impact of a D-to-R sheriff turnover (treatment group) compared to a D-to-D transition (control group). Baseline Diff. is the estimated search rate difference across racial groups (Black − Non-Black) in D-to-R counties in $t-1$, derived from the same regression in that column. Panels A, B, C, and D report the estimates for samples that only include safety stops, investigation stops, stops conducted by stayers, and stops conducted by non-stayers. To maintain a balanced panel structure in each Panel, I exclude county-cycles where at least one county-year has zero Black or Non-Black stops among the four types of stops. This criterion excludes 3 county-cycles in Panels A and B; 9 county-cycles in Panels C and D. Columns 2, 4, 6, and 8 report estimates from samples that exclude two outliers, Lincoln County in the 2010 cycle (D-to-R) and Jackson County in the 2014 cycle (D-to-D). The two counties experience the largest change in search rates of Black drivers between $t-2$ and $t+1$, 0.23 for Lincoln County and 0.12 for Polk County. Dep. means are computed from D-to-R counties in year $t-1$. The Dep. mean is weighted by the number of stops. All regression specifications include county-cycle and calendar-year fixed effects.

Table 11: Impact of Sheriff Party Affiliation on Unconditional Hit Rates

	All contraband All stops		Black contraband Black stops		Non-Black contraband Non-Black stops		Black - Non-Black Uncon. hit rate diff.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Observations unweighted								
t-2 x D-to-R	0.0095	0.0097	0.0179	0.0200	0.0095	0.0095	0.0084	0.0105
	(0.0059)	(0.0063)	(0.0098)	(0.0106)	(0.0058)	(0.0062)	(0.0098)	(0.0106)
	[0.1141]	[0.1359]	[0.0764]	[0.0657]	[0.1102]	[0.1370]	[0.3960]	[0.3286]
t x D-to-R	-0.0038	-0.0042	0.0080	0.0083	-0.0021	-0.0024	0.0102	0.0107
	(0.0056)	(0.0060)	(0.0144)	(0.0154)	(0.0068)	(0.0073)	(0.0180)	(0.0192)
	[0.4975]	[0.4851]	[0.5786]	[0.5911]	[0.7586]	[0.7466]	[0.5749]	[0.5797]
t+1 x D-to-R	0.0047	0.0043	0.0178	0.0184	0.0024	0.0016	0.0155	0.0168
	(0.0082)	(0.0091)	(0.0093)	(0.0099)	(0.0104)	(0.0114)	(0.0128)	(0.0138)
	[0.5698]	[0.6426]	[0.0629]	[0.0704]	[0.8216]	[0.8856]	[0.2335]	[0.2306]
Dep. mean	0.0832	0.0863	0.0337	0.0351	0.0296	0.0310		
Baseline Diff.							0.0041	0.0041
							(0.0067)	(0.0072)
							[0.5488]	[0.5726]
Panel B: Observations weighted by number of stops								
t-2 x D-to-R	0.0034	0.0039	0.0025	0.0034	0.0041	0.0045	-0.0016	-0.0011
	(0.0032)	(0.0033)	(0.0062)	(0.0065)	(0.0030)	(0.0031)	(0.0059)	(0.0063)
	[0.2904]	[0.2529]	[0.6884]	[0.6045]	[0.1811]	[0.1586]	[0.7840]	[0.8589]
t x D-to-R	-0.0008	-0.0013	-0.0021	-0.0029	-0.0022	-0.0027	0.0001	-0.0002
	(0.0036)	(0.0040)	(0.0060)	(0.0061)	(0.0041)	(0.0045)	(0.0070)	(0.0071)
	[0.8204]	[0.7479]	[0.7234]	[0.6389]	[0.5955]	[0.5498]	[0.9924]	[0.9828]
t+1 x D-to-R	0.0040	0.0037	0.0076	0.0070	0.0011	0.0006	0.0065	0.0064
	(0.0048)	(0.0052)	(0.0072)	(0.0075)	(0.0050)	(0.0053)	(0.0072)	(0.0074)
	[0.4081]	[0.4825]	[0.2947]	[0.3536]	[0.8189]	[0.9115]	[0.3698]	[0.3931]
Dep. mean	0.0633	0.0672	0.0264	0.0274	0.0201	0.0223		
Baseline Diff.							0.0060	0.0065
							(0.0033)	(0.0034)
							[0.0778]	[0.0601]
N	248	240	248	240	248	240	496	480
Sample	All	No outlier	All	No outlier	All	No outlier	All	No outlier
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. Columns 1-6 reports β_e^* in equation 2. Columns 7-8 report γ_e^{1*} in equation 4. All reported estimates represent the average treatment effect on the treated of the impact of a D-to-R sheriff turnover (treatment group) compared to a D-to-D transition (control group). Baseline Diff. is the estimated unconditional hit rate difference across racial groups (Black − Non-Black) in D-to-R counties in $t-1$, derived from the same regression in that column. In Panel B, county-year observations are weighted by the average number of stops, the number of Black stops, and the number of Non-Black stops of the county in $t-2$ and $t-1$. Columns 2, 4, and 6 report estimates from samples that exclude two outliers, Lincoln County in the 2010 cycle (D-to-R) and Jackson County in the 2014 cycle (D-to-D). The two counties experience the largest change in search rates of Black drivers between $t-2$ and $t+1$. Dep. means are computed from D-to-R counties in year $t-1$. The Dep. means are weighted by the number of stops in Panel B. All regression specifications include county-cycle and calendar-year fixed effects.

