The Impact of Partisan Politics on Policing Practices: Evidence from North Carolina's Sheriff's Offices

Wei-Lin Chen*

August 7, 2023

Abstract

I study the impact of partisan leaders on traffic stop policing behaviors in North Carolina. Using a difference-in-differences design that exploits sheriff turnovers, I find that offices with a Democrat-to-Republican sheriff turnover rather than a Democrat-to-Democrat sheriff transition have an increase of black drivers' share in traffic stops by 3.2 percentage points, a 13.5% increase compared to baseline. Decomposing the changes in black driver's share along two dimensions: stop purposes and officers, I find that the increase is driven by changes *within* safety stops instead of investigation stops, and driven by changes in incumbent officers' tendency to stop black drivers. The increase in racial disparities is not accompanied by an increase in unconditional hit rates. Overall, the results suggest that partisan leadership, a crucial feature of the US criminal justice system, plays an important role in shaping racial disparities in frontline policing.

*Department of Economics, UC San Diego. wec155@ucsd.edu

1 Introduction

The criminal justice system in the United States is deeply related to and influenced by partisan politics due to the political process of personnel selection. Although leaders of local law-enforcement agencies are often elected, the impact of political preferences on frontline policing is not well-understood. This paper studies the impact of the political party affiliation of leaders on one of the most frequent interactions Americans have with law-enforcement officers: traffic stops.

I examine the impact of partisan leadership 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 more likely to be searched than White drivers (Pierson et al., 2020). A vast literature studies to what extent the racial disparities come from racial bias and has established evidence of racial discrimination at the officer level (Antonovics and Knight, 2009; Goncalves and Mello, 2021). I start from a different point in the hierarchy of law-enforcement agencies and ask if leaders matter in determining racial disparities of frontline traffic stops.

This paper focuses on sheriff's offices in North Carolina. I focus on sheriff's offices instead of police departments since sheriffs are elected through partisan elections. I can thus directly identify sheriff's party affiliations. By exploiting party turnovers of sheriffs induced by elections, I identify the impact of the party affiliation of sheriffs on offices' traffic stop behaviors. One central challenge in estimating the relationship between party affiliation of local law-enforcement leaders and traffic stop behaviors 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 crime rate changes and gentrification development, 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 Democrat-to-Democrat (henceforth D-to-D) sheriff transition that does not necessarily involve a leader turnover; the treatment group is counties that experience Democrat-to-Republican (henceforth D-to-R) sheriff turnover. I analyze turnovers from the 2010, 2014, and 2018 elections. For each election, we examine traffic stops in an election cycle defined as from 3 years before the election to 1 year after the election. This definition of election cycle allows us to stack up data from 3 election cycles without having overlapping timing periods.

Using a differences-in-differences framework, I find that Republican sheriffs' leadership alters the racial composition of stopped drivers. Republican sheriffs increase the share of Black drivers by 3.2 percentage points, a 13.5% increase compared to the baseline period (two years before the election). To investigate which new policies and instruments the Republican sheriffs use that result in an increase in racial disparities, I decompose the changes in the black driver's share along two dimensions: the initial purpose of stops and the type of patrolling 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). How much focus should a law-enforcement agency put on each type of stop is under debate in North Carolina. The Fayetteville police department chief, in 2013, proposed to minimize the number of stops due to non-moving violations to avoid unnecessary traffic stops. The Mecklenburg County sheriff proposed a similar policy in 2022 because he was presented with information that Black drivers are disproportionately affected by investigation traffic stops.

To see if Republican sheriffs' focus on the two types of stops systematically differs from Democrat sheriffs and thus contributes to the changes in black drivers' share, I examine the impact of partisan leadership on the share of safety stops. I find that Republican sheriffs decrease the share of safety stops by 8.8 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 16.5% of the increase in the black driver's share. 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 role. In particular, the change in black drivers' share *within* safety stops accounts for 68% of the overall change in black drivers' share. This decomposition result suggests that in understanding racial disparities in stops, contrary to the literature's focus on investigation stops, in which officers are thought to have more discretion and hence more likely to exhibit racial bias. Policies in conducting traffic safety stops may require more attention.

I consider two channels regarding personnel policies that may result in a change in traffic stop practice: (1) reshuffling of officers based on officers' policy preferences regarding traffic stops; (2) incumbent officers change their stop practices in response to the new leadership. I find evidence supporting the latter channel. Regarding personnel reshuffling, I find that D-to-R transitions are associated with more reshuffling of officers. The share of stops conducted by incumbent officers in D-to-R counties is 19 percentage points (a 34% increase compared to baseline) lower than in D-to-D counties post-elections. However, the reshuffling does not lead to a change in overall stop practices. The officers who were shuffled *in* are similar to those shuffled *out* regarding the share of black drivers among their stops.

Do officers alter their traffic practices in response to the new leadership? I find that the incumbent officers, who continued to conduct traffic stops in post-election years in D-to-R counties, increased the black driver's share in their stops by four percentage points compared to the incumbent officers in D-to-D counties, a 17.5 % increase compared to the baseline. Further, I find that the increase in the black driver' share among incumbent officers is not driven by a few bad apples but by many officers having small to medium-level changes in the tendency to stop black drivers. I thus provide a case where the reshuffling of officers does not lead to systematic changes in the observed policy practices, but the leaders reshape the policy practices by making incumbent officers alter their ways of conduct.

I next analyze an important decision officers make after stopping a driver: whether to search a vehicle. I examine the impact of sheriff's party affiliation on the overall search rates and search rates within racial groups. Note that, with the new sheriff's traffic stop practices, relevant characteristics of the stopped driver composition (regarding suspiciousness of holding contraband, for example) likely change in the post-election year. 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. I find no significant impacts of sheriff's party affiliation on the overall and within racial group search rates.

Whether a trade-off between racial disparities in traffic stops and *efficiency* exists is a central focus in the literature (Feigenberg and Miller, 2022). Since finding contraband is at least a part of the goals in conducting traffic stops, an reasonable efficiency measure is the unconditional hit rate, defined as the number of searches with found contraband divided by the number of total stops. I find that the D-to-R transition is *not* associated with statistically significant changes in the overall unconditional hit rates.

At last, I examine the long-term impact. I find that the impact of sheriff's party affiliation on traffic stop disparities may be short-lived. I argue that such a short-lived impact may not be surprising given that sheriffs face temporal electoral incentives every fours years and drivers may swiftly change their driving routine 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 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 preferences of leaders matter in determining racial disparities in frontline policing, where literature has identified the importance of voters the leaders face (Facchini et al., 2020), the race of the leaders (Bulman, 2019), and the racial composition of the police force (McCrary, 2007). Very recent literature identified the heterogeneity of racial bias at the officer level (Goncalves and Mello, 2021) and suggested that officers with different levels of bias have varied traffic stop behaviors responding to short-term political events (Grosjean et al., 2022).

The impact of partisanship on law enforcement is not without ambiguity *ex ante*. Although survey evidence shows that party affiliation of the general public is correlated with attitudes toward policing policies such as body cams and police force size (Hansen and Navarro, 2021), the political preferences of the 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.

I also contribute to the literature that emphasizes the importance of political turnover in personnel in public organizations. Political turnover is often associated with personnel changes on account of patronage. Colonnelli et al. (2020) finds that supporters of the party in power in Brazil are more likely to be hired and are negatively selected on their competence. Akhtari et al. (2022) finds that local mayor election turnovers in Brazil are linked to new personnel turnovers in schools and are further accompanied by lower student test scores. I provide a case where leaders' political party turnovers are associated with a new assignment of duties (assigned to traffic stop teams or not), but the new assignment seems not to be based on specific policy preferences. 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 research design and empirical methods in section 4. Results are discussed in section 5 and a tentative conclusion is presented in section 6.

2 Background

2.1 Law-Enforcement Agencies in North Carolina

Sheriffs' offices are the top law enforcement agencies in counties. They perform duties in unincorporated areas within counties. Police departments in municipal governments are in charge of law enforcement in incorporated areas. The main functionality of sheriff's offices includes management of jails and detention centers, crime investigation, immigrants detention, patrol, and document application such as gun permits. In this paper, I focus on the traffic stop and search. Patrol officers account for a fifth of the personnel in sheriff's offices in North Carolina, while jailers and detectives/investigators account for respectively 36% and 10% of the personnel. Police departments do not manage jails, so they assign more personnel to patrol and investigation, 46% for patrol and 14% for investigation.[1] As a result, police officers conduct much more stops than deputy sheriffs. During 2008-2019 (my sample period), on average, deputy sheriffs conducted about a hundred thousand stops a year, while police officers conducted about six hundred seventy thousand stops.

Each of the one hundred counties in North Carolina has one sheriff's office. Voters directly elect all sheriffs in North Carolina. The elections are partisan; they occur every four years in November, and there are no term limits. The newly elected sheriffs are sworn in on November 30, and the deputies would also take their oath on the same day. This feature guides our analysis of officer turnover starting from the election year, not one year after the elections. All of the elected sheriffs since 1998 are affiliated with either the democratic party or the republican party. We use sheriffs' turnovers induced by elections as the main variation of change of control. In particular, we focus on sheriff's turnovers that involve party turnovers. Police chiefs, who are the leaders

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

of the police departments, on the other hand, are appointed by municipal councilors.

