Racial Representation in Local Government and Racial Disparities in Policing

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Abstract

We draw on statewide data from North Carolina to examine the impacts of racial and ethnic representation in city councils on policing. Specifically, we focus on outcomes of traffic stops; e.g., whether a driver receives a warning or a citation after being stopped. We first document large Black-white and Latino-white disparities in the likelihood of consequence (arrest or citation) after a traffic stop. We then use a difference-in-differences design, focusing on changes following (narrow) elections of nonwhite (rather than white) councilmembers, and find that increased nonwhite council representation significantly reduces Black-white gaps in stops and actions taken after a stop. The magnitude of the reduction is similar with and without officer fixed effects, suggesting that results are largely driven by individual officer-level behavior change rather than a change in the composition of the police force.

Keywords: policing, representation, race

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1 Introduction

Black and Latino drivers throughout the United States are disproportionