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

Wei-Lin Chen*

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Abstract

I study the impact of partisan leaders on traffic stop policing practices 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 3.8 percentage points, a 15.7% 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.

*New Zealand Policy Research Institute, Auckland University of Technology. wei-lin.chen@aut.ac.nz. I am grateful to Julie Cullen, Paul Niehaus, Sam Bazzi, Gordon Dahl, Jeffrey Clemens, and Craig McIntosh for their support and helpful comments. I thank David Arnold, Prashant Bharadwaj, Tanner Eastmond, Jinhyeon Han, Sam Krumholz, and seminar participants at UCSD, WEAI Economics of Crime Workshop, Taiwan Economics Research Workshop, Victoria University of Wellington, and New Zealand Association of Economists Annual Conference for their feedback.

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 of leaders 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 as 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 practices. 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 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, I 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 me to stack up data from 3 election cycles without having overlapping timing periods.

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). The estimates are robust to a weaker parallel trend assumption, assuming the parallel trend holds conditional on the urban categories of the counties. In addition, I find no changes in Black drivers' share in the stops in the same counties but done by officers in the corresponding police departments.

To investigate which new policies and instruments the Republican sheriffs adopt 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 a law enforcement agency should put on each stop type is under debate in North Carolina. 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 in a context where the Sheriff was presented with information that Black drivers are disproportionately affected by investigation traffic stops.[1] Fliss et al. (2020) used a synthetical control method and found that the safety-focused stop policy in Fayetteville reduces traffic crashes and injuries and decreases the share of Black drivers in traffic stops. This evidence prompts me to examine the importance of partisan leaders' potential differences in focus across stop types in explaining the increase in the black driver's share.

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

[1]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.

only account for 12% of the Black driver's share increase. 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 63% of the overall change in Black drivers' share. In understanding racial disparities in stops, researchers have focused on investigation stops, in which officers are thought to have more discretion and are likely to exhibit racial bias. My results suggest that 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) change in incumbent officers' stop practices in response to the new leadership. I find evidence supporting the second 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% decrease compared to baseline) lower than in D-to-D counties in post-election years. 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, an 18% increase compared to the baseline. Further, I find that the increase in the Black driver's share among incumbent officers is not driven by a few officers but by many officers having medium-level changes in the tendency to stop Black drivers. The evidence suggests that the reshuffling of officers does not lead to systematic changes in the observed policy practices. Still, the leaders reshape policy practices in a way that changes incumbent officers' conduct.

Next, I analyze officers' decisions 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 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. I find no significant impacts of sheriff's party affiliation on the overall and within racial group search rates.

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). Since finding contraband is one of the goals in conducting traffic stops, a 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.

Finally, I examine the long-term impact. I find that the effect of the 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 four years. In addition, 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 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 preferences 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., 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 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 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 assignments are not 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 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 top law enforcement agencies in counties. They perform duties in unincorporated areas within counties, while 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.[2] Police officers conduct many more stops than deputy sheriffs. From 2008 to 2019 (my sample period), on average, deputy sheriffs conducted about a hundred thousand stops yearly, while police officers conducted about six hundred seventy thousand.

[2]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 personnel size in each agency as the weights.

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 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, and the deputies also take their oath on the same day. Since 1998, all elected sheriffs have been affiliated with either the Democratic Party or the Republican Party. I use sheriffs' turnovers induced by elections as the main variation of change of control. In particular, I focus on sheriff's turnovers that involve party turnovers. Police chiefs, who are the leaders of the police departments, on the other hand, are appointed by the municipal government.

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 non-moving 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.

By law, officers can search a vehicle as long as the officers have probable cause to believe that a law has been broken. This is a decision in that officers have much discretionary power. 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 the sheriff's election record to analyze the effect of sheriffs' party affiliation on officers' traffic stop and search behaviors.

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 election websites. 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.

Table 1 reports the sheriff election results from 2010 to 2018. 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 elections. I do not focus on comparing county-election cycles with Republican-to-Democrat elections and those with Republican-to-Republican turnover elections because the relevant sample size is small (10 for R-to-R and 3 for R-to-D).

Panel D of Table 1 shows the winners' vote share distribution. All Democrat-to-Republican elections have winners' vote shares of less than 80%. To match the control and treatment groups on the winners' vote shares, I confine my sample to the county-cycles where the winners' vote shares are less than 80%. Panel B shows the number of county-cycles in each election type after I apply this restriction.

The last sample restriction is about the number of stops each year within an election cycle. I exclude the county-cycles where a sheriff's office reports less than 50 stops in at least an election year within the four-year election cycle. Two reasons for this criterion. First, the decomposition analysis in section 5.2 and 5.3 would not make sense if the number of stops within certain types (safety and investigation stops) and by certain officers (incumbent officers who conduct traffic stops before and after elections and others) is tiny. Second, I aim to select sheriff's offices that have consistent "report" quality across the years. Some counties excluded by this restriction have considerable 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 of stops 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. Panel C of table 1 presents the resulting number of county-cycles of each election type. Figure 1 plots the maps of the D-to-D and D-to-R counties included in Panel C.

3.2 County and Sheriff Characteristics

Party turnovers of sheriffs may correlate with county and other sheriff characteristics. I thus examine in Table 3 the county and sheriff characteristics of the county-election cycles included in Panel C, Table 1. Note that a county may appear multiple times across rows. For example, a county that experienced a D-to-D election in 2010, a D-to-R election in 2014, and an R-to-R election in 2018 would be included in the samples in the columns D-to-D, D-to-R, and R-to-R.

Panel A in Table 3 presents the urban categories and population characteristics. Examination of urban categories is important in analyzing traffic stop practices because driving and crime behaviors may exhibit different time trends across urban and rural counties. If D-to-D and D-to-R county-cycles do not overlap in the same urban categories, the parallel trend assumption may not hold. I use the urban classifications 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 metro includes counties with MSAs of 50,000 to 999,999 population. Nonmetropolitan includes the other counties. D-to-D and D-to-R counties overlap in the three urban classifications, but D-to-D counties are more urban than D-to-R counties. Forty-six percent of D-to-D counties are nonmetropolitan (22 out of 47), while sixty percent of D-to-R counties are nonmetropolitan (9 out of 15). The urban features of D-to-D counties are reflected in the population characteristics. The D-to-D counties have a higher share of Black people and college-educated people. The population characteristics are population-weighted averages derived from county-level data from the 2010, 2014, and 2018 American Community Survey accessed via NHGIS.

Panel B in Table 3 shows how correlated race and gender are with the party affiliation of sheriffs. Most elections result in white-to-white and male-to-male sheriff transitions, regardless of the party transition types of elections. Among the 54 unique sheriffs in D-to-D and D-to-R elections, two are women, and eleven are Black. All women and Black sheriffs are Democrats. On average, D-to-R elections compared to D-to-D elections are, in turn, associated with a *decrease* in the female share from 6% to 0 and a *decrease* in the Black share from 27% to 0.

3.3 Traffic Stop and Search Records.

We obtain the traffic stop and search records from the North Carolina State Bureau of Investigation database. The data set 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 ones associated with speed limit violations, stop light/sign violations, driving while impaired, and safe movement violations. Investigation stops include ones 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.[3] 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. With this categorization, I construct the variables the share of stops done by stayers and the share of Black drivers in the stops done by stayers or non-stayers.

The data set includes the time and the location of each stop. The location can be a county, a city/town, a census-designated place (CDP), or some local location names. Around 60% of the stops only record the location at the county level, which significantly restricts our analysis of officers' patrolling location decisions.