2.2 Traffic Stop

Law-enforcement officers stop drivers for two main reasons. First, the driver exhibits reckless driving, such as speeding. Second, officers stop drivers for nonmoving violations. This includes equipment failures such as broken tail lights, 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 use vehicle regulation violations as a pretext to stop drivers in pursuit of potential criminal investigations or searches for drug possession. The weight of focus on the two types is a salient policy issue in North Carolina. For example, in 2013, the police chief in Fayetteville announced that the police department would minimize the number of traffic stops due to nonmoving violations. In 2022, the sheriff's office in Mecklenburg County announced that they would no longer stop drivers for nonmoving violations. [2] During the traffic stop process, an officer decides whether to search the vehicle. This is a decision in that officers have much discretionary power. By law, officers can search a vehicle as long as the officers have probable cause to believe that a law has been broken. Regardless of whether a search is conducted, a traffic stop leads to four actions: no action, warning, citation, and arrest. During searches, an officer might find contraband, including drugs, alcohol, or weapons.

3 Data

I use traffic stop and search data and sheriff elections record to analyze the effect of sheriffs' party affiliation on officers' traffic stop and search behaviors.

Traffic Stop and Search Records.

The traffic stop and search records are available upon request in North Carolina.

[2] See https://www.usatoday.com/story/news/nation/2021/04/15/police-reform-fayetteville-burlington-nc-traffic-stops-policing/7225318002/ for a coverage about Fayetteville police department and see https://www.foxnews.com/us/north-carolina-sheriffs-office-stops-pulling-drivers-non-moving-traffic-violations for a coverage about Mecklenburg county sheriff's office. Fliss et al. (2020) used a synthetical control method and found that the policy in Fayetteville leads to a reduction of traffic crashes and injuries and a decrease of Black percent of traffic stops.

The data set contains the driver's race, ethnicity, gender, and age. Unique officer IDs are included in the data. I use the IDs to identify officers who stop performing traffic-stop tasks after elections.[3] The IDs are not linked to other information about officers, such as names, races, or ages. The data set includes the time and the name of the location of each stop. The name of the location can be a county, a city/town, a census-designated place (CDP), or some location names used by locals. Around 60% of the stops only record the location at the county level. This significantly restricts our analysis of officers' patrolling location decisions.

Each stop is associated with one of the twelve stop purposes: speed limit violation, stop light/sign violation, driving while impaired, safe movement violation, vehicle equipment violation, vehicle regulatory violation, seat belt violation, investigation, other motor vehicle violation, and checkpoint. Following Baumgartner et al. (2018), I exclude the sample associated with the checkpoint because such stops are recorded only when searches are conducted. I classify stops into two types: safety and investigation. Safety stops include ones associated with speed limit violations, stop light/sign violations, driving while impaired, and safe movement violation. Investigation stops include ones associated with vehicle equipment violations, vehicle regulatory violations, seat belt violations, investigation, and other motor vehicle violations. I also use information about whether a search is conducted and whether any contraband is found during a search to construct outcome variables.

Sheriff Election Records.

Sheriff's election results since 2008 are publicly available on the North Carolina State Board of Elections website. Party affiliation and the names of the elected sheriffs are used to determine if a county went through sheriff turnovers and party turnovers. Vote shares of the winners are used to assess the competitiveness of the elections.

[3] The officer ID is only unique within the law enforcement agency. We cannot track officers across agencies.

4 Empirical methods

4.1 Research Design

I aim to identify the causal effect of sheriff's party affiliation on traffic stop practices. To this goal, I adopt a difference-in-differences design, comparing counties that experience elections resulting in Democrat-to-Democrat transitions with counties that experienced Democrat-to-Republican transitions. I define an election cycle from three years before an election to one year after. This definition allows no overlapping calendar years across election cycles but limits the time horizon of the analysis. Since new sheriffs are sworn in on November 30, I define an election year from December to November.

Table 1 reports the sheriff election results from 2010 to 2018. Only four elections involve Republican-to-Democrat type turnover. I do not compare counties with elections of Republican-to-Democrat with counties with elections of Republican-to-Republican turnover in this paper due to power concerns. I define the control group as the county-election cycles that experience Democrat-to-Democrat-type elections. The treatment group includes county-election cycles that experience Democrat-to-Republican type elections.

Panel D of Table 1 shows the winners' vote share distribution. All Democrat-to-Republican elections have winner's vote shares of less than 80%. To match on the winners' vote shares, I confine samples to the county-cycles where the winner's vote shares are less than 80%. Panel B shows the number of county-cycles in each election type after I apply this restriction. Our analysis of personnel turnover includes the county-cycles in Panel B.

I exclude the county-cycles where a sheriff's office conduct less than 50 stops in at least an election year within the election cycle. Two reasons for this criterion. First, the decomposition analysis would not make sense if the number of stops within certain types (safety and investigation stops) and by certain officers (incumbent officers and others) is small. Second, I aim to have consistent "report" quality across years. Some counties excluded by this restriction have huge fluctuations in the number of stops across the years. e.g., New Hanover had four stops in 2009 and 890 stops in 2010. Some counties have zero stops in a year and hundreds in adjacent years. These patterns cast doubt on whether the reported traffic stops reflect a representative sample of all stops in counties where the number of stops fluctuates dramatically. I chose the number 50 based on my judgment of trading off losing too many counties and including bad-quality reports. The resulting number of county-cycles of each election type are presented in Panel C of table 1.

Summary Statistics.

Table 2 presents the summary statistics of traffic stops and searches in the county-cycles I include in our analysis (Panel C in Table 1). I report descriptive shares on race, gender, and traffic stop types. The driver is female in 35% of the stops, black in 25% of the stops, Hispanic in 7% of the stops, and white in 65% of stops. Due to the small share of Hispanic drivers, in the following analysis, I divide the drivers into black and non-black groups. [4] 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%). The difference in the search rates between black and white drivers is much smaller than the one seen in Feigenberg and Miller (2022).

Dividing stops 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 in 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 two types of stops, around 31%.

4.2 Estimating specifications

To estimate the causal effect of sheriff's party affiliation on traffic stop practices, I estimate an ordinary least square regression with a difference-in-differences type specification:

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

where Y_{cle} is a variable at county-year level for county c in year e in cycle l at county-year level. Treatment group status in each election cycle is denoted by D_{cl}^{D-to-R}, δ_{cl} is county-cycle fixed effects. I separate data into three election cycles, denoted as l. I use election results from 2010, 2014, and 2018. Hence l can take three values, 2010, 2014, and 2018. I treat the year before the election as the baseline year. In tables

[4] Other races, including Asians, Native Americans, and Other/Unknown, account for around 2% of stops and are included in the non-black group.

and figures, the time convention is as follows: I denote the year when the election happened as t and other years as $t-2, t-1, t+1$. In regression specifications, the time convention chronologically in an election cycle is denoted as $e = -1, -1, 0, 1$. Since the new sheriff is sworn in on November 30, I define an election year starting from December to November. For example, the year t ($e = 0$) in the 2010 election cycle involved observations from December 2009 to November 2010. Hence, δ_{le} uniquely defines the timing of each stop in year e in cycle l. I use the year before the election as the omitted base year. I analyze at the county level instead of the stop level because I are interested in the causal effect of leadership on law enforcement agencies.

The coefficients of interest are β_e, which captures the differences between control and treatment groups across years within a cycle. All standard errors are clustered at the county level throughout the paper unless stated otherwise. I first examine if partisan leadership impacts law enforcement practices toward minority race groups by considering the outcome variable of the share of black drivers among all stopped drivers. The first traffic stop policy I look at is the relative weight on traffic safety and investigation. The outcome variable here is the share of safety stops among all stops.

5 Results

5.1 Black driver's share

Graphical Evidence.

I plot the raw data in Figure 1 to show the data 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 counties have higher black driver's shares than D-to-R and R-to-R counties since D-to-D counties are generally more urban areas. Before the election, the black driver's share gap between the three groups stays roughly constant across the years within an election cycle. This gives me confidence that the parallel pre-trend assumption, required by the difference-in-differences research design, is satisfied in this setting. One year after the election, however, the black driver's share in D-to-R counties increased while the shares in D-to-D and R-to-R counties barely change. I present regression results capturing the increase in D-to-R counties next.

Regression estimation results.

Table 3 reports the estimates of β_e in equation 1 with black driver's share as the outcome variable. Column (1) shows that the black driver's share increases by 3.2 percentage points in D-to-R counties one year after the election 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 13.5% increase in the black driver's share.

From Columns (2)-(4), I probe the robustness of the impact of sheriff's party affiliation on black driver's shares by weighting the sample, restricting the sample to close elections, and examining a placebo scenario. In Column (2), I report the regression results with a sample weighted by the number of stops for the county two years before elections ($t - 2$). The weight of a county within a cycle is thus fixed. The estimates would be similar to Column (1) results if there is not much causal effect heterogeneity along the number of stops dimension. The standard errors may be smaller when I weigh the samples by the number of stops if the number of stops varies tremendously and the error term variation mostly comes from within county-cycle (see (Solon et al., 2015) for simple examples comparing regression results with and without weights). I find that the magnitude of the estimate from the weighted regression is similar to the unweighted one, suggesting that the effect of sheriff's party affiliation does not vary on the traffic stop size. The s.e. becomes slightly larger.