Table 12: Impact of Sheriff Party Affiliation on Unconditional Hit Rates by Stop Purposes and Officer Types

	All contraband All stops		Black contraband Black stops		Non-Black contraband Non-Black stops		Black - Non-Black Uncon. hit rate diff.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Safety stops								
t-2 x D-to-R	0.0040	0.0050	0.0132	0.0157	0.0014	0.0021	0.0118	0.0136
	(0.0041)	(0.0043)	(0.0108)	(0.0114)	(0.0033)	(0.0034)	(0.0104)	(0.0111)
	[0.3378]	[0.2524]	[0.2312]	[0.1792]	[0.6807]	[0.5448]	[0.2631]	[0.2284]
t x D-to-R	-0.0024	-0.0025	-0.0026	-0.0028	-0.0034	-0.0036	0.0008	0.0008
	(0.0043)	(0.0046)	(0.0076)	(0.0081)	(0.0047)	(0.0051)	(0.0082)	(0.0086)
	[0.5751]	[0.5976]	[0.7344]	[0.7355]	[0.4695]	[0.4857]	[0.9183]	[0.9268]
t+1 x D-to-R	0.0108	0.0105	0.0199	0.0159	0.0065	0.0063	0.0133	0.0096
	(0.0068)	(0.0073)	(0.0112)	(0.0113)	(0.0066)	(0.0070)	(0.0107)	(0.0108)
	[0.1215]	[0.1563]	[0.0844]	[0.1672]	[0.3261]	[0.3754]	[0.2208]	[0.3795]
Dep. mean	0.0154	0.0166	0.0158	0.0160	0.0153	0.0168		
Baseline Diff.							0.0029	0.0028
							(0.0040)	(0.0045)
							[0.4660]	[0.5380]
Panel B: Investigation stops								
t-2 x D-to-R	0.0016	0.0015	-0.0109	-0.0112	0.0069	0.0070	-0.0178	-0.0182
	(0.0044)	(0.0046)	(0.0069)	(0.0072)	(0.0046)	(0.0048)	(0.0067)	(0.0070)
	[0.7093]	[0.7472]	[0.1208]	[0.1292]	[0.1426]	[0.1570]	[0.0113]	[0.0128]
t x D-to-R	0.0007	-0.0003	-0.0061	-0.0074	0.0003	-0.0007	-0.0064	-0.0067
	(0.0044)	(0.0047)	(0.0100)	(0.0101)	(0.0051)	(0.0055)	(0.0119)	(0.0123)
	[0.8779]	[0.9419]	[0.5427]	[0.4679]	[0.9552]	[0.8955]	[0.5921]	[0.5896]
t+1 x D-to-R	-0.0013	-0.0016	-0.0013	-0.0007	-0.0027	-0.0034	0.0014	0.0027
	(0.0053)	(0.0057)	(0.0092)	(0.0098)	(0.0064)	(0.0068)	(0.0102)	(0.0107)
	[0.8076]	[0.7733]	[0.8879]	[0.9442]	[0.6751]	[0.6178]	[0.8912]	[0.8020]
Dep. mean	0.0297	0.0325	0.0390	0.0408	0.0271	0.0299		
Baseline Diff.							0.0097	0.0103
							(0.0044)	(0.0046)
							[0.0319]	[0.0309]
N	236	228	236	228	236	228	472	456
Panel C: Stayers								
t-2 x D-to-R	0.0015	0.0014	0.0039	0.0039	0.0011	0.0009	0.0028	0.0030
	(0.0057)	(0.0057)	(0.0076)	(0.0076)	(0.0061)	(0.0061)	(0.0066)	(0.0066)
	[0.7888]	[0.8120]	[0.6159]	[0.6170]	[0.8600]	[0.8877]	[0.6775]	[0.6542]
t x D-to-R	-0.0003	-0.0004	-0.0011	-0.0011	-0.0029	-0.0030	0.0018	0.0018
	(0.0032)	(0.0033)	(0.0048)	(0.0049)	(0.0042)	(0.0042)	(0.0072)	(0.0072)
	[0.9197]	[0.9142]	[0.8176]	[0.8172]	[0.4862]	[0.4832]	[0.8012]	[0.7993]
t+1 x D-to-R	0.0015	0.0014	0.0001	0.0001	-0.0000	-0.0003	0.0001	0.0003
	(0.0052)	(0.0052)	(0.0070)	(0.0070)	(0.0049)	(0.0049)	(0.0064)	(0.0064)