The dataset also has information about searches and contraband. I use information about whether a search is conducted in a traffic stop and whether contraband is found during a search to construct two outcome variables. 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 2 presents the summary statistics of traffic stops and searches in the D-to-D and D-to-R county-cycles in Panel C, Table 1. I report descriptive shares on race,

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

gender, and traffic stop types. The driver is female in 35% of the stops, black in 26% of the stops, Hispanic in 7% of the stops, and white in 65% of stops. [4] Due to the small share of Hispanic drivers, I divided the drivers into Black and non-Black groups in the regression analysis.[5] 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 Empirical Methods

I aim to identify the causal effect of the sheriff's party affiliation on traffic stop practices. To this end, I adopt a difference-in-differences design, comparing counties that experience elections resulting in Democrat-to-Democrat transitions with counties that experience Democrat-to-Republican transitions. In the main analysis, 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.

I estimate the average treatment effect on the treated (henceforth ATT) with two steps. First, I estimate an ordinary least square regression with a difference-in-differences type specification. In particular, I estimate election cycle specific effects by posting a saturated model of treatment status, election years, and election cycles

[4] 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.

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

dummy variables, with county cycle and calendar year fixed effects:

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

where Y_{cle} is a variable at county-year level for county c in year e in cycle l. 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. In tables 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 $e = -2, -1, 0, 1$. Since the new sheriff is sworn in on the first Monday in December, I define a year starting from December to November. For example, the year t ($e = 0$) in the 2010 election cycle involved stops from December 2009 to November 2010. Hence, δ_{le} uniquely defines the timing of each stop in year e in cycle l. I called δ_{le} calendar year fixed effects in this paper. 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 am interested in the causal effect of leadership on law enforcement agencies.

In the second step of estimating ATT, I aggregate the election cycle specific estimates into one estimate based on the empirical distribution of calendar years in the D-to-R elections. Specifically, I report the following estimates:

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

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 thus respectively $\frac{4}{15}, \frac{6}{15}, \frac{5}{15}$ (see the empirical distribution of calendar years in Panel C, Table 1). [6] I estimate standard

[6]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 Appendix in Gardner (2021)

errors clustered at the county level throughout the paper. In the results section, I also report the estimation results from a regression weighting the county-year observations with the number of stops in each county at the beginning of the election cycle ($t - 3$).

The difference-in-differences model relies on the parallel trend assumption. In this paper's context, the parallel trend assumption is that the outcome variable (e.g., the black driver's share among all stops) in the D-to-R counties would exhibit the same time trend as the D-to-D counties after elections if the Republican candidates did not win the elections. The parallel trend assumption is not directly testable, but one can report estimates based on different versions of parallel trend assumptions to probe the robustness of the estimates. In the context of traffic stop practice, one may particularly worry that the parallel trend assumption will not hold because the D-to-D counties are more urban than D-to-R counties. I therefore report a set of estimates based on a potentially weaker *conditional* parallel trend assumption, which assumes that the parallel trend assumptions hold *conditional on the urban categories* reported in Panel A, Table 3. I follow the estimation procedures recommended in Wooldridge (2021) and proceed in two steps. First, I estimate the following regression, which is a saturated model of the treatment status, election years, urban categories, and election cycles dummy variables, with county-cycle and calendar year-urban category fixed effects:

$$
\begin{aligned}
Y_{cle} = & \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \sum_{u=2}^{3} \beta_{leu} D_{cl}^{D-to-R} \cdot \eta_e \cdot \eta_u \cdot \eta_l + \\
& \sum_{e=-2}^{1} \sum_{u=2}^{3} \beta_{eu} D_{cl}^{D-to-R} \cdot \eta_e \cdot \eta_u + \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \beta_{le} D_{cl}^{D-to-R} \cdot \eta_e \cdot \eta_l + \\
& \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot \eta_e + \delta_{le} + \delta_{cl} + \epsilon_{cle}
\end{aligned} \tag{3}
$$

where $u \in \{1, 2, 3\}$ represents the urban categories. Other notations are denoted in the same way as in equation 1. Second, I aggregate the estimates into ATT estimates based on the empirical joint distribution of urban categories and calendar years in

D-to-R county-cycles. Specifically, I report

$$\begin{aligned}
\beta_e^{U*} =& \frac{\omega_{2010,u=1}^{D-to-R}}{\sum_{l=2010}^{2018}\sum_{u=1}^{3}\omega_{lu}^{D-to-R}} \cdot \beta_e + \\
& \sum_{l=2014}^{2018}\{\frac{\omega_{l,u=1}^{D-to-R}}{\sum_{l=2010}^{2018}\sum_{u=1}^{3}\omega_{lu}^{D-to-R}}(\beta_e + \beta_{le})\} + \\
& \sum_{u=2}^{3}\{\frac{\omega_{l=2010,u}^{D-to-R}}{\sum_{l=2010}^{2018}\sum_{u=1}^{3}\omega_{lu}^{D-to-R}}(\beta_e + \beta_{eu})\} + \\
& \sum_{l=2014}^{2018}\sum_{u=2}^{3}\{\frac{\omega_{lu}^{D-to-R}}{\sum_{l=2010}^{2018}\sum_{u=1}^{3}\omega_{lu}^{D-to-R}}(\beta_{leu} + \beta_{eu} + \beta_{le} + \beta_e)\} \qquad , \\
e =& -2, 0, 1
\end{aligned} \tag{4}$$

where ω_{lu}^{D-to-R} is the number of D-to-R county-cycles in the urban category u in election cycle l.

I examine changes in racial disparities by looking at the share of Black drivers among stops and the search rates within racial groups. When I analyze the efficiency of traffic stop practices, I look at the unconditional hit rates in the overall stops and within racial groups. Specification 1 is appropriate for such analysis.

In section 5.1, I compare the change in the number of stops across racial groups. I also examine if the partisan leadership has a different impact on the number of stops when the elections are close and when the incumbent candidates participate in the elections. There, I estimate the following regression, a saturated model of treatment status, election years, groups, and election cycles dummy variables, with county cycle and calendar year fixed effects:

$$\begin{aligned}
Y_{cleg} =& \sum_{l=2014}^{2018}\sum_{e=-2}^{1}\gamma_{el}^{1}D_{cl}^{D-to-R}\cdot\eta_e\cdot G_g\cdot\eta_l + \sum_{e=-2}^{1}\gamma_e^1 D_{cl}^{D-to-R}\cdot\eta_e\cdot G_g + \\
& \sum_{l=2014}^{2018}\sum_{e=-2}^{1}\gamma_{le}^{0}D_{cl}^{D-to-R}\cdot\eta_e\cdot\eta_l + \sum_{e=-2}^{1}\gamma_e^0 D_{cl}^{D-to-R}\cdot\eta_e + \\
& D_{cl}^{D-to-R}\cdot G_g + \eta_l\cdot G_g + \eta_e\cdot G_g + G_g + \delta_{le} + \delta_{cl} + \epsilon_{cle},
\end{aligned} \tag{5}$$

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*}):

$$\begin{aligned}
\gamma_e^{0*} =& \frac{\omega_{2010}^{D-to-R}}{\sum_{l=2010}^{2018}\omega_l^{D-to-R}} \cdot \gamma_e^0 + \sum_{l=2014}^{2018}\{\frac{\omega_l^{D-to-R}}{\sum_{l=2010}^{2018}\omega_l^{D-to-R}}(\gamma_e^0 + \gamma_{le}^0)\}, \\
\gamma_e^{1*} =& \frac{\omega_{2010}^{D-to-R}}{\sum_{l=2010}^{2018}\omega_l^{D-to-R}} \cdot \gamma_e^1 + \sum_{l=2014}^{2018}\{\frac{\omega_l^{D-to-R}}{\sum_{l=2010}^{2018}\omega_l^{D-to-R}}(\gamma_e^1 + \gamma_{le}^1)\}, \\
e =& -2, 0, 1.
\end{aligned} \tag{6}$$

5 Results

I first present results on racial disparities in the share of Black drivers in traffic stops in section 5.1. I then present decomposition analysis along two dimensions of traffic stops: types of traffic stops (safety and investigation) in section 5.2 and identity of officers (stayers or non-stayers) in section 5.3. I then analyze the second stage during a traffic stop process: the searches in section 5.5. After examining racial disparities, I investigate in section 5.6 if a change in the efficiency of traffic stops, measured by the unconditional hit rates, is accompanied by changes in racial disparities. Finally, section 5.7 discusses the impact of partisan leadership in the longer term and provides cautions on interpreting the changes in racial disparities presented in section 5.1.