In Column (3), I follow the spirit of regression discontinuity designs with close elections and restrict the sample to counties with winners' vote share below 60%. The magnitude of the estimate is similar to Column (1), but the standard errors become much larger, resulting in the statistical insignificance of the estimate. In Column (4), I look into a placebo scenario, the traffic stops done by the police departments in the D-to-D and D-to-R counties. Although deputy sheriffs and police officers may focus on different neighborhoods in patrolling, the placebo scenario should still capture changes in the driver's population (if any) to some extent. I find that the magnitude of the estimate of the interaction term between $t + 1$ and D-to-R dummy variables is much smaller for stops done by police officers. The similar magnitudes of the estimates of the post-election interaction term in Column (1)-(3) and the much smaller magnitudes in Column (4) suggest that the increase of the black driver's share in D-to-R counties after the elections are driven by the change of traffic stop practices associated with the newly elected Republican sheriffs, instead of changes in the driver's population in specific counties.

Changes in Levels.

Table 3 focus on the change in shares; I now turn to the changes in the levels to know if more Black drivers are stopped. Table 4 columns (1) and (2) report the regression estimates from the same estimating specification as in equation 1, with the natural log of the number of stops in the separate race groups. Although the magnitudes of the coefficients of the post-election and D-to-R interaction term is large in columns (1), we cannot reject the null of no change in the number of Black stops at the 10 percent significance level.

To compare the change of levels across racial groups, I estimate a triple difference-in-differences specification as follows:

$$\begin{aligned} Y_{cleg} = &\sum_{e=-2}^{1} \gamma_e D_{cl}^{D-to-R} \cdot \eta_e \cdot G_g + \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot \eta_e \\ &+ D_{cl}^{D-to-R} \cdot G_g + G_g + \eta_e \cdot G_g + \delta_{le} + \delta_{cl} + \epsilon_{cle}, \end{aligned} \tag{2}$$

where G denotes groups: black and non-black. Other notations are defined as in equation 1. I report the estimates of γ_e and β_e in equation **??** in column (3). Black stops marginally significantly increase more than non-black stops by 15%. Combining the estimates in Column (1)-(3), I interpret the changes in the black driver's shares observed in Table 3 driven by an increase in the number of stops of the black drivers, instead of a decrease of the number of stops of the white drivers.

A notable pattern, the decrease of the number of stops in the election year t is shown in Table 4, Column (1)-(3). I form two hypotheses based on the following two observations. I observe that the D-to-R elections are more likely to be close elections and less likely to have incumbents participating in elections. Two-thirds of the D-to-R elections have the winner's vote share below 60% (close elections), while one-third of the D-to-D elections are close elections. Incumbent sheriffs participate in the elections in one-third of the D-to-R elections. For D-to-D, such share is 70%. Incumbent sheriffs may shirk their effort if they do not run for elections and hence do not have incentives to win votes (Losak and Makowsky, 2022), leading to a lower number of traffic stops. On the other hand, voters may care more about arrest rates or other measures of law enforcement performance than traffic stop practices. Elections campaign activities may crowd out sheriff's efforts on managing deputies. Both channels would imply that deputies in counties with competitive elections may conduct fewer stops.

In Column (4), I test the hypothesis by comparing the number of stops between counties with close elections (winner's vote share below 60%) and others. The counties included in the estimation are the same as in Columns (1) and (2). I denote the counties with close elections as one with the Close dummy variable, zero otherwise. The magnitude of the estimate of the interaction term between the election year t dummy and the Close dummy variables is much smaller than the magnitude of the estimate of the interaction term between the election year t dummy and the D-to-R dummy variable seen in Columns (1) and (2), suggesting that the competitiveness of the sheriff elections does not drive the decrease in the number of stops. I will test the second hypothesis in the future.

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

5.2 Initial purpose of traffic stops

The first traffic stop policy dimension we examine is the initial purpose of traffic stops. Motivated by the policy proposals seen in the Fayetteville police department and the Mecklenburg county sheriff's office, and the literature which finds that officers enjoy more discretionary power in investigatory stops (Roach et al., 2022), we examine if the share of safety stops changes as the counties elected new Republican sheriffs. In Table 5, column (2), I display the estimation results of equation 1 with the outcome variable the share of safety stops. I find that the share of safety stops decreases by 8.8 percentage points after electing a Republican sheriff. Compared to the dependent variable mean in D-to-R counties in the year before the election, this is a 16.6% decrease. We also find a marginally significant decrease of the share of safety stops in the election year in D-to-R counties. In our next steps, I will examine if the election competitiveness or whether the incumbent sheriff participate in the election can explain the decrease.

Changes in the focus on safety and investigatory stops can have racial disparate impact. Black driver's share is generally higher in safety stops than in investigation stops (see Table 2). Assuming that the black driver's share within the safety and investigation stop stays constant after the election in each county, the mere change in the share of safety stops can generate changes in the overall black driver's share. On the other hand, sheriffs may make officers 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 into four parts: 1) the part contributed by the changes in the share of safety stops (while holding black driver's share within two types of stops constant), 2) the part contributed by the changes within the safety stops, 3) the part contributed by the changes within the investigation stops, and 4) the left-over second order changes. The derivation of the decomposition is in Appendix.

I report the decomposition results in Table 5, Columns (3)-(6). Note that coefficients in Columns (3)-(6) add up to the coefficient in Column (1). Column (3) shows that the changes in the share of safety stops contribute to the change in the black driver's share but to a small extent. Only 16% of the changes in the black driver's share can be explained by the changes in the share of safety stops. Most of the increase in the black driver's share associated with the Republic sheriffs is contributed by the changes within the safety stops, not investigation stops, as seen in Columns (4) and (5). In future steps, I will reach out to people familiar with traffic stop practices and try to have more informative speculations on what kind of traffic stop practices within safety stops might increase black drivers' share.

The previous exercise hold the black share constant within the safety and investigation category before and after the elections. I now examine the magnitude of the black driver's share changes in each group. Table 3, column (4) and (5) shows that black driver's share increases significantly, 4.4%, in safety stops and is statistically significant at 95% confidence level. The magnitudes of the changes in investigatory stops is also large, 1.6%, but the standard error is large. I thus cannot reject the null of no changes.

5.3 Personnel policies

Officers play important roles in shaping racial disparities in traffic stops (Antonovics and Knight, 2009; Goncalves and Mello, 2021; Grosjean et al., 2022). Literature, however, knows little about how officers respond to leadership and whether leaders assign traffic stop tasks based on officers' traffic stop styles that may be related to the share of black drivers officers stop. I test two mechanisms related to officers that may lead to the increase in black drivers' share. First, officers respond to the new sheriff's policy preferences by changing traffic stop practices. Second, officers do not change their traffic stop practices, but the reshuffling of the personnel done by the new sheriffs makes the agencies have a higher Black drivers' share in traffic stops.

To test the two mechanisms, I define two sets of officers and decompose the difference in black drivers' share at the agency level across years into four parts, in the same way as in section . I define "stayers" as those who conduct traffic stops both before and after elections. I define "non-stayers" as those conducting traffic stops only before or after the elections. The black driver's share is a weighted average of the black driver's shares of two groups of stops: those conducted by stayers and others done by non-stayers. The changes in the black driver's share across years can thus be decomposed into first, holding the black driver's share within stayer and non-stayer stops the same as in the base year, changes in the share of stops done by stayers, second and third, holding the share of stops done by stayers the same as in the baseline year, changes in the black driver' share within stayer stops and non-stayer stops, and fourth, the second-order changes. For details of the decomposition, see Appendix.

The first mechanism, the officers' response to new leadership, would be captured by the second decomposed part: the changes in the black driver's share *within* stayer stops. The second mechanism, the personnel reshuffling, would be captured by the first and third decomposed parts. The first decomposed part would explain some of the total changes if the new sheriff shuffled specific types of officers out of the patrolling team, making stayers and non-stayers (before the election) stops have very different levels of the black driver's share. The third decomposed part would contribute to the total changes if the new sheriffs shuffled specific types of officers out *or* in, making the levels of the black driver's shares within non-stayers vary over time.

Table 6 reports the decomposition results. The total changes in the black driver's share in Column (1) are decomposed into four parts in Columns (3)-(6). Column 2 shows that D-to-R transitions are significantly associated with a smaller share of stops done by stayers post elections, a 19 percentage points decrease. This is consistent with a scenario where new sheriffs assign patrolling duties to different officers after elections. Although the shares of non-stayers stop increase after the elections, such changes cannot explain the changes in the black driver's share. as the interaction term between post-election and D-to-R dummy is small and insignificant in Column (3). This suggests that the selection of officers continuing the patrolling duty among all who conducted stops before the elections is not based on the black driver's share at the officer level. The bulk of the changes in the black driver's share at the agency level is explained by the second decomposed parts, shown in Column (4), the changes of the black driver' share *within* stayers. Within stayer changes (holding the share of stayer stops constant) account for 79% of the total changes in black drivers' shares. The changes within non-stayers, shown in Column (5), are a non-negligible magnitude but not significant. Overall, the decomposition results in Table 6 offers evidence in favor of the mechanism where officers' response to new leaders contributes to the changes in the black driver's share. I next provide further evidence supporting this mechanism.