	[0.7709]	[0.7967]	[0.9910]	[0.9914]	[0.9948]	[0.9586]	[0.9862]	[0.9590]
Dep. mean	0.0223	0.0223	0.0250	0.0250	0.0217	0.0217		
Baseline Diff.							0.0049	0.0049
							(0.0032)	(0.0032)
							[0.1301]	[0.1309]
Panel D: Non-stayers								
t-2 x D-to-R	0.0049	0.0045	0.0115	0.0115	0.0040	0.0035	0.0075	0.0080
	(0.0085)	(0.0085)	(0.0113)	(0.0113)	(0.0101)	(0.0101)	(0.0140)	(0.0140)
	[0.5703]	[0.6023]	[0.3171]	[0.3185]	[0.6932]	[0.7337]	[0.5977]	[0.5719]
t x D-to-R	-0.0012	-0.0011	0.0034	0.0034	-0.0027	-0.0026	0.0061	0.0060
	(0.0132)	(0.0133)	(0.0151)	(0.0151)	(0.0138)	(0.0138)	(0.0119)	(0.0120)
	[0.9292]	[0.9348]	[0.8255]	[0.8255]	[0.8429]	[0.8513]	[0.6120]	[0.6207]
t+1 x D-to-R	0.0060	0.0065	0.0230	0.0230	-0.0010	-0.0003	0.0240	0.0233
	(0.0085)	(0.0085)	(0.0118)	(0.0118)	(0.0119)	(0.0119)	(0.0188)	(0.0188)
	[0.4840]	[0.4486]	[0.0591]	[0.0595]	[0.9306]	[0.9817]	[0.2088]	[0.2240]
Dep. mean	0.0220	0.0220	0.0263	0.0263	0.0205	0.0205		
Baseline Diff.							-0.0007	-0.0007
							(0.0119)	(0.0119)
							[0.9522]	[0.9523]
N	212	208	212	208	212	208	424	416
Sample	All	No outlier	All	No outlier	All	No outlier	All	No outlier
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. Columns 1-6 report β_e^* in equation 2. Columns 7-8 report γ_e^{1*} in equation 4. All reported estimates represent the average treatment effect on the treated of the impact of a D-to-R sheriff turnover (treatment group) compared to a D-to-D transition (control group). Baseline Diff. is the estimated unconditional hit rate difference across racial groups (Black − Non-Black) in D-to-R counties in $t-1$, derived from the same regression in that column. Panels A, B, C, and D report the estimates for samples that only include safety stops, investigation stops, stops conducted by stayers, and stops conducted by non-stayers. To maintain a balanced panel structure in each Panel, I exclude county-cycles where at least one county-year has zero Black or Non-Black stops among the four types of stops. This criterion excludes 3 county-cycles in Panels A and B; 9 county-cycles in Panels C and D. Columns 2, 4, 6, and 8 report estimates from samples that exclude two outliers, Lincoln County in the 2010 cycle (D-to-R) and Jackson County in the 2014 cycle (D-to-D). The two counties experience the largest change in search rates of Black drivers between $t-2$ and $t+1$, 0.23 for Lincoln County and 0.12 for Polk County. Dep. means are computed from D-to-R counties in year $t-1$. The Dep. mean is weighted by the number of stops in Panel B. All regression specifications include county-cycle and calendar-year fixed effects.