5.1 Black driver's share

Graphical Evidence.

I plot the raw data in Figure 2 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 county-cycles have higher Black driver's shares than D-to-R and R-to-R county-cycles since D-to-D county-cycles are generally more urban areas. Before the election, the gap in the Black driver's share between the three groups stays roughly constant across the years within an election cycle. However, one year after the election, the Black driver's share in D-to-R counties increased while the shares in D-to-D and R-to-R counties barely changed.

Estimation results with parallel trend.

In Figure 3, I plot the ATT estimates of β_e^*, which are derived as in equation 2 from regression estimates of equation 1 with the Black driver's share as the outcome variable. I report β_e^* in panel A, Table 4. Before the elections ($t-2$ and t), the interaction term estimates were small and non-significant, giving me confidence that the parallel trend assumption, required by the difference-in-differences research design, is likely to be satisfied in this setting. Right after the election, the Black driver's share increased by 3.8 percentage points in D-to-R counties one year after the election compared to D-to-D counties. (Table 4, Column (1)). Given that the dependent variable mean in D-to-R counties in the year before the elections is 0.24, this amounts to a 15.8% increase in the Black driver's share.

From Columns (2)-(5), 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 ATT estimates from a regression with a sample weighted by the number of stops of 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 the ones in Column (1) if there is not much causal effect heterogeneity along the number of stops dimension. The standard errors may be smaller when I weight the samples by the number of stops if the number of stops varies tremendously with some county-cycles having very small number of stops and the error term variation mostly coming 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 with the number of traffic stops. 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 county-cycles with winners' vote share below 60%. The magnitude of the estimate is smaller than the one in Column (1), but still large. The standard errors become much larger, resulting in the estimate's statistical insignificance.

In Columns (4)-(5), I look into a placebo scenario, the traffic stops done by the police departments in the D-to-D and D-to-R county-cycles. 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.

For this placebo test, I need to confine county-cycles to those that not only satisfy the sample inclusion criteria in the main sample (winner's vote share is less than 80% and yearly number of stops in the sheriff's office is more than 50 every year within an election cycle) but also satisfy such criteria in police departments. The inclusion criterion for the police departments is that the yearly number of stops of the police department is more than 50 every year in the election cycle. With such criteria, I have 30 D-to-D county-cycles and 12 D-to-R county-cycles.

Panel A, Column (4) shows that the ATT estimates with the confined sample are similar to the main sample. Panel A, Column (5) shows that the ATT estimate for the post-election year in the same set of county-cycles with the police department traffic stop data is much smaller than the one seen in Column (4). The much smaller magnitudes in Column (5) suggest that the increase of the Black driver's share in D-to-R counties after the elections is driven by the change of traffic stop practices associated with the newly elected Republican sheriffs, instead of county-specific time trends such as changes in the driver's population in those counties.

Estimation results with parallel trend conditional on urban categories.

I provide ATT estimates under the parallel trend assumption *conditional* on urban categories in Panel B, Table 4. The reported estimates are β_e^{U*} in equation 4, aggregated from estimates in regressions specified as in equation 3, with Black driver's share as the outcome variable. Essentially, I compare the D-to-D and D-to-R county-cycles within each calendar *times* urban category group and then aggregate the differences according to the empirical joint distribution of the urban category group and calendar years within the D-to-R group.

Two patterns emerge comparing the estimates in Panel B with those in Panel A. First, the magnitude of the pre-election interaction terms is larger in Panel B, while the standard errors are roughly the same. All interaction terms are still not significant (except the marginal significant $t-2$ in Column (2)), giving me confidence that the conditional parallel trend assumption is likely to hold in this setting. Second, the magnitude of the post-election interaction terms becomes larger (especially in close election samples in Column (3)), and the standard errors become smaller, leading to significance for all post-election estimates across Columns (1)-(4). The fact that the point estimates do not become smaller once we restrict the comparisons to within urban categories suggests that the large impact of partisan leadership seen in Panel A is not driven by differential trends across urban categories. I view the ATT estimate in Column (1) Panel A as the main estimate instead of the one in Panel B since the number of county-cycles in each urban category *times* calendar year cell is pretty small, which may lead to a larger bias in the point estimates. For the same reason, I report the ATT estimates based on the parallel trend assumption for the rest of the analysis.

Changes in Levels.

Table 4 focuses on the change in shares; I now turn to the changes in the levels to know if more Black drivers are stopped. Table 5 columns (1) and (2) report the ATT estimates as in equation 2 aggregated from a regression specified as equation 1 with the natural log of the number of stops in the separate race groups as outcome variables. Although the magnitude of the coefficient 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 changes in the number of stops across racial groups, Column (3) reports the ATT estimates of γ_e^{0*} and γ_e^{1*} in equation 6, aggregated from a regression as in equation 7. The number of stops associated with Black drivers marginally significantly increased more than those associated with non-Black drivers by 19 percentage points. Combining the estimates in Columns (1)-(3), I interpret the changes in the Black driver's shares observed in Table 4 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.

Column (1)-(3) in Table 5 also shows a notable pattern—the decrease of the number of stops in the election year t. Since D-to-R elections are more competitive than D-to-D ones (Table 1, Panel B), I test whether the competitiveness of the D-to-R elections drives the lower number of stops. In Column (4), I test the hypothesis by further singling out the D-to-R county-cycles with close elections (winner's vote share below 60%). I denote the counties with close elections as one with the Close dummy variable, zero otherwise. Although the interaction term $t \times$ D-to-R $\times$ Close is large (0.29) compared to the baseline interaction term $t \times$ D-to-R (-0.57), the interaction term $t \times$ D-to-R $\times$ Close is not significant. Interpreting the sign of the interaction terms, if anything, the lower number of stops in the election year is driven by the county-cycles with non-competitive elections.

An alternative explanation is that for places where incumbent sheriffs did not participate in the sheriff elections, they might exert less effort (Losak and Makowsky, 2023). I thus differentiate counties based on whether the incumbent sheriff participated in the election. Incumbent sheriff participated in thirty-three out of forty-seven D-to-D county-cycles and five out of fifteen D-to-R county-cycles. The dummy variable $Incumbent$ is 1 if the incumbent sheriff participated in the election. The interaction term $t \times$ D-to-R $\times$ Incumbent in Column (6) is small and insignificant, showing that the magnitude of the decrease in the number of stops in the election year in D-to-R counties is similar no matter the incumbent sheriff participated in the election or not.

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 6, column (2), I display ATT estimates (β_e^*) from an OLS regression with specification as in equation 1, and aggregated as in equation 2, with the outcome variable the share of safety stops among all stops. Compared to the year before the election, the share of safety stops decreases by 9.1 percentage points after the elections. Compared to the dependent variable mean in D-to-R counties in the year before the election, this is a 17% decrease.

Changes in the focus on safety and investigatory stops can have a racially 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 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 into four parts: 1) the part contributed by the changes in the share of safety stops (while holding the 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 the Appendix.

I report the decomposition results in Table 6, 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 12.6% of the changes in the Black driver's share can be explained by the changes in the share of safety stops. The major contributor is the change within the safety stops instead of the investigation stops. Changes within the safety stops account for 63% of the total changes (Column (4)).

5.3 Personnel policies

Officers play essential 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 relate to Black drivers' share in traffic stops. I test two mechanisms related to officers that may lead to a change in Black drivers' share. First, officers respond to the new sheriff's leadership by changing traffic stop practices. Second, officers do not change their traffic stop practices, but the reshuffling of the personnel by the new sheriffs makes the agencies have a higher Black drivers' share in traffic stops.