Column (4) and (5) in Table 6 takes into account the changes within stayers/non-stayers while assigning the weight by the share of stayer/non-stayer stops for each agency. I now give every agency the same weight and directly examine the changes in black driver's share within stayers and non-stayers by running a regression with specification 1 with outcome variables: black driver's share within stayer stops and black driver' share within non-stayer stops. The estimation results are reported in Columns (1) and (2) in Table 7. Column (1) shows that stayers in D-to-R agencies, on average, increase the black driver' share by four percentage points after elections relative to the changes in stayers in D-to-D agencies. The non-stayers in post-election years in D-to-R agencies do not behave very differently compared to pre-election years relative to the behavior changes in non-stayers in D-to-D agencies. Column (1) suggests that the stayers as a whole group change their traffic stop practices in D-to-R agencies, but it speaks little to whether the changes come from a small set of officers, i.e., a few bad apples, or come from a wide set of officers. The evidence presented next goes against the a few bad apple theory.

To examine whether stayers change their traffic stop practices against certain racial groups, I measure the "tendency to stop black drivers" in the following way and examine the changes in the tendencies at the officer level across the years. The tendency to stop black drivers at the officer level is measured in two steps. First, I regress a dummy variable of whether a driver is Black on stop time fixed effects and stop location fixed effects. Stop times are at the quarter-period level. There are four quarters in a year and four time periods in a day divided by three time points: six am, noon, and six pm. Stop locations are the finest geography level recorded for the stop. They can be county, city, census-designated places (CDP), or intersections. Second, I take the average of the residuals of the regression at the officer level. I aim to distinguish whether a small or wide set of stayers change their tendencies after elections. To do so, for each officer in an election cycle, I take two averages, one comes from stops before and one comes from stops after elections. I then take the differences in the tendencies within officers and plot the cumulative distribution function of the differences in Figure 2. Two features in Figure 2 go against the a few bad apple theory. First, the officers ranked top ten percent in terms of their tendency changes in D-to-D and D-to-R counties have similar levels of differences. If the behavior changes in Column (1) in Table 7 are driven by a few officers, I would expect the top ten percent of officers in D-to-R counties to have larger tendency changes than the ones in D-to-D counties. Second, the officers ranked between 30-90 percentiles in D-to-R counties have higher-level changes in their tendencies to stop black drivers post elections than in D-to-D counties.

I conclude the personnel analysis by examining how much more reshuffling happens in D-to-R counties than in D-to-D counties. I run a regression in specification 1 with outcome variables being the share of officers who are non-stayers and who are new officers at the agency level. An officer is a new officer in that year if the first traffic stop done by him/her in that agency is recorded in that year. [5] Column (3) in Table 7 shows that D-to-R counties have an increase in the share of non-stayers by sixteen percentage points, compared to D-to-D counties after elections. The increase in the share of non-stayers, not just the share of stops done by non-stayers (Column (2) in Table 6, suggests that the new Republican sheriffs shuffle many officers who did not conduct traffic stops into the teams that involve traffic stop duties. In particular, Column (4) in Table 7 suggests that Republican sheriffs shuffle in many "new" officers.

I provide two takeaways from the analysis of officers. First, a large set of officers in D-to-R counties seem to change their traffic stop practices in response to the new Republican leadership. Second, new Republican sheriffs reshuffle the patrolling teams by assigning new officers to the teams. But the officers shuffled out and in behave similarly in terms of the share of black drivers they stopped. The two takeaways contribute to the literature by showing that officers' behavior may be malleable by a leader's management/policy. Policymakers who aim to reduce racial disparities in

[5] We can only identify unique officer IDs within agencies so we cannot identify the first traffic stop in an officer's career in North Carolina.

traffic stops can potentially learn from the differences in the management/policies done by law enforcement leaders from different party affiliations.

5.4 Patrol Policies

The last policy dimension I look at is the patrolling time and locations. To see if the Republican sheriffs focus on patrolling at times and locations with more black drivers on the road, I conduct an exercise to see if predictions on whether a stopped driver is Black in post-election years based on time and locations using pre-election data can explain the changes in black driver's share seen in Column (1) Table 3.

The exercise consists of two steps. First, using only the pre-election data within an election cycle, I regress a dummy variable indicating whether a stop is a Black stop (driver is Black) on stop location or stop time fixed effects. As defined in the previous sections, the Stop times are at the quarter-period level. There are four quarters in a year and four time periods in a day divided by three time points: six am, noon, and six pm. Stop locations are the finest geography level recorded for the stop. They can be county, city, census-designated places (CDP), or intersections. Unfortunately, only 60% of the stops contain geographical information finer than the county level. I then use the OLS coefficients on the stop time or stop location fixed effects (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 a regression in specification 1 with such 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 locations do not significantly change in D-to-R counties in post-election years. This holds true for both safety (Column (3)-(4)) and investigation stops (Column (5)-(6)). The regression estimation results suggest that the changes in the black driver' share under the new Republican sheriffs' leadership are not driven by a shift of focus in patrolling certain neighborhoods or times of the day. I conclude on the patrolling policies by providing a caution: around 40% of the stops do not have stop neighborhood information in the estimation sample. A shift of focus in the neighborhood may not be detectable with such data. Further research on the impact of leaders on traffic stops should try to find a setting with better stop location data.

5.5 Search Rate

Thus far, I examine if partisan leadership affects whom to stop. I now turn to the behaviors after stopping a driver: whether to search a vehicle or not. I report the changes in the search rate for all stops and stops in different racial groups. I then further examine the search rate separately for safety and investigation stops. Since the stop decision is shown to be affected by the previous sections, the changes in the search rates should be interpreted as the *combined* impacts of stop and search policy changes associated with the new Republic sheriff. In particular, one should not interpret the changes in the search rates (if any) as the changes in the officer's search behavior, holding the stopped driver's population the same as before elections. [6] Instead, the thought exercise here is to hold the at-risk population of being stopped the same. In particular, the proportion of drivers with contraband and drivers with unsafe driving behaviors in each racial group is thought to be unchanged right before and after elections.

Table 9 reports the regression estimation results in specification 1 with overall search rates, Black driver search rates, and non-Black driver search rates at the county-year level being outcome variables. Results in Panel A show that search rates in all stops (combining safety and investigatory) do not significantly change in D-to-R counties in post-election years.

Next, I examine the search rates separately for safety and investigation stops in Panel B and C. Sheriffs may have specific policies for different types of stops, creating heterogeneity. I find that, if anything, the search rates in safety stops increase in D-to-R counties after elections, and the increase seems to appear in all racial groups. Again, this potential increase in search rates should be considered as the impact of combining (a) the decreased share of safety stops (Column (2) in Table 5) and (b) any search behavior changes. Overall, there are no changes in search rate racial disparities associated with sheriff's party affiliation.

5.6 Efficiency

Finding contraband has long been considered an important part of a law enforcement agency's objective function. The unconditional hit rate, defined as the total search

[6] The thought exercise of holding the stopped driver's population the same is often evoked in papers that aim to explore the racial bias of officers in search behaviors (Antonovics and Knight, 2009) and to explore the efficiency of searches across racial groups (Feigenberg and Miller, 2022)

with found contraband divided by the total number of stops, can thus be seen as an efficiency measure of the law-enforcement agency's traffic stop performance. Slightly different from the search rate racial disparity versus unconditional hit rate trade-offs more commonly seen in the literature (Feigenberg and Miller, 2022), here, the trade-off is between the stop racial disparity and the unconditional hit rate.

Table 10 reports estimation results of specification 1 with unconditional hit rates as the outcome variables. Results in Column (1) in Panel A show that the overall unconditional hit rates do not change in D-to-R counties in post-election years. Although the unconditional hit rates in Black stops marginally significantly increase, especially in safety stops (Column (2) in Panel A and Panel B), the magnitude is not large enough to drive an increase in the overall unconditional hit rates.

Taking the results in Table 3 and Table 10 together, the newly elected Republican sheriffs enact policies that induce larger racial disparities in traffic stops without a discernible increase in the efficiency measured by the unconditional hit rates.

5.7 Long(er)-term impacts

In previous sections, I focus on the short-term impacts of partisan leadership, comparing traffic stop practices right after the elections with those before the elections. A natural request is to examine the long-term impact permitted by the research design restrictions. To this purpose, I extend the analysis period to four years before and after the elections and estimate the partisan leadership impacts with a specification similar to 1. The only difference is that one election cycle now contains eight years, so $e \in \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$, where 0 denotes the year the elections happened.

Two caveats should be kept in mind in the longer-term analysis. First, drivers may respond to the new traffic stop practices initiated by the new sheriffs in the longer term. 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 pressure for their next election. Among the counties included in the estimation sample, 40% of D-to-D counties have the winner's vote share in the next election larger than 80%, while 60% of D-to-R counties fall into such category. The parallel trend assumption thus may fail as the counties progress into the next elections. The number of county-cycles that satisfy the sample selection criterion (the number of stops is larger than 50 every year) decreases from 61 to 47 once I extend the election cycle to eight years.