Table 13: Impact of Sheriff Party Affiliation on Motor Vehicle Accidents

	# of accidents	# of people injured or killed	Share of accidents involving Black drivers	Share of accidents involving speeding	Share of accidents resulting in injury or fatality
	(1)	(2)	(3)	(4)	(5)
t-2 x D-to-R	-7.8782	0.6235	-0.0200	0.0610	0.0503
	(5.1867)	(1.3576)	(0.0489)	(0.0686)	(0.0370)
	[0.1365]	[0.6485]	[0.6850]	[0.3788]	[0.1815]
t x D-to-R	3.4025	-0.3208	-0.0369	0.0886	0.0191
	(6.2696)	(1.1868)	(0.0675)	(0.0560)	(0.0258)
	[0.5903]	[0.7883]	[0.5878]	[0.1213]	[0.4628]
t+1 x D-to-R	1.1702	-1.3825	0.0344	0.0844	-0.0136
	(5.5102)	(1.2026)	(0.0560)	(0.0545)	(0.0269)
	[0.8329]	[0.2569]	[0.5424]	[0.1292]	[0.6168]
Dep. mean	52.2	5.7	0.2937	0.0754	0.0517
N	248	248	248	248	248
County-Cycle	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Weight	Agency	Agency	Agency	Agency	Agency

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. This table reports the ATT estimates of D-to-R sheriff turnovers relative to D-to-D transitions (β_e^* in equation 2). Motor vehicle accidents that fit all of the following criteria are included: reportable (the accidents include a fatality, injury, or property damage of $1,000.00 or greater), on a local street, outside municipalities, or in municipalities without police departments, or in municipalities where the police departments are not required to report traffic stops. Dep. means are computed from D-to-R counties in year $t-1$. All regression specifications include county-cycle and calendar-year fixed effects.

Table 14: Long-term Impact of Sheriff Party Affiliation on the Share of Black Drivers

	$\frac{\text{\# of Black stops}}{\text{\# of all stops}}$				
	Sheriff's offices				Police departments
	(1)	(2)	(3)	(4)	(5)
t-3 x D-to-R	0.0003	0.0170	-0.0142	0.0062	0.0103
	(0.0112)	(0.0199)	(0.0127)	(0.0169)	(0.0096)
	[0.9784]	[0.3990]	[0.2815]	[0.7186]	[0.2968]
t-2 x D-to-R	0.0052	0.0067	-0.0204	-0.0036	-0.0171
	(0.0179)	(0.0137)	(0.0215)	(0.0293)	(0.0123)
	[0.7715]	[0.6272]	[0.3588]	[0.9039]	[0.1797]
t x D-to-R	0.0042	0.0026	0.0053	0.0056	0.0007
	(0.0074)	(0.0064)	(0.0108)	(0.0095)	(0.0151)
	[0.5702]	[0.6915]	[0.6308]	[0.5574]	[0.9611]
t+1 x D-to-R	0.0321	0.0265	0.0111	0.0313	0.0070
	(0.0120)	(0.0130)	(0.0124)	(0.0115)	(0.0121)
	[0.0118]	[0.0510]	[0.3877]	[0.0131]	[0.5709]
t+2 x D-to-R	0.0310	0.0239	-0.0093	0.0387	-0.0120
	(0.0201)	(0.0163)	(0.0244)	(0.0208)	(0.0108)
	[0.1324]	[0.1537]	[0.7080]	[0.0782]	[0.2794]
t+3 x D-to-R	0.0092	0.0152	-0.0272	-0.0069	-0.0065
	(0.0177)	(0.0157)	(0.0189)	(0.0199)	(0.0168)
	[0.6055]	[0.3415]	[0.1731]	[0.7318]	[0.7026]
t+4 x D-to-R	0.0124	0.0174	0.0002	0.0067	-0.0082
	(0.0139)	(0.0161)	(0.0176)	(0.0175)	(0.0156)
	[0.3775]	[0.2894]	[0.9920]	[0.7048]	[0.6041]
Dep. mean	0.2471	0.1881	0.2446	0.2656	0.3501
# of control	35	35	8	24	24
# of treatment	12	12	9	7	7
# cluster	31	31	15	21	21
N	376	376	144	248	248
Sample	All	All	Close election	Good data quality in sheriff's office and police departments	
County-Cycle	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Weight	Agency	# of stops	Agency	Agency	Agency