To test the two mechanisms, I decompose the difference in Black drivers' share at the agency level across years into four parts, in the same way as in section 5.2. Here, the stops are categorized based on who conducted the stops: stayers or non-stayers. The changes in the Black driver's share across years can 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. The second and third parts are changes in the Black driver's share within stayer and non-stayer stops, holding the share of total stops done by stayers the same as in the baseline year. The fourth part is 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 in the Black driver's share if the new sheriff shuffled specific types of officers out of the patrolling team, making stayers and non-stayers (before the election) stops have 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 Black driver's shares within non-stayers vary over time.

Table 7 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). Columns (3)-(6) display ATT estimates from an OLS regression with specification as in equation 7 in the Appendix, and aggregated as in equation 2. 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 share of non-stayers stops increases after the elections, such change cannot explain the changes in the Black driver's share. The ATT estimate of the post-election and D-to-R dummy interaction term in Column (3) is small and insignificant. This suggests that the selection of officers continuing the patrolling duty among all who conducted stops before the elections is not correlated to 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 70% of the total changes in Black drivers' shares. The changes within non-stayers, shown in Column (5), are a non-negligible magnitude but marginally not significant. Overall, the decomposition results in Table 7 offer evidence in favor of the mechanism where officers' responses to new leaders contribute to the changes in the Black driver's share.

I now directly examine the changes in Black drivers' share within stayers and non-stayers by estimating ATT from regressions with outcome variables: Black drivers' share within stayer stops, and Black drivers' share within non-stayer stops. The estimation results are reported in Columns (1) and (2) in Table 8. Column (1) shows that stayers in D-to-R agencies, on average, increase the Black driver's share by four percentage points after elections relative to the changes in stayers in D-to-D agencies. Column (2) shows that the non-stayers in post-election years in D-to-R agencies do not behave significantly 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 or wide set of officers. The evidence presented next supports that the changes come from a common practice change.

To examine how widespread it is across officers that 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 and 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 two averages for each officer in an election cycle: one comes from stops before, and one comes from stops after elections. I then take the differences in the tendencies within each officer and plot the cumulative distribution function of the differences in Figure 4. Figure 4 goes against the hypothesis that the practice change is confined to a small set of officers. If the behavior changes in Column (1) in Table 8 is driven by a few officers, I would expect that the cumulative distributions are only different at the top. Instead, figure 4 shows that the two cumulative distributions are different at the top *and* in the middle (above 0.5). This implies that partisan leadership impact is an aggregate of many stayers increasing their tendency to stop black drivers.

I conclude the personnel analysis by examining how much more reshuffling happens in D-to-R counties than in D-to-D counties. I report ATT estimates from regression as in equation 1 with outcome variables being the share of non-stayers and 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. [7] Column (3) in Table 8 shows that D-to-R counties have an increase in the share of non-stayers by seventeen percentage points, compared to D-to-D counties after elections. The increase in the

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

share of non-stayers, not just the share of stops done by non-stayers (Column (2) in Table 7), suggests that the new Republican sheriffs shuffle in patrol teams *many* officers who did not conduct traffic stops in the two years before the elections. In particular, many newly shuffled-in officers have not conducted any traffic stops in the agency before the elections (Column (4)).

I provide two takeaways from the officer analysis. First, a large set of officers in D-to-R counties seem to have changed 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 in and out behave similarly in terms of the share of Black drivers they stopped. The two takeaways contribute to the literature by showing that a leader's management/policy may malleate officers' behavior. Policymakers who aim to reduce racial disparities in traffic stops can potentially learn from the differences in the management/policies of 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 Table 4 Column (1).

The exercise consists of two steps. First, using only the stop data before the elections (not confined to the same election cycle), I regress a dummy variable indicating whether the stopped 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 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 regression specified as in 1 with such averages as the outcome variable.

Table 9 reports the estimation results. Across columns, I find that the predicted probabilities of a stop associated with a Black driver based on time or location 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 estimates suggest that the changes in the Black driver's share under the new Republican sheriffs' leadership are not driven by a shift of focus in patrolling specific 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. Such data may not detect a shift of focus in the neighborhood. 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 have examined 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 changes in the officer's search behavior, holding the stopped driver population the same as before elections. [8] 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 10 reports ATT estimates from regression as in equation 1 with outcome variables being overall search rates, Black driver search rates, and non-Black driver search rates at the county-year level. Results in Panel A show that search rates in all stops (combining safety and investigatory) do not significantly change in D-to-R

[8] 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)

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 search rates in safety and investigatory stops do not significantly increase, though the magnitude of the ATT estimates for the post-election year in safety stops are much larger than the ones in investigatory stops.

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 number of searches with found contraband divided by the number of stops, can thus be an efficiency measure of the law enforcement agency's traffic stop performance. 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 11 reports ATT estimates from regression as in equation 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 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 4 and Table 11 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. For this purpose, I extend the analysis period to four years before and after the elections. I estimate the partisan leadership impacts with a specification similar to equation 1, and aggregate the estimates into ATT estimates like in equation 2. 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. The sample inclusion criteria are the same as the short-term analysis: the winner's vote share is less than 80%, and the yearly number of stops must be above 50 every year within an election cycle. Some counties that satisfy the short-term sample inclusion criteria do not satisfy the long-term sample inclusion criteria because they have fewer than 50 stops in the extended election cycle. This results in fewer county-cycles in the long-term analysis, 47 as opposed to 62 in the short-term analysis.

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 thus may fail as the counties progress into the next elections.

I present the Black driver's share in the longer cycles in Figure 5. The gap between D-to-D and D-to-R groups shrinks significantly right after the elections, the same pattern as in the shorter election cycles in Figure 2. As we progress to the end of the election cycle, the gap widens to a similar level as in pre-election periods.

Table 12, Column (1) confirms the pattern seen in Figure 5. 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. 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 years 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 at the beginning of the cycle 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 Black drivers' share of stops made by police officers in D-to-D and D-to-R counties exhibit 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 partisan leadership 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, e.g., pressure from the upcoming elections. Drivers may 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

I present evidence that partisan leadership affects traffic stop behaviors. A Democratic-to-Republican sheriff turnover, compared to a Democratic-to-Democratic transition (which may or may not involve sheriff turnover), increases the Black driver's share among all stops by 3.8 percentage points. 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 practices 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. 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.

Given the evidence I find that Republican sheriffs are associated with changes in the share of traffic safety stops, one interesting direction of future research is to examine if efficiency measures on traffic safety stops (e.g., number of crashes) respond to the party affiliation of sheriffs.