I present the Black driver's share in the longer cycles in Figure 3. The gap between D-to-D and D-to-R groups decreases significantly right after the elections, the same pattern as in the shorter election cycles in Figure 1. Progress to the end of the election cycle, the gap widens to a similar level to pre-election periods. The increase in gaps is driven by D-to-R counties having a lower Black driver's share three and four years after the elections.

Table 11, Column (1) confirms the pattern seen in Figure 3. Black driver's share increases by 2.7 percentage points in D-to-R counties one year after the election compared to D-to-D counties. The magnitude of the estimate is similar for the year after, but the standard errors become larger. Three and four years after the election (or one and two year before the next election), The difference in Black driver's share in D-to-D and D-to-R counties become much smaller and are not statistically significantly different from differences in the baseline year ($t-1$). Weighting the observations by the number of stops each year increases the magnitude of the coefficients for all post-election periods (Column (2)), suggesting that some small agencies may drive the decrease in the magnitudes in Column (1). Unfortunately, the standard errors also become larger in Column (2), making the estimates non-significant. In future steps, it may be worthwhile to include R-to-R counties in the control group and see if the standard errors become smaller. The decrease in magnitudes in Column (1) can also not be explained by sheriff's offices responding to any policy changes in sheriff's departments. Column (4) shows that Black drivers' share of stops done by police officers in D-to-D and D-to-R counties exhibit a similar trend along the whole electoral cycle.

Overall, the long(er)-term results provide a caution to the interpretation of the results in section 5.1. The impact of partisan leadership on racial disparities in traffic stops may be short-lived. The short-lived impact is perhaps not surprising: law-enforcement leaders' policy choices may be influenced by temporal incentives over time, e.g., election pressure from the coming up elections. 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. Drivers may respond to the new traffic stop policies in a short period of time.

6 Tentative conclusion and future steps

I present evidence that partisan leadership affects traffic stop behaviors. A Democratic-to-Republican sheriff turnover, compared to a Democratic-to-Democratic turnover (may not involve sheriff turnover), leads to an increase of 3.2 percentage points in the black driver's share among all stops. Speaking to the recent policy proposals that aim at reducing racial disparities by changing the composition of traffic safety and investigation stops, I find evidence that most of the black driver' increase comes from changes *within* safety stops, rather than changes in the composition of safety and investigation stops. In relation to the importance of officer-level bias in determining racial disparities, I find evidence that the same set of officers can behave differently in their tendencies to stop Black drivers in response to leadership changes. In particular, I find evidence more consistent with the increase in the black driver's share driven by medium-level changes across a large set of officers, instead of drastic changes concentrated in a small set of officers (a few bad apples). With the limited amount of geographical information recorded in the dataset, I find no evidence that the increase in the black driver's share is driven by Republican sheriffs focusing on patrolling neighborhoods or at times of the day different from the previous sheriffs.

The increase in the racial disparities in traffic stops, however, does not come with an increase in the efficiency measured by the unconditional hit rates, despite that the Republican sheriffs seem to put more focus on crime investigation than traffic safety.

In future steps, motivated by the evidence that Republican sheriffs are associated with a decrease in the safety stops' share, I will explore if efficiency measures on traffic safety (e.g., number of fatality crashes) respond to the party affiliation of sheriffs.

References

M. Akhtari, D. Moreira, and L. Trucco. Political turnover, bureaucratic turnover, and the quality of public services. *American Economic Review*, 112(2):442–93, February 2022. doi: 10.1257/aer.20171867. URL https://www.aeaweb.org/articles?id=10.1257/aer.20171867.

K. Antonovics and B. G. Knight. A New Look at Racial Profiling: Evidence from the Boston Police Department. *The Review of Economics and Statistics*, 91(1):163–177, 02 2009. ISSN 0034-6535. doi: 10.1162/rest.91.1.163. URL https://doi.org/10.1162/rest.91.1.163.

F. R. Baumgartner, D. A. Epp, and K. S. Shoub. *Suspect Citizens: What 20 Million Traffic Stops Tell Us About Policing and Race*. Cambridge University Press, 2018.

G. Bulman. Law enforcement leaders and the racial composition of arrests. 57(4): 1842–1858, 2019. ISSN 0095-2583, 1465-7295. doi: 10.1111/ecin.12800. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/ecin.12800.

A. Cohen and C. S. Yang. Judicial politics and sentencing decisions. 11(1):160–191, 2019. ISSN 1945-7731, 1945-774X. doi: 10.1257/pol.20170329. URL https://pubs.aeaweb.org/doi/10.1257/pol.20170329.

E. Colonnelli, M. Prem, and E. Teso. Patronage and selection in public sector organizations. *American Economic Review*, 110(10):3071–99, October 2020. doi: 10.1257/aer.20181491. URL https://www.aeaweb.org/articles?id=10.1257/aer.20181491.

G. Facchini, B. Knight, and C. Testa. The franchise, policing, and race: Evidence from arrests data and the voting rights act, 2020. URL http://www.nber.org/papers/w27463.pdf.

B. Feigenberg and C. Miller. Would Eliminating Racial Disparities in Motor Vehicle Searches have Efficiency Costs?*. *The Quarterly Journal of Economics*, 137(1): 49–113, 05 2022. ISSN 0033-5533. doi: 10.1093/qje/qjab018. URL https://doi.org/10.1093/qje/qjab018.

M. D. Fliss, F. Baumgartner, P. Delamater, S. Marshall, C. Poole, and W. Robinson. Re-prioritizing traffic stops to reduce motor vehicle crash outcomes and racial disparities. *Injury Epidemiology*, 7(1):3, Dec. 2020. ISSN 2197-1714. doi: 10.1186/s40621-019-0227-6. URL https://injepijournal.biomedcentral.com/articles/10.1186/s40621-019-0227-6.

F. Goncalves and S. Mello. A few bad apples? racial bias in policing. *American Economic Review*, 111(5):1406–41, May 2021. doi: 10.1257/aer.20181607. URL https://www.aeaweb.org/articles?id=10.1257/aer.20181607.

P. Grosjean, F. Masera, and H. Yousaf. Inflammatory political campaigns and racial bias in policing. *The Quarterly Journal of Economics*, page qjac037, 2022. ISSN

0033-5533, 1531-4650. doi: 10.1093/qje/qjac037. URL https://academic.oup.com/qje/advance-article/doi/10.1093/qje/qjac037/6710386.

M. A. Hansen and J. C. Navarro. Gender and Racial Gaps in Support for Policing and Correctional Reforms: Are the Gaps a Consequence of Political Partisanship? *Crime & Delinquency*, page 001112872110647, Dec. 2021. ISSN 0011-1287, 1552-387X. doi: 10.1177/00111287211064788. URL http://journals.sagepub.com/doi/10.1177/00111287211064788.

S. R. Losak and M. D. Makowsky. Lame Duck Law Enforcement. 2022. URL https://ssrn.com/abstract=4350956.

J. McCrary. The effect of court-ordered hiring quotas on the composition and quality of police. *American Economic Review*, 97(1):318–353, March 2007. doi: 10.1257/aer.97.1.318. URL https://www.aeaweb.org/articles?id=10.1257/aer.97.1.318.

E. Pierson, C. Simoiu, J. Overgoor, S. Corbett-Davies, D. Jenson, A. Shoemaker, V. Ramachandran, P. Barghouty, C. Phillips, R. Shroff, and S. Goel. A large-scale analysis of racial disparities in police stops across the united states. *Nature Human Behaviour*, 4(7):736–745, 2020. ISSN 2397-3374. doi: 10.1038/s41562-020-0858-1. URL http://www.nature.com/articles/s41562-020-0858-1.

K. Roach, F. R. Baumgartner, L. Christiani, D. A. Epp, and K. Shoub. At the intersection: Race, gender, and discretion in police traffic stop outcomes. *Journal of Race, Ethnicity, and Politics*, 7(2):239–261, 2022. doi: 10.1017/rep.2020.35.

G. Solon, S. J. Haider, and J. M. Wooldridge. What are we weighting for? *Journal of Human Resources*, 50(2):301–316, 2015. ISSN 0022-166X. doi: 10.3368/jhr.50.2.301. URL https://jhr.uwpress.org/content/50/2/301.

D. M. Thompson. How partisan is local law enforcement? evidence from sheriff cooperation with immigration authorities. *American Political Science Review*, 114(1):222–236, 2020. doi: 10.1017/S0003055419000613.