Notes: The sample inclusion criteria and regression specification are the same as in Table 4, except that I extend the length of the election cycle to 8 years. Some county-cycles that fit the sample inclusion criterion in the short cycle do not fit the criterion in the longer cycle (e.g., have ≤ 50 stops in later years). As a result, the number of county-cycles included is smaller than in Table 4. Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. The D-to-R dummy variable is one if the county experienced a D-to-R election in that cycle and zero if the county experienced a D-to-D election. This table reports the ATT estimates of D-to-R sheriff turnovers relative to D-to-D transitions (β_e^* in equation 2). Columns 1-4: sheriff's office stops. Column 5: police department stops. Columns 1-2: all county-cycles in Panel D, Table 1. Column 2 weights the observations by the average number of stops in pre-election years $t-2$ and $t-1$. Column 3 includes elections where the winner's vote share is less than 60%. Column 4 restricts to stops conducted by deputy sheriffs in county-cycles where at least one police department records more than 50 stops in every year of the election cycle. Column 5 limits the sample to stops from those police departments only. All regressions include county-cycle and calendar-year fixed effects. Dep. means are computed from D-to-R counties in year $t-1$. The number of controls and treatments is in the unit of county-cycles. The number of clusters is the number of unique counties, i.e., the number of groups in the estimation of cluster-robust s.e..

Appendices

A Additional tables

Table A.1: Impact of Sheriff Party Affiliation on the Share of Black Drivers: R-to-R versus R-to-D

Panel A: ATT estimates

	$\frac{\text{\# of Black driver}}{\text{\# of all stops}}$
	(1)
t-2 x D-to-R	-0.0438
	(0.0199)
	[0.0485]
t x D-to-R	0.0094
	(0.0115)
	[0.4314]
t+1 x D-to-R	-0.0095
	(0.0278)
	[0.7377]
Dep. mean	0.3731
N	52
# of control	10
# of treatment	3
County-Cycle	Yes
Year	Yes

Panel B: Urban category distribution among R-to-R and R-to-D groups

	R-to-R	R-to-D
Large Metro	0	1
Median Metro	1	2
Nonmetropolitan	9	0

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at county-year level. t refers to the year of election in that election cycle. R-to-D county-cycles with good traffic stop data only appear in the 2018 election cycle (Panel D, Table 1). County-cycles included in the estimation are 10 R-to-R counties and 3 R-to-D counties in the 2018 election cycle. Column 1, Panel A reports β_e^* in equation 2; the corresponding estimates represent the average treatment effect on the treated of the impact of an R-to-D sheriff turnover as opposed to an R-to-R transition under the parallel trend assumption. The Dep. mean is computed from R-to-D counties in year $t-1$, one year before the sheriff election. Panel B reports the urban category distribution of the 10 R-to-R and 3 R-to-D county-cycles included in the estimation.

Table A.2: Robustness checks of the Impact of Sheriff Party Affiliation on the Share of Black Drivers