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Figures

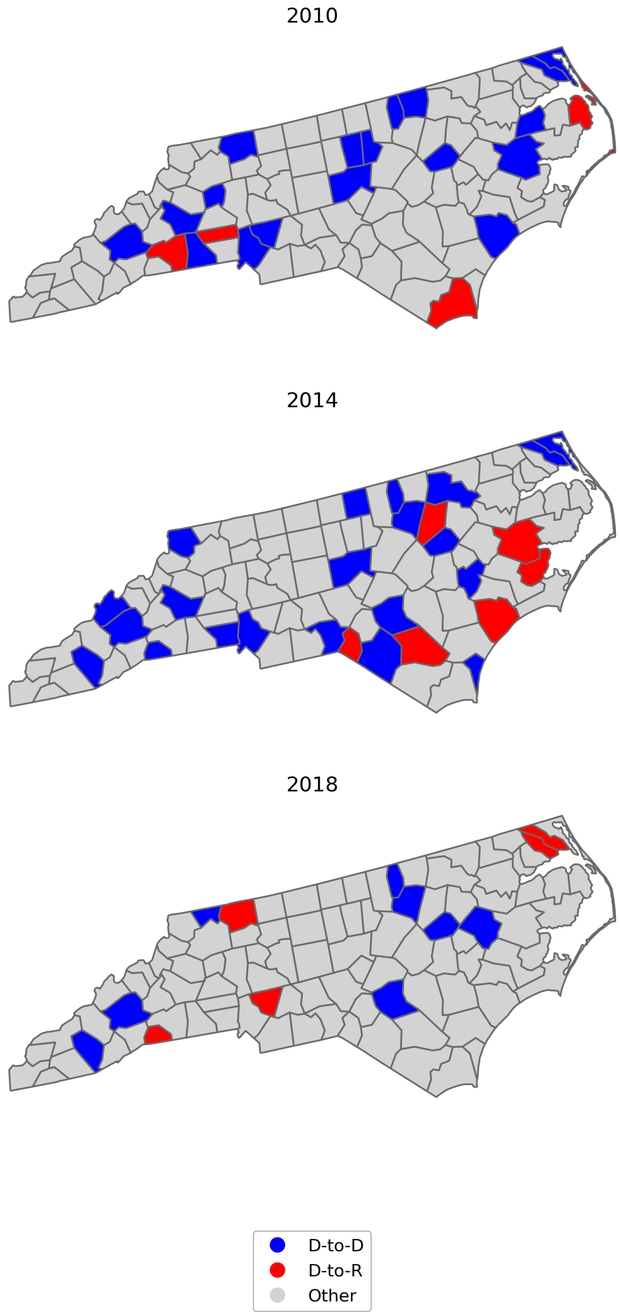

Figure 1: D-to-D and D-to-R county maps

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

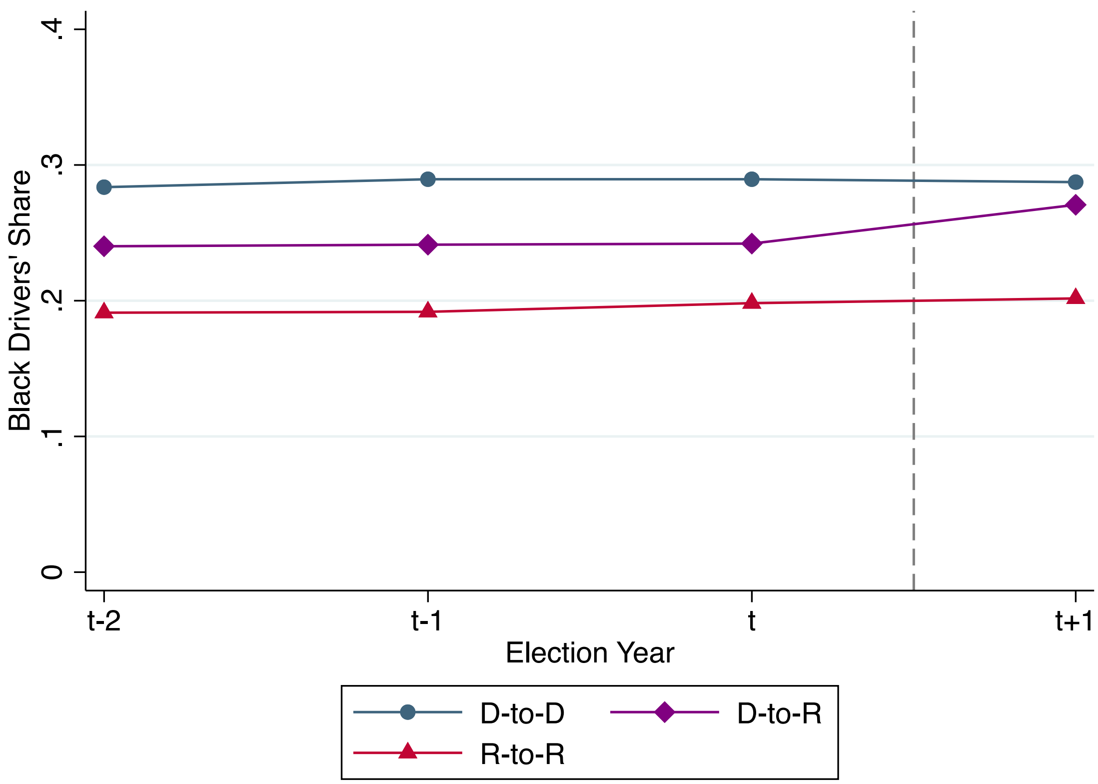

Figure 2: Black Drivers' Share Among All Stops

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, stacking up the three election cycles. Each dot thus contains samples from three years.

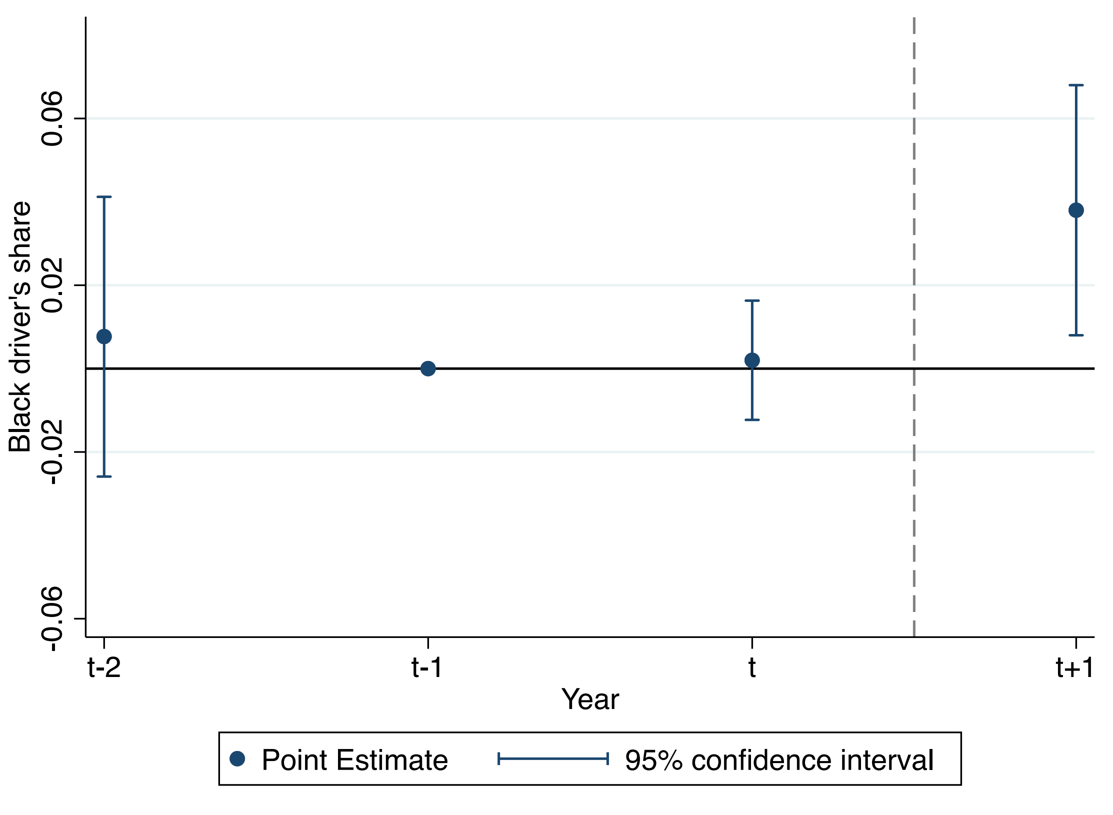

Figure 3: The Impact of Partisan Sheriffs on Black Driver's Share

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 driver's share as the outcome variable. t denotes the year when the election happened.