Figures

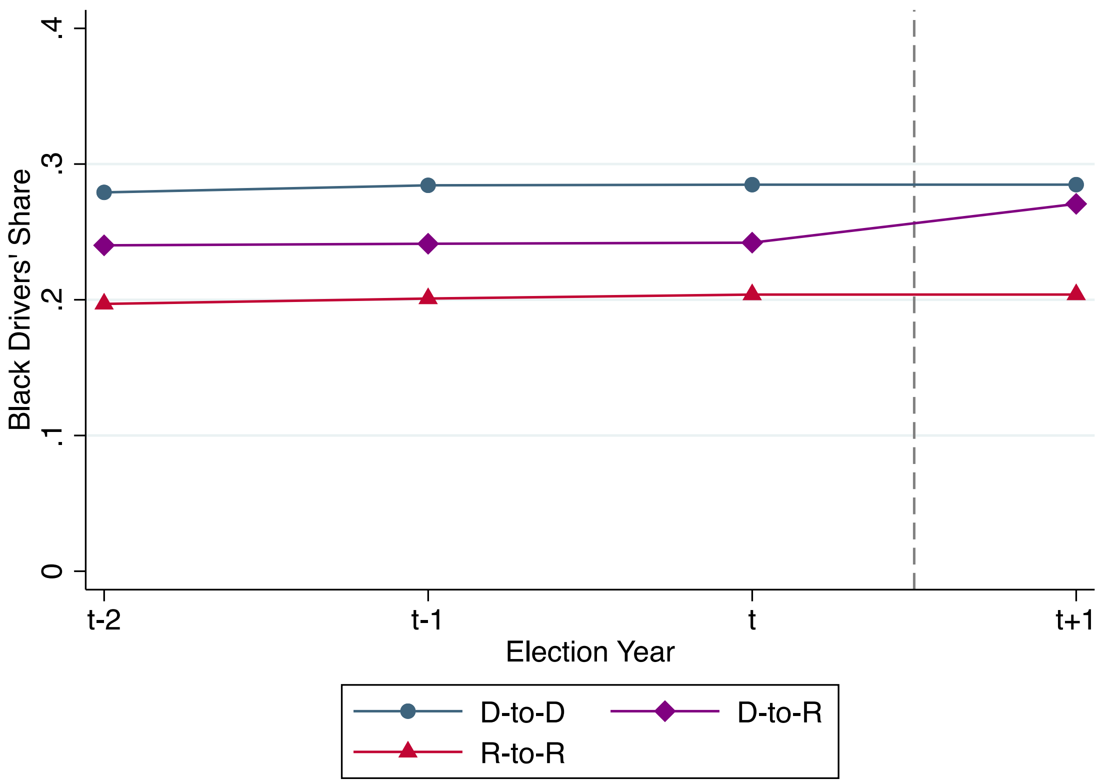

Figure 1: Black Drivers' Share Among All Stops

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, across the three election cycles. Each dot contains samples from three years.

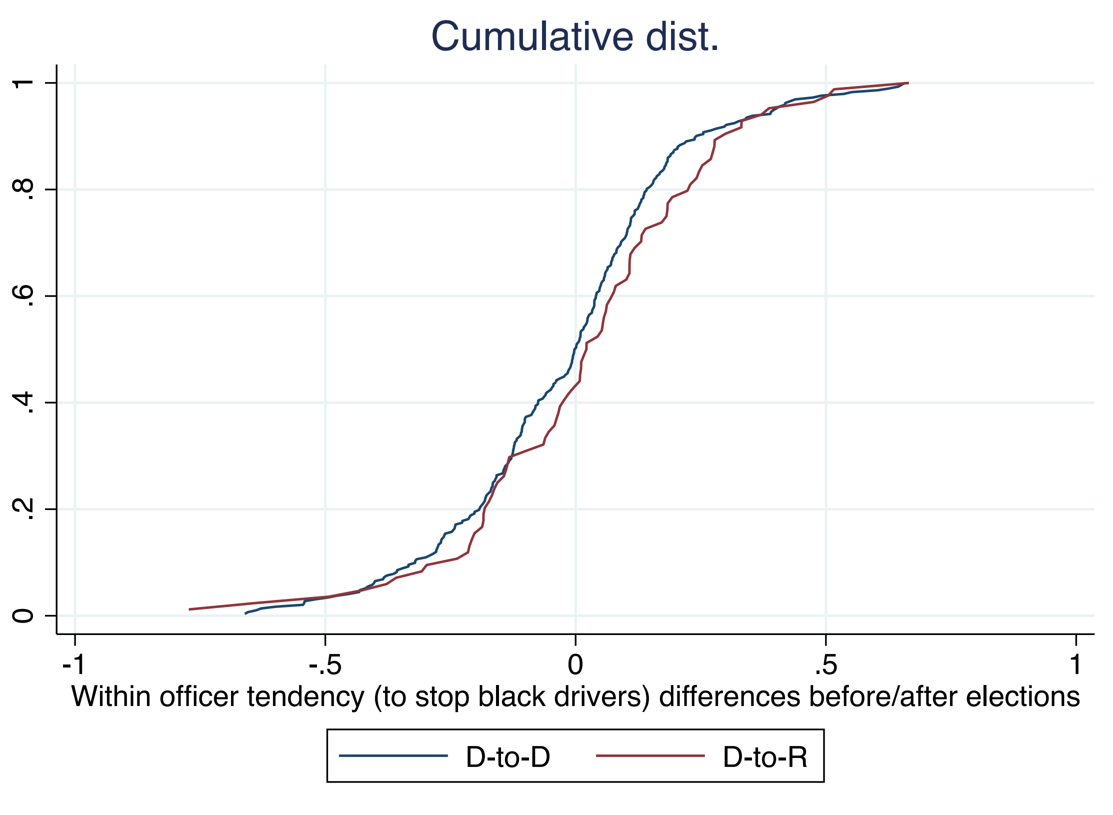

Figure 2: Cumulative distributions of the differences in the tendency of stopping black drivers before and after elections among stayers

Notes: This figure plots two cumulative distributions of the difference in the tendencies to stop black drivers before and after elections at the officer level, one for the officers in the D-to-D counties and one for the officers in the D-to-R counties. The tendency to stop black drivers is derived from two steps. First, I regress Black stop (one if the stop driver is black, zero otherwise) on stop location and stop time fixed effects, and get the residuals. Stop locations are counties or cities/towns. I divide a day into four time periods by three time points: 6 am, 12 am, and 6 pm. Stop time is quarter (four quarters in a year) $\times$ time period. Second, I compute the average of the residuals for each officer. Only stayers are included in this graph since I need the officers to conduct stops both before and after elections.

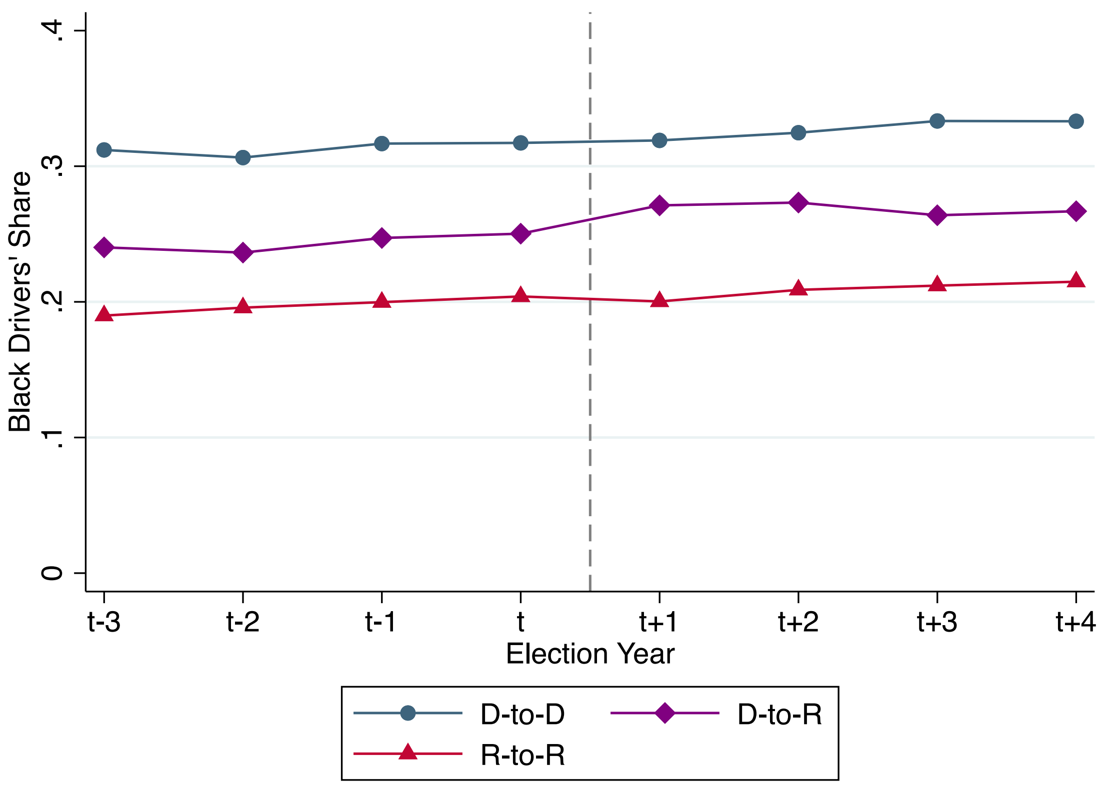

Figure 3: Black Drivers' Share Among All Stops in Longer Electoral Cycles

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, across the three election cycles. Each election cycle is eight year, four year before and after the elections. Each dot contains samples from three years.