	$\frac{\text{\# of Black stops}}{\text{\# of all stops}}$				
	Positive stops	Rural counties	Same sheriff race	Not-yet-treated	Sheriff turnover
	(1)	(2)	(3)	(4)	(5)
Panel A: Without urban group × year fixed effects					
t-2 x D-to-R	0.0091	-0.0068	0.0152	0.0042	0.0128
	(0.0146)	(0.0263)	(0.0138)	(0.0257)	(0.0191)
	[0.5380]	[0.7998]	[0.2780]	[0.8727]	[0.5090]
t x D-to-R	0.0122	0.0083	0.0100	0.0050	0.0000
	(0.0096)	(0.0092)	(0.0071)	(0.0094)	(0.0137)
	[0.2106]	[0.3777]	[0.1628]	[0.5982]	[0.9978]
t+1 x D-to-R	0.0402	0.0583	0.0365	0.0405	0.0214
	(0.0149)	(0.0222)	(0.0125)	(0.0154)	(0.0199)
	[0.0099]	[0.0163]	[0.0061]	[0.0147]	[0.2918]
Panel B: With urban group × year fixed effects					
t-2 x D-to-R	0.0133	-0.0068	0.0150	0.0029	0.0269
	(0.0160)	(0.0263)	(0.0134)	(0.0266)	(0.0249)
	[0.4090]	[0.7998]	[0.2711]	[0.9143]	[0.2911]
t x D-to-R	0.0121	0.0083	0.0108	0.0124	0.0067
	(0.0078)	(0.0092)	(0.0070)	(0.0071)	(0.0144)
	[0.1264]	[0.3777]	[0.1319]	[0.0947]	[0.6457]
t+1 x D-to-R	0.0478	0.0583	0.0354	0.0491	0.0433
	(0.0164)	(0.0222)	(0.0138)	(0.0224)	(0.0231)
	[0.0055]	[0.0163]	[0.0143]	[0.0381]	[0.0720]
Dep. mean	0.2392	0.2481	0.2216	0.2413	0.2413
# of control	55	22	42	21	14
# of treatment	17	9	13	15	15
# cluster	45	21	38	25	28
N	288	124	220	144	116
County-Cycle	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Weight	Agency	Agency	Agency	Agency	Agency

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. The sample is at the county-year level. t is the year of the election in that election cycle. This table reports, across different sample restrictions, the average treatment effect on the treated of D-to-R sheriff turnovers relative to D-to-D transitions (β_e^* in equation 2). Column 1 includes all county-cycles where the number of stops is always positive in a cycle, i.e., county-cycles in Panel C, Table 1. Column 2 restricts the sample to rural counties (nonmetropolitans in Table 2). Column 3 restricts the samples to elections where the sheriff's race did not change. Column 4 restricts the control group to D-to-D county-cycles with future D-to-R turnovers (future includes 2022 elections). Column 5 restricts the sample to elections with sheriff turnovers. All regressions include county-cycle and calendar-year fixed effects. Regressions in Panel B further include urban group *times* year fixed effects, where urban groups are large metro, small and medium metro, and nonmetropolitan as defined in Table 2. Dep. means are computed from D-to-R counties in year $t-1$. The number of controls and treatments is in the unit of county-cycles. The number of clusters refers to the number of unique counties, i.e., the number of groups used in the estimation of cluster-robust standard errors.

Table A.3: Decline in Stops Before Elections: Heterogeneity by Close Elections and Incumbent Participation

	# of all stops		Natural log of number of all stops	
	(1)	(2)	(3)	(4)
t-2 x D-to-R	-1.7282	-58.9250	-0.2245	-0.1867
	(224.4499)	(301.4427)	(0.1929)	(0.1674)
	[0.9939]	[0.8460]	[0.2511]	[0.2712]
t x D-to-R	-696.7205	-526.2500	-0.5736	-0.5354
	(157.0856)	(190.5244)	(0.2564)	(0.1907)
	[0.0001]	[0.0086]	[0.0308]	[0.0076]
t+1 x D-to-R	91.2487	568.2250	0.0058	-0.2585
	(323.5009)	(1,132.0192)	(0.2554)	(0.3826)
	[0.7793]	[0.6184]	[0.9820]	[0.5030]
t-2 x D-to-R x Close	228.1782		0.3624	
	(355.0663)		(0.2514)	
	[0.5240]		[0.1570]	
t x D-to-R x Close	620.3038		0.2954	
	(226.5330)		(0.3237)	
	[0.0091]		[0.3668]	
t+1 x D-to-R x Close	576.9679		0.1352	
	(1,116.1138)		(0.3934)	
	[0.6080]		[0.7328]	
t-2 x D-to-R x Incumbent		106.5650		-0.0372
		(342.1451)		(0.2855)
		[0.7570]		[0.8969]
t x D-to-R x Incumbent		265.2786		0.0654
		(204.5278)		(0.2462)
		[0.2019]		[0.7920]
t+1 x D-to-R x Incumbent		-721.1336		0.4412
		(1,112.6720)		(0.4154)
		[0.5205]		[0.2944]
Average # of stops			1,311	
# of non-close/incumbents not participating D-to-R elections	6	10	6	10
# of close/incumbents participating D-to-R elections	9	5	9	5
N	248	248	248	248
County-Cycle	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses. P-values against the null hypothesis that the estimate is zero are in the brackets. All outcome variables are at the county-year level. The outcome variables in columns 1-2 are the number of all stops. The outcome variables in columns 3-4 are the natural log of the number of all stops. t refers to the year of election in that election cycle. This table reports $\gamma_e^{0*}, \gamma_e^{1*}$ in equation 4. All regression specifications include county-cycle and calendar-year fixed effects. "Close" indicates whether the county experienced an election in which the winner's vote share is below 60%. "Incumbent" indicates whether the incumbent sheriffs participate in the elections. The average number of stops is computed from D-to-R counties in year $t-1$.