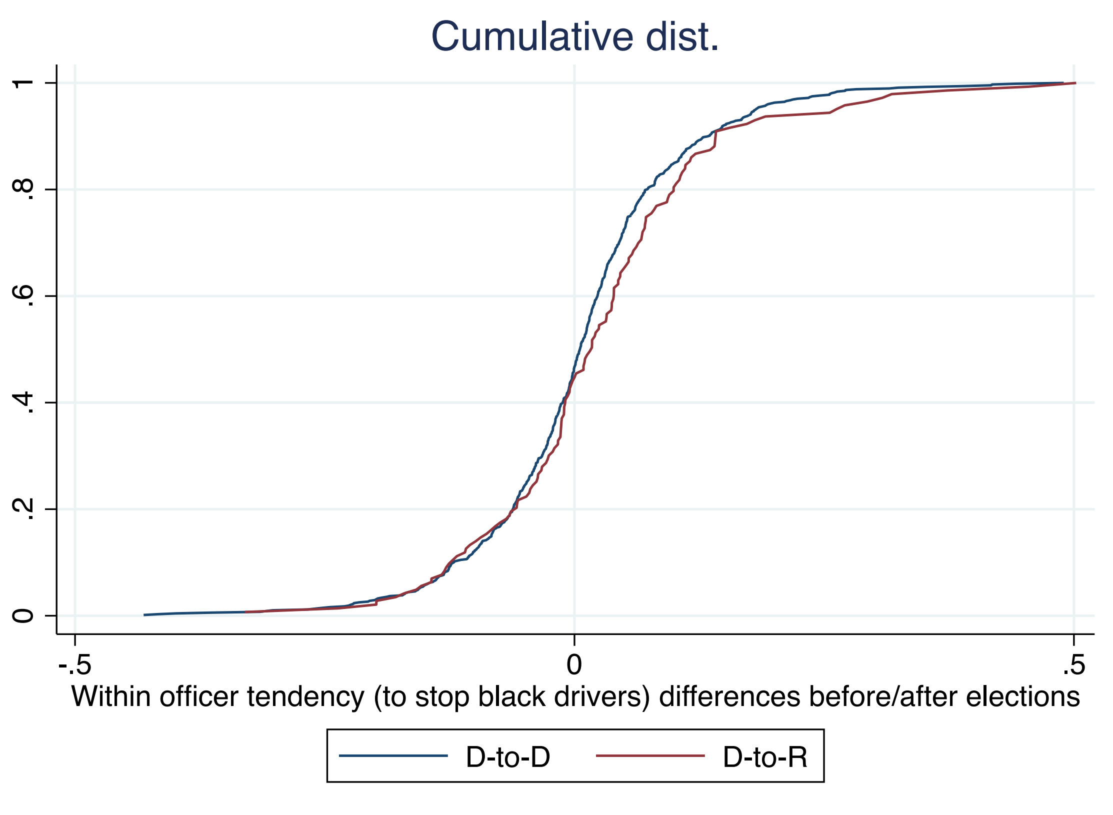

Figure 4: Cumulative distributions of the differences in the tendency of stopping black drivers before and after elections among stayers

Notes: This figure plots two cumulative 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) × 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.

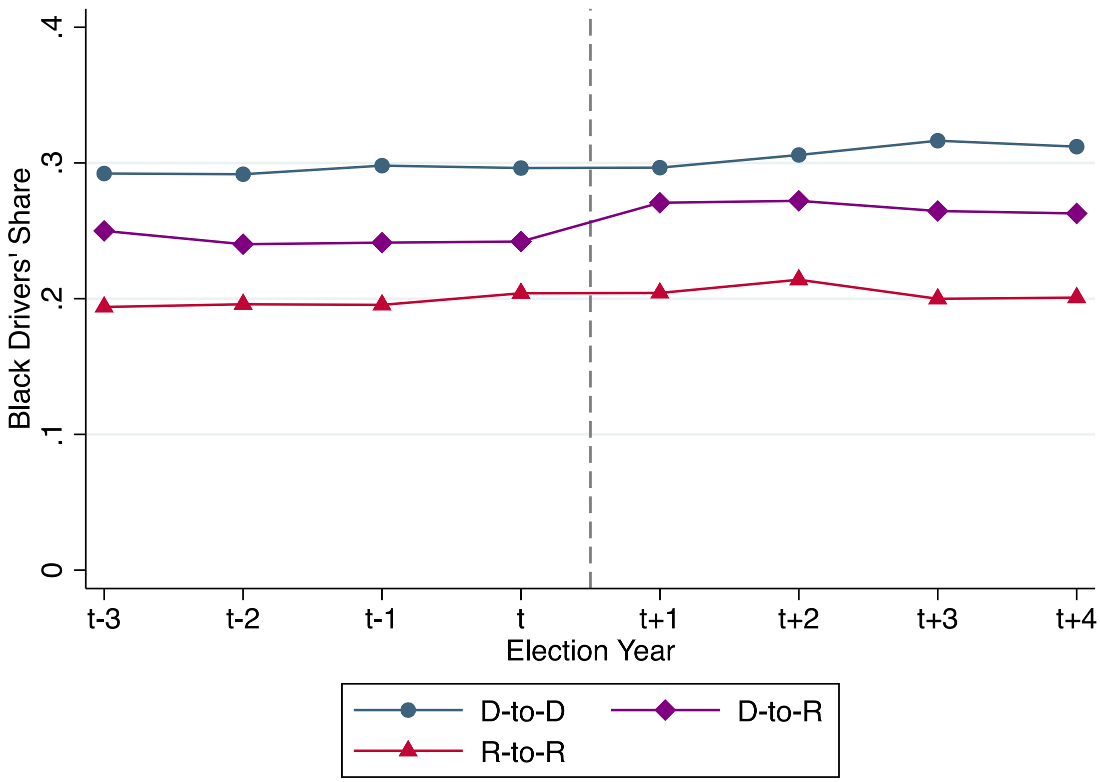

Figure 5: Black Drivers' Share Among All Stops in Longer Electoral Cycles

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. The number of county-cycles are 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

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	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 > 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 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	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 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. In Panel B, I drop elections in which the winner's vote share is larger than 80%. This criterion is chosen to match the vote share support of D-to-R elections. In Panel C, I drop elections that are dropped in Panel B and drop the ones in which the county had at least one year with 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. I use county-cycles in Panel C in the estimation.

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 C in 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 metro includes counties with MSAs of 50,000 to 999,999 population. Nonmetropolitan includes the other counties. The population share of the counties are population-weighted averages derived from county-level data from 2010, 2014, and 2018 American Community Survey 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 households 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	White	Safety	Investigation	
Share Black	1.000	0.000	0.000	0.239	0.280	0.259
Share Hispanic	0.000	1.000	0.000	0.068	0.070	0.069
Share White	0.000	0.000	1.000	0.667	0.632	0.650
Share Female	0.362	0.240	0.359	0.357	0.344	0.351
Share Safety Stops	0.478	0.510	0.530	1.000	0.000	0.517
Share Investigatory Stops	0.522	0.490	0.470	0.000	1.000	0.483
Search Rate	0.079	0.086	0.061	0.051	0.084	0.067
Unconditional Hit Rate	0.024	0.016	0.021	0.016	0.027	0.022
Observations	85,607	22,764	214,956	170,814	159,732	330,546

Notes: This table presents summary statistics including all D-to-D and D-to-R county-cycles included in Panel C 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.