Tables

Table 1: Sheriff Election Results in North Carolina

Panel A: All Sheriffs' Offices						
Election Year	R to R Turnover	R to R No Turnover	R to D	D to D Turnover	D to D No Turnover	D to R
2010	10	24	0	15	46	5
2014	5	33	1	14	37	10
2018	13	32	3	16	27	9
Panel B: Offices with Winners' vote share $<$ 80%						
2010	8	17	0	12	26	5
2014	3	16	1	8	21	9
2018	4	12	3	6	8	8
Panel C: 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	3	5
Panel D: 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	13	3	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 refers to the Republican party. North Carolina has 100 sheriff's offices, one for one county. Panel A presents the party turnover distributions in all elections from 2010 to 2018. Panel B drops elections in which the winner's vote share is smaller than 80%. This criterion is chosen to match the vote share support of D-to-R elections. Panel C drops elections that are dropped in Panel B and further drops the ones in which the county had at least one year that had fewer than 50 traffic stops in that four-year cycle (from 3 years before the election to 1 year after the election). Panel D presents the winner's vote share distribution in all D-to-D (turnover and no turnover) and D-to-R elections. An election with the winner's vote share being one means there was only one candidate in that election. We use county-cycles in Panel B for general personnel analysis. We use county-cycles in Panel C in traffic stop policing analysis.

Table 2: Summary Statistics of Traffic Stops and Searches

	Stops by Motorists' Group			Stops by Types		All
	Black	Hispanic	White	Safety	Investigation	
Share Black	1.000	0.000	0.000	0.238	0.278	0.257
Share Hispanic	0.000	1.000	0.000	0.068	0.070	0.069
Share White	0.000	0.000	1.000	0.669	0.634	0.652
Share Female	0.361	0.239	0.359	0.357	0.343	0.350
Share Safety Stops	0.478	0.511	0.530	1.000	0.000	0.517
Share Investigatory Stops	0.522	0.489	0.470	0.000	1.000	0.483
Search Rate	0.079	0.087	0.061	0.051	0.085	0.067
Unconditional Hit Rate	0.024	0.017	0.021	0.016	0.027	0.022
Observations	84,595	22,600	214,132	169,809	158,730	328,539

Notes: This table presents summary statistics including all county-cycles included in Panel C in Table 1. All stops can be categorized into safety or investigatory stops. Safety stops includes stops due to Speed Limit Violation, Stop Light/Sign Violation, Driving While Impaired, Safe Movement Violation. Investigatory stops include stops due to Vehicle Equipment Violation, Vehicle Regulatory Violation, Seat Belt Violation, Investigation, and Other Motor Vehicle Violation.

Table 3: Impact of partisan sheriffs on black driver's share

	$\frac{\text{\# of black driver}}{\text{\# of all stops}}$			
	(1)	(2)	(3)	(4)
	Sheriff's offices			Police departments
t-2 x D-to-R	0.0080	0.0094	-0.0196	-0.0123
	(0.0173)	(0.0082)	(0.0277)	(0.0120)
t x D-to-R	0.0007	0.0084*	-0.0116	-0.0018
	(0.0082)	(0.0049)	(0.0146)	(0.0137)
t+1 x D-to-R	0.0326**	0.0312*	0.0319	0.0039
	(0.0151)	(0.0172)	(0.0230)	(0.0145)
County-Cycle	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Weight	Agency	# of stops	Agency	Agency
Sample	All	All	Close election	All
N	244	244	104	164
Dep. mean	0.2413	0.1878	0.2425	0.2293

Notes: Clustered standard errors at the county level in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All outcome variables are at county-year level. t refers to the year of election in that election cycle. I report the coefficients of the interaction terms between the (reletative) election year dummy variables with the D-to-R dummy variable. 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)-(3) reports regression results with traffic stop samples from sheriff's offices. Column (4) reports results with samples from police departments in the same set of counties as in Columns (1) and (2). The sample size is smaller in Column (4) because not all counties have police departments. All regression specifications include county-cycle and election-year fixed effects. I weight the county-year observations by the number of stops of that county in $t-2$ in Column (2). In Column (3), I restrict the samples to counties where the winner's vote share is below 60%. Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Table 4: Impact of Partisan Sheriffs on the Number of Stops by Race and Stop Purposes

	ln(number of stops)			
	(1)	(2)	(3)	(4)
	Black	Non-Black	Diff b/w races	All stops
t-2 x D-to-R	-0.211	-0.198	-0.194	
	(0.173)	(0.148)	(0.145)	
t x D-to-R	-0.437**	-0.505***	-0.495***	
	(0.178)	(0.170)	(0.169)	
t+1 x D-to-R	0.183	0.0032	0.0137	
	(0.288)	(0.279)	(0.276)	
t-2 x DtoR x Black			-0.0210	
			(0.0944)	
t x DtoR x Black			0.0481	
			(0.0584)	
t+1 x DtoR x Black			0.158*	
			(0.0918)	
t-2 x Close				-0.490***
				(0.153)
t x Close				-0.126
				(0.166)
t+1 x Close				-0.0112
				(0.217)
County-Cycle	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
N	244	244	488	244
Average # of stops	268	1042		1336

Notes: Clustered standard errors at the county level are in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. I report the coefficients of the interaction terms between the (relative) election year dummy variables with the D-to-R dummy variable in equation 1 in Columns (1)-(2). 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 (3) reports the regression estimation results from specification **??**, where Black is a dummy variable being one if the county-year observation is the number of stops on Black drivers, 0 otherwise. Column (4) reports estimation results with specification 1 with the log of the number of all stops as the outcome variable. I include the same county-cycles as in Column (1) in the estimation reported in Column (4). Close is a dummy variable being one if the county experienced an election in which the winner's vote share is below 60%, 0 otherwise. The average number of stops is computed from D-to-R (Close election) counties in year $t-1$, one year before the sheriff election.

Table 5: Decomposition of the changes in black driver's share: type of traffic stops

	(1)	(2)	(3)	(4)	(5)	(6)
	$\frac{\text{All Black Stops}}{\text{All Stops}}$	$\frac{\text{All Safety Stops}}{\text{All Stops}}$	$\Delta S_{i,(-1,t)}(B_{1i,-1} - B_{2i,-1})$	$S_{i,-1}\Delta B_{1i,(-1,t)}$	$(1 - S_{i,-1})\Delta B_{2i,(-1,t)}$	$\Delta S_{i,(-1,t)}(\Delta B_{1i,(-1,t)} - \Delta B_{2i,(-1,t)})$
			Changes in the share of safety stops	Changes within safety stops	Changes within investigation stops	Second order changes
t-2 x D-to-R	0.0080	-0.0075	-0.0025	0.0062	0.0039	0.0005
	(0.0173)	(0.0288)	(0.0029)	(0.0076)	(0.0091)	(0.0014)
t x D-to-R	0.0007	-0.0337*	0.0026*	0.0026	-0.0044	-0.0000
	(0.0082)	(0.0188)	(0.0014)	(0.0049)	(0.0066)	(0.0013)
t+1 x D-to-R	0.0326**	-0.0882***	0.0054**	0.0224**	0.0072	-0.0023
	(0.0151)	(0.0234)	(0.0022)	(0.0111)	(0.0107)	(0.0034)
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
N	244	244	183	183	183	183
Dep. mean	0.2413	0.5281	0	0	0	0

Notes: Columns (1) and (2) in the table report estimation coefficients from an OLS regression with specification as in equation 1. Column (3)-(6) reports estimation coefficients from an OLS regression with specification as in equation 3 in the Appendix. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. 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 safety and investigation stops for county i in year t. There are four time periods, $t = -2, -1, 0, 1$. We set $t = -1$ as the baseline period. We denote S_{it} as the share of safety stops of all stops. Then $1 - S_{it}$ is the share of investigation stops of all stops. We 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 black driver's share from changes in the share of safety and investigation type of stops of all stops (while keeping the black driver's share in each type of stop constant). Columns (4) and (5) represent the contribution from changes in the black drivers' share 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 driver's share in safety and investigation stops). See the Appendix for the derivation of the decomposition. Clustered standard errors at the county level are in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dep. mean computed from D-to-R counties in year $t - 1$.

Table 6: Decomposition of the changes in black driver's share: officer

	(1)	(2)	(3)	(4)	(5)	(6)
	$\frac{\text{All Black Stops}}{\text{All Stops}}$	$\frac{\text{All Stayer Stops}}{\text{All Stops}}$	$\Delta S_{i,(-1,t)}(B_{1i,-1} - B_{2i,-1})$	$S_{i,-1}\Delta B_{1i,(-1,t)}$	$(1 - S_{i,-1})\Delta B_{2i,(-1,t)}$	$\Delta S_{i,(-1,t)}(\Delta B_{1i,(-1,t)} - \Delta B_{2i,(-1,t)})$
			Changes in the share of stayer stops	Changes within stayer stops	Changes within non-stayer stops	Second order changes
t-2 x D-to-R	0.0080	-0.0246	-0.0013	0.0196	-0.0052	-0.0049
	(0.0173)	(0.0352)	(0.0035)	(0.0133)	(0.0081)	(0.0061)
t x D-to-R	0.0007	-0.0050	-0.0003	0.0032	-0.0078	0.0056
	(0.0082)	(0.0762)	(0.0092)	(0.0062)	(0.0084)	(0.0137)
t+1 x D-to-R	0.0326**	-0.191**	0.0088	0.0258*	0.0131	-0.0151
	(0.0151)	(0.0822)	(0.0104)	(0.0147)	(0.0095)	(0.0115)
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
N	244	244	183	183	183	183
Dep. mean	0.2413	0.5520	0	0	0	0

Notes: Columns (1) and (2) in the table report estimation coefficients from an OLS regression with specification as in equation 1. Column (3)-(6) reports estimation coefficients from an OLS regression with specification as in equation 3 in the Appendix. Clustered standard errors at the county level are in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. Dep. mean computed from D-to-R counties in year $t - 1$. 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, $t = -2, -1, 0, 1$. We set $t = -1$ as the baseline period. We denote S_{it} as the share of stops done by stayers. Then $1 - S_{it}$ is the share of stops done by non-stayers. We 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 black drivers' share 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.