B Decomposition of the Total Changes in the Share of Black Drivers

Let B_{it} denote the share of Black drivers in all stops for county i in year t. Following the timing convention in this paper, $t = -2, -1, 0, 1$, I set $t = -1$ as the baseline period. Let S_{it} be the share of safety stops of all stops. Then $1 - S_{it}$ is the share of investigation stops of all stops. I denote B_{1it} and B_{2it} as the share of Black drivers in all safety and investigation stops. I can then write:

$$B_{it} = S_{it} \times B_{1it} + (1 - S_{it}) \times B_{2it}.$$

Rewriting the level of shares as the baseline level plus deviations, we have:

$$\begin{aligned}
B_{it} &= B_{i,-1} + \Delta B_{i,(-1,t)}, \\
S_{it} &= S_{i,-1} + \Delta S_{i,(-1,t)}, \\
B_{1it} &= B_{1i,-1} + \Delta B_{1i,(-1,t)}, \\
B_{2it} &= B_{2i,-1} + \Delta B_{2i,(-1,t)}.
\end{aligned}$$

Taking the difference $B_{it} - B_{i,-1}$, we have:

$$\begin{aligned}
B_{it} - B_{i,-1} = \; & \underbrace{[S_{i,-1} \cdot \Delta B_{1i,(-1,t)}]}_{\text{Changes within Safety Stops}} + \underbrace{[(1 - S_{i,-1}) \cdot \Delta B_{2i,(-1,t)}]}_{\text{Changes within Investigation Stops}} \\
& + \underbrace{[\Delta S_{i,(-1,t)} \cdot B_{1i,-1} - \Delta S_{i,(-1,t)} \cdot B_{2i,-1}]}_{\text{Changes from Shares of Safety Stops}} \\
& + \underbrace{[\Delta S_{i,(-1,t)} \cdot (\Delta B_{1i,(-1,t)} - \Delta B_{2i,(-1,t)})]}_{\text{Second Order Changes}}.
\end{aligned}$$

Decomposing the difference, the first bracket is the contribution from the changes in the share of Black drivers in all safety stops; the second bracket is the contribution from the changes in the share of Black drivers in all investigation stops. The first and second brackets are the outcome variables in Columns 4-5 in Table 6. The third bracket represents the contribution from changes in the share of safety stops among all stops, while the fourth bracket represents the leftover second-order term. The third and fourth brackets are the outcome variables in Columns 3 and 6 in Table 6.

The coefficients of interest are the same, no matter whether I specify the outcome variable as the difference between two periods or the level in that year/ To see this, I duplicate equation 1 below:

$$Y_{cle} = \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \beta_{le} D_{cl}^{D-to-R} \cdot \eta_e \cdot \eta_l + \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot \eta_e + \\ \delta_{le} + \delta_{cl} + \epsilon_{cle}$$

Taking the difference $Y_{cle} - Y_{cl,-1}$, we have:

$$Y_{cle} - Y_{cl,-1} = \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \beta_{le} D_{cl}^{D-to-R} \cdot (\eta_e - \eta_{-1}) \cdot \eta_l + \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot (\eta_e - \eta_{-1}) + \\ + (\delta_{le} - \delta_{l,-1}) + (\epsilon_{cle} - \epsilon_{cl,-1}). \tag{5}$$

Hence, I can use the terms in the four brackets above as outcome variables, and estimate four regressions with specifications 5 (similar to equation 1 but without county-cycle fixed effects), and have four sets of regression coefficient estimates that would add up to the coefficient estimates using the share of Black drivers as the outcome variable. The ATT estimates can be derived as in equation 2.

The decomposition analysis in section 5.3 is done in the same procedure by defining B_{1it} and B_{2it} as the share of Black drivers within stops done by stayers and non-stayers for county i in year t.