Table 4: The Impact of Partisan Sheriffs on Black Driver's Share: Regression Estimates and a Placebo Test

	$\frac{\text{\# of black driver}}{\text{\# of all stops}}$				
	Sheriff's offices				Police departments
	(1)	(2)	(3)	(4)	(5)
Panel A: Parallel trend between D-to-D and D-to-R counties					
t-2 x D-to-R	0.0077	0.0140	-0.0243	0.0080	-0.0068
	(0.0166)	(0.0122)	(0.0254)	(0.0210)	(0.0105)
	[0.6467]	[0.2569)]	[0.3508)]	[0.7073)]	[0.5205)]
t x D-to-R	0.0020	0.0032	-0.0041	-0.0004	0.0054
	(0.0071)	(0.0069)	(0.0122)	(0.0077)	(0.0121)
	[0.7776]	[0.6478)]	[0.7428)]	[0.9560)]	[0.6570)]
t+1 x D-to-R	0.0380	0.0360	0.0314	0.0399	0.0057
	(0.0149)	(0.0149)	(0.0225)	(0.0166)	(0.0107)
	[0.0144]	[0.0201)]	[0.1766)]	[0.0234)]	[0.5946)]
Panel B: Parallel trend conditional on urban categories between D-to-D and D-to-R counties					
t-2 x D-to-R	0.0129	0.0209	-0.0243	0.0170	-0.0094
	(0.0170)	(0.0121)	(0.0157)	(0.0223)	(0.0135)
	[0.4530]	[0.0902)]	[0.1376)]	[0.4501)]	[0.4912)]
t x D-to-R	0.0064	0.0111	-0.0101	0.0063	0.0050
	(0.0066)	(0.0091)	(0.0118)	(0.0077)	(0.0129)
	[0.3397]	[0.2302)]	[0.4032)]	[0.4191)]	[0.7032)]
t+1 x D-to-R	0.0471	0.0373	0.0665	0.0514	-0.0012
	(0.0138)	(0.0130)	(0.0252)	(0.0136)	(0.0113)
	[0.0015]	[0.0065)]	[0.0155)]	[0.0007)]	[0.9150)]
Dep. mean	0.2413	0.1869	0.2425	0.2288	0.2681
# of control	47	47	8	30	30
# of treatment	15	15	9	12	12
N	248	248	104	168	168
Sample	All	All	Close election	Good data in police departments	
County-Cycle	Yes	Yes	Yes	Yes	Yes
Year	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 county-year level. t refers to the year of election in that election cycle. Panel A reports β_e^* in equation 2; panel B reports β_e^{U*} in equation 4. β_e^* (β_e^{U*}) are estimates of the average treatment effect on the treated of the impact of a D-to-R sheriff turnover (treatment group) as opposed to a D-to-D transition (control group) under parallel trend assumption (conditional parallel trend assumption). Columns (1)-(4) report results with traffic stop samples from sheriff's offices. Column (5) reports results with samples from police departments. Column (1)-(2) includes all county-cycles in Panel C, Table 1. Column (3) further confines samples to elections where the winner's vote share is less than 60%. Columns (4)-(5) confine samples to the county-cycles where the winner's vote share is less than 80% in the sheriff's elections and every police department (and sheriff's offices) within the county has a yearly number of stops above 50 in every year within an election cycle. All regression specifications include county-cycle and calendar-year fixed effects. I weight the county-year observations by the number of stops of that county in $t-2$ in Column (2). Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Table 5: The Impact of Partisan Sheriffs on the Number of Stops by Race

	Black	Non-Black	Both Race	All stops	All stops
	(1)	(2)	(3)	(4)	(5)
t-2 x D-to-R	-0.2217	-0.1998	-0.1998	-0.2245	-0.1867
	(0.1653)	(0.1366)	(0.1478)	(0.1929)	(0.1674)
	[0.1873]	[0.1512)]	[0.1839)]	[0.2511)]	[0.2712)]
t x D-to-R	-0.3238	-0.3977	-0.3977	-0.5736	-0.5354
	(0.1959)	(0.1917)	(0.2074)	(0.2564)	(0.1907)
	[0.1059]	[0.0443)]	[0.0621)]	[0.0308)]	[0.0076)]
t+1 x D-to-R	0.2628	0.0714	0.0714	0.0058	-0.2585
	(0.3200)	(0.3061)	(0.3312)	(0.2554)	(0.3826)
	[0.4163]	[0.8166)]	[0.8303)]	[0.9820)]	[0.5030)]
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)]		
t-2 x D-to-R x Close				0.3624	
				(0.2514)	
				[0.1570)]	
t x D-to-R x Close				0.2954	
				(0.3237)	
				[0.3668)]	
t+1 x D-to-R x Close				0.1352	
				(0.3934)	
				[0.7328)]	
t-2 x D-to-R x Incumbent					-0.0372
					(0.2855)
					[0.8969)]
t x D-to-R x Incumbent					0.0654
					(0.2462)
					[0.7920)]
t+1 x D-to-R x Incumbent					0.4412
					(0.4154)
					[0.2944)]
Average # of stops	269	1,042		1,311	1,311
N	248	248	496	248	248
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 county-year level. The outcome variables in column (1)-(5) are respectively the natural log of number of Black stops, number of non-Black stops, number of stops in the group (Black or non-Black), number of all stop, and number of all stops. t refers to the year of election in that election cycle. Column (1)-(2) report β_e^* in equation 2. Column (3)-(5) report $\gamma_e^{1*}, \gamma_e^{2*}$ in equation 4. $\beta_e^*, \gamma_e^{1*}, \gamma_e^{2*}$ are estimates of average treatment effect on the treated of the impact of a D-to-R sheriff turnovers (treatment group) as opposed to a D-to-D transition (control group) under parallel trend assumption. 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 $t-1$, one year before the sheriff election.

Table 6: 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	0.0077	-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

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. 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)-(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) reports ATT estimates from an OLS regression with specification as in equation 7 in the Appendix, and aggregated as in equation 2. 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 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. I 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. 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 black driver's share from changes in the share of safety stops of all stops (while keeping the black driver's share within 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.Dep. mean computed from D-to-R counties in year $t - 1$.

Table 7: 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	0.0077	-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 x 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 x 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	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 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)-(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 7 in the Appendix, and aggregated as in equation 2. 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 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. Dep. mean computed from D-to-R counties in year $t - 1$.

Table 8: Officer Behavior Change and Personnel Turnover

	(1) $\frac{\text{Black Stops by Stayers}}{\text{All Stops by Stayers}}$	(2) $\frac{\text{Black Stops by Non-Stayers}}{\text{All Stops by Non-Stayers}}$	(3) $\frac{\text{\# of non-stayers}}{\text{\# of all officers}}$	(4) $\frac{\text{\# of new officers}}{\text{\# of all officers}}$
t-2 x D-to-R	0.0525	-0.0344	-0.0025	-0.0120
	(0.0358)	(0.0358)	(0.0311)	(0.0481)
	[0.1503]	[0.3431)]	[0.9352)]	[0.8035)]
t x D-to-R	0.0104	-0.0310	0.0335	0.0303
	(0.0125)	(0.0285)	(0.0416)	(0.0544)
	[0.4094]	[0.2831)]	[0.4254)]	[0.5809)]
t+1 x D-to-R	0.0419	0.0041	0.1696	0.1889
	(0.0219)	(0.0298)	(0.0572)	(0.0638)
	[0.0624]	[0.8917)]	[0.0050)]	[0.0051)]
Dep. mean	0.2294	0.2661	0.6125	0.3658
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 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. 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. Dep. means are computed from D-to-R counties one year before the election.

Table 9: 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 D-to-R	0.0030	0.0023	0.0028	0.0004	0.0028	0.0079
	(0.0023)	(0.0038)	(0.0020)	(0.0041)	(0.0033)	(0.0047)
	[0.2076]	[0.5449)]	[0.1838)]	[0.9255)]	[0.4034)]	[0.1051)]
t x D-to-R	-0.0007	-0.0054	-0.0006	-0.0043	-0.0012	-0.0057
	(0.0014)	(0.0036)	(0.0014)	(0.0048)	(0.0018)	(0.0047)
	[0.6081]	[0.1445)]	[0.6851)]	[0.3764)]	[0.5285)]	[0.2344)]
t+1 x D-to-R	-0.0004	-0.0031	-0.0010	-0.0030	-0.0007	-0.0033
	(0.0013)	(0.0035)	(0.0016)	(0.0045)	(0.0015)	(0.0042)
	[0.7392]	[0.3830)]	[0.5474)]	[0.5143)]	[0.6286)]	[0.4363)]
Dep. mean	0.2427	0.2403	0.2428	0.2376	0.2426	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 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. For Columns (1), (3), and (5), we predict whether the stop is associated with a Black driver (Black stop) by the share of Black stops before (including) the election year in each location cell. For Columns (2), (4), and (6), we predict whether the stop is a Black stop by 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 computed from D-to-R counties before the election.