Table 7: Officer Behavior Change and Personnel Turnover

	(1)	(2)	(3)	(4)
	$\frac{\text{Black Stops by Stayers}}{\text{All Stops by Stayers}}$	$\frac{\text{Black Stops by Non-Stayers}}{\text{All Stops by Non-Stayers}}$	$\frac{\text{\# of non-stayers}}{\text{\# of all officers}}$	$\frac{\text{\# of new officers}}{\text{\# of all officers}}$
t-2 x D-to-R	0.0454	-0.0360	-0.00735	-0.00261
	(0.0303)	(0.0399)	(0.0316)	(0.0592)
t x D-to-R	0.00951	-0.0306	0.0265	0.0394
	(0.0130)	(0.0311)	(0.0457)	(0.0589)
t+1 x D-to-R	0.0403^{**}	-0.00510	0.167^{***}	0.217^{***}
	(0.0194)	(0.0323)	(0.0579)	(0.0603)
County-Cycle	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
N	244	244	244	244
Dep. mean	0.2294	0.2661	0.6125	0.3756

Notes: This table reports regression estimation results with specification 1 with four outcome variables listed at the head of the table. Stayers are officers who conduct traffic stops both before and after elections. Non-stayers are officers who conduct traffic stops either before or after elections. An officer is a new officer in that year if his/her first traffic stop record in that agency is observed in that year. 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. Clustered standard errors at the county level are in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dep. means are computed from D-to-R counties one year before the election.

Table 8: Patrol Location and Time Policy

	(1)	(2)	(3)	(4)	(5)	(6)
			Predicted Black stops			
			Stops			
	All		Safety Stops		Investigatory	
	Location	Time	Location	Time	Location	Time
t-2 x DtoR	0.00307	-0.00125	0.000880	-0.00234	0.00331	0.00317
	(0.00476)	(0.00453)	(0.00395)	(0.00497)	(0.00672)	(0.00417)
t x DtoR	-0.000966	0.000437	0.000549	0.00184	-0.00301	-0.000249
	(0.00369)	(0.00347)	(0.00415)	(0.00370)	(0.00411)	(0.00419)
t+1 x DtoR	0.00505	-0.00212	0.00685	-0.00288	0.000816	-0.00327
	(0.00439)	(0.00410)	(0.00451)	(0.00417)	(0.00512)	(0.00460)
County-Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
N	244	244	244	244	244	244
dep_mean	0.2417	0.2401	0.2396	0.2352	0.2444	0.2462

Notes: Clustered standard errors at the county level in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dep. mean computed from D-to-R counties before the election. For Columns (1), (3), and (5), we predict whether the stop is a Black stop by the share of Black stops pre-election in each location cell. Locations are places where at least 40 traffic stops were recorded under that place name in the estimation sample. For Columns (2), (4), and (6), we predict whether the stop is a Black stop by the share of Black stops pre-election in each time group x county cell. A day is divided into four time groups by four points: 6 am, noon, 6 pm, midnight.

Table 9: Effect of Partisan Leadership on Search Rates by Drivers' Race

Panel A: All stops	(1) All searches / All stops	(2) Black searches / Black stops	(3) Non-black searches / Non-black stops
t-2 x DtoR	0.0107	-0.0109	0.0169
	(0.0122)	(0.0176)	(0.0123)
t x DtoR	-0.00139	-0.0216	0.00214
	(0.00930)	(0.0234)	(0.00973)
t+1 x DtoR	0.0177	0.0331	0.0168
	(0.0156)	(0.0246)	(0.0157)
Dep. mean	0.0832	0.1102	0.0768
Panel B: Safety stops			
t-2 x DtoR	0.00871	-0.00472	0.00472
	(0.0175)	(0.0315)	(0.0179)
t x DtoR	-0.00625	-0.0333	-0.00587
	(0.0109)	(0.0232)	(0.0114)
t+1 x DtoR	0.0344*	0.0500	0.0263
	(0.0180)	(0.0354)	(0.0179)
Dep. mean	0.0788	0.0982	0.0723
Panel C: Investigation stops			
t-2 x DtoR	0.0124	-0.0224	0.0260*
	(0.0119)	(0.0178)	(0.0144)
t x DtoR	0.00397	-0.00748	0.0122
	(0.0130)	(0.0302)	(0.0141)
t+1 x DtoR	0.00310	-0.000444	0.00853
	(0.0174)	(0.0313)	(0.0183)
Dep. mean	0.1045	0.1038	0.1078
N	244	244	244
County-Cycle FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses.Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. Dep. means are computed from D-to-R counties in year $t - 1$, one year before the sheriff election.

Table 10: Effect of Partisan leadership on unconditional hit rates by drivers' race

Panel A: All stops	(1) All contraband All stops	(2) Black contraband Black stops	(3) Non-Black contraband Non-black stops
t-2 x D-to-R	0.0103	0.0169	0.0105
	(0.00625)	(0.0102)	(0.00637)
t x D-to-R	-0.00285	0.00629	-0.00178
	(0.00563)	(0.0145)	(0.00702)
t+1 x D-to-R	0.00746	0.0181*	0.00578
	(0.00824)	(0.0101)	(0.0102)
Dep. mean	0.0304	0.0337	0.0296
Panel B: Safety stops			
t-2 x D-to-R	0.0154*	0.0109	0.0124
	(0.00847)	(0.0186)	(0.00915)
t x D-to-R	-0.00616	-0.00639	-0.00683
	(0.00629)	(0.0114)	(0.00764)
t+1 x D-to-R	0.0169**	0.0249*	0.0109
	(0.00808)	(0.0137)	(0.0111)
Dep. mean	0.0244	0.0257	0.0247
Panel C: Investigation stops			
t-2 x D-to-R	0.00671	0.0117	0.0105
	(0.00866)	(0.0116)	(0.0102)
t x D-to-R	0.00141	0.0148	0.00635
	(0.00854)	(0.0208)	(0.0108)
t+1 x D-to-R	-0.000221	0.0117	0.00144
	(0.0109)	(0.0137)	(0.0151)
Dep. mean	0.0376	0.0428	0.0354
N	244	244	244
County-Cycle FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Clustered standard errors at the county level are in parentheses.Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election. Contraband refers to searches that found contraband successfully.

Table 11: Longer-term Effect of Partisan Leadership on Black Driver's Share

	$\frac{\text{\# of black driver}}{\text{\# of all stops}}$			
	(1)	(2)	(3)	(4)
	Sheriff's offices			Police departments
t-3 x D-to-R	-0.0006	0.0106	-0.0173	0.0101
	(0.0119)	(0.0098)	(0.0159)	(0.0098)
t-2 x D-to-R	0.0039	0.0051	-0.0250	-0.0175
	(0.0186)	(0.0072)	(0.0248)	(0.0156)
t x D-to-R	0.0031	0.0070*	-0.0013	0.0021
	(0.0082)	(0.0038)	(0.0137)	(0.0240)
t+1 x D-to-R	0.0278**	0.0312	0.0091	0.0096
	(0.0122)	(0.0193)	(0.0146)	(0.0258)
t+2 x D-to-R	0.0262	0.0300	-0.0013	-0.0130
	(0.0194)	(0.0200)	(0.0241)	(0.0205)
t+3 x D-to-R	0.0061	0.0269	-0.0228	-0.0032
	(0.0170)	(0.0194)	(0.0171)	(0.0258)
t+4 x D-to-R	0.0106	0.0146	0.0031	-0.0028
	(0.0126)	(0.0198)	(0.0166)	(0.0239)
County-Cycle	Yes	Yes	Yes	Yes
Year-Cycle	Yes	Yes	Yes	Yes
Weight	Agency	# of stops	Agency	Agency
Sample	All	All	Close election	All
N	376	376	144	232
Dep. mean	0.2471	0.1720	0.2446	0.2867

Notes: Clustered standard errors at the county level are in parentheses. Statistical significance is denoted: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All outcome variables are at the county-year level. t refers to the year of election in that election cycle. Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Appendix

Decomposition of the Total Changes in the Black Driver's Share

Let B_{it} denote the share of black driver's of all stops for county i in year t. Following the timing convention in this paper, $t = -2, -1, 0, 1$, we 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. We denote B_{1it} and B_{2it} as the share of black drivers of all safety and investigation stops. We can then write:

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

Re-writing 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:

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

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

To see that the estimation results for the coefficients of interest are the same no matter whether we have the difference between two periods or the level in the year as outcome variables, we duplicate equation 1 below:

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

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

$$Y_{cle} - Y_{cl,-1} = \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot (\eta_e - \eta_{-1}) + (\delta_{le} - \delta_{l,-1}) + (\epsilon_{cle} - \epsilon_{cl,-1}). \tag{3}$$

Hence, we can use the terms in the four brackets above as outcome variables, estimate four regressions with specifications 3 (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 black driver's share as outcome variables.

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.