Table 10: The Impact of Partisan Sheriffs 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 D-to-R	0.0085	-0.0124	0.0126
	(0.0113)	(0.0169)	(0.0107)
	[0.4534]	[0.4673)]	[0.2454)]
t x D-to-R	-0.0043	-0.0252	0.0034
	(0.0087)	(0.0236)	(0.0106)
	[0.6268]	[0.2919)]	[0.7484)]
t+1 x D-to-R	0.0130	0.0319	0.0064
	(0.0158)	(0.0230)	(0.0187)
	[0.4140]	[0.1726)]	[0.7321)]
Dep. mean	0.0832	0.1102	0.0768
Panel B: Safety stops			
t-2 x D-to-R	0.0062	-0.0073	-0.0031
	(0.0189)	(0.0363)	(0.0192)
	[0.7440]	[0.8413)]	[0.8715)]
t x D-to-R	-0.0112	-0.0405	-0.0086
	(0.0112)	(0.0275)	(0.0138)
	[0.3255]	[0.1494)]	[0.5347)]
t+1 x D-to-R	0.0303	0.0459	0.0164
	(0.0198)	(0.0387)	(0.0220)
	[0.1335]	[0.2428)]	[0.4610)]
Dep. mean	0.0667	0.0818	0.0661
Panel C: Investigation stops			
t-2 x D-to-R	0.0103	-0.0257	0.0254
	(0.0123)	(0.0194)	(0.0155)
	[0.4062]	[0.1932)]	[0.1085)]
t x D-to-R	0.0028	-0.0107	0.0179
	(0.0148)	(0.0353)	(0.0190)
	[0.8521]	[0.7632)]	[0.3500)]
t+1 x D-to-R	-0.0036	-0.0001	-0.0038
	(0.0220)	(0.0312)	(0.0296)
	[0.8723]	[0.9970)]	[0.8979)]
Dep. mean	0.1037	0.1346	0.0923
N	248	248	248
County-Cycle	Yes	Yes	Yes
Year	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 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. Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Table 11: The Impact of Partisan Sheriffs 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	0.0095	0.0179	0.0092
	(0.0059)	(0.0098)	(0.0062)
	[0.1126]	[0.0751)]	[0.1473)]
t x D-to-R	-0.0038	0.0080	-0.0016
	(0.0055)	(0.0143)	(0.0076)
	[0.4956]	[0.5769)]	[0.8320)]
t+1 x D-to-R	0.0047	0.0178	0.0006
	(0.0082)	(0.0093)	(0.0115)
	[0.5681]	[0.0618)]	[0.9602)]
Dep. mean	0.0304	0.0337	0.0337
Panel B: Safety stops			
t-2 x D-to-R	0.0152	0.0128	0.0103
	(0.0095)	(0.0213)	(0.0125)
	[0.1184]	[0.5512)]	[0.4146)]
t x D-to-R	-0.0081	-0.0062	-0.0084
	(0.0068)	(0.0131)	(0.0088)
	[0.2397]	[0.6409)]	[0.3473)]
t+1 x D-to-R	0.0156	0.0246	0.0073
	(0.0084)	(0.0141)	(0.0124)
	[0.0690]	[0.0873)]	[0.5615)]
Dep. mean	0.0244	0.0257	0.0257
Panel C: Investigation stops			
t-2 x D-to-R	0.0054	0.0102	0.0099
	(0.0100)	(0.0145)	(0.0134)
	[0.5950]	[0.4835)]	[0.4610)]
t x D-to-R	0.0011	0.0166	0.0087
	(0.0105)	(0.0254)	(0.0153)
	[0.9181]	[0.5174)]	[0.5737)]
t+1 x D-to-R	-0.0052	0.0109	-0.0065
	(0.0146)	(0.0154)	(0.0240)
	[0.7235]	[0.4830)]	[0.7884)]
Dep. mean	0.0376	0.0428	0.0354
N	248	248	248
County-Cycle	Yes	Yes	Yes
Year	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 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. Contraband refers to searches that found contraband successfully. Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Table 12: Longer-term Impact of Partisan Sheriffs on Black Driver's Share

	$\frac{\text{\# of black driver}}{\text{\# of all stops}}$				
	(1)	(2)	(3)	(4)	(5)
	Sheriff's offices				Police departments
t-3 x D-to-R	0.0003	0.0187	-0.0142	0.0062	0.0064
	(0.0121)	(0.0192)	(0.0138)	(0.0182)	(0.0090)
	[0.9799]	[0.3365)]	[0.3197)]	[0.7379)]	[0.4890)]
t-2 x D-to-R	0.0052	0.0073	-0.0204	-0.0036	-0.0142
	(0.0192)	(0.0134)	(0.0233)	(0.0315)	(0.0134)
	[0.7868]	[0.5872)]	[0.3969)]	[0.9107)]	[0.3010)]
t x D-to-R	0.0042	0.0016	0.0053	0.0056	0.0000
	(0.0079)	(0.0065)	(0.0117)	(0.0102)	(0.0164)
	[0.5969]	[0.8037)]	[0.6578)]	[0.5857)]	[0.9978)]
t+1 x D-to-R	0.0321	0.0280	0.0111	0.0313	0.0036
	(0.0128)	(0.0134)	(0.0135)	(0.0124)	(0.0134)
	[0.0182]	[0.0463)]	[0.4253)]	[0.0200)]	[0.7883)]
t+2 x D-to-R	0.0310	0.0224	-0.0093	0.0387	-0.0179
	(0.0215)	(0.0162)	(0.0265)	(0.0224)	(0.0121)
	[0.1602]	[0.1770)]	[0.7300)]	[0.1002)]	[0.1571)]
t+3 x D-to-R	0.0092	0.0181	-0.0272	-0.0069	-0.0141
	(0.0190)	(0.0153)	(0.0205)	(0.0214)	(0.0173)
	[0.6304]	[0.2474)]	[0.2072)]	[0.7503)]	[0.4244)]
t+4 x D-to-R	0.0124	0.0175	0.0002	0.0067	-0.0086
	(0.0149)	(0.0163)	(0.0191)	(0.0189)	(0.0161)
	[0.4108]	[0.2912)]	[0.9927)]	[0.7249)]	[0.5999)]
Dep. mean	0.2471	0.1720	0.2446	0.2656	0.3559
# of control	35	35	8	24	24
# of treatment	12	12	9	7	7
N	376	376	144	248	248
Sample	All	All	Close election	Good data quality in police departments	
County-Cycle	Yes	Yes	Yes	Yes	Yes
Year	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 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. Columns (1)-(4) report results with traffic stop samples from sheriff's offices. Column (5) reports results with samples from police departments. Columns (1)-(2) includes all county-cycles where the winner's vote share is less than 80% and the yearly number of stops are above 50 in every year within an election cycle. Column (3) further confines samples to elections where winner's vote share is less than 60%. Columns (4)-(5) confine samples to the county-cycles where the winner's vote share is less than 80% in the sheriff's elections and every police departments (and sheriff's offices) within the county have yearly number of stops above 50 in every year within an election cycle. All regression specifications include county-cycle and calendar year fixed effects. I weight the county-year observations by the number of stops of that county in $t-2$ in Column (2). Dep. means are computed from D-to-R counties in year $t-1$, one year before the sheriff election.

Appendix

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

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:

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

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 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 Column (4)-(5) in Table 6. 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 6.

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

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

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

$$
\begin{aligned}
Y_{cle} = & \sum_{l=2014}^{2018} \sum_{e=-2}^{1} \beta_{le} D_{cl}^{D-to-R} \cdot (\eta_e - \eta_{-1}) \cdot \eta_l + \sum_{e=-2}^{1} \beta_e D_{cl}^{D-to-R} \cdot (\eta_e - \eta_{-1}) + \\
& + (\delta_{le} - \delta_{l,-1}) + (\epsilon_{cle} - \epsilon_{cl,-1}).
\end{aligned} \tag{7}
$$

Hence, I can use the terms in the four brackets above as outcome variables, and estimate four regressions with specifications 7 (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 ATT estimates can be derived with the same aggregation 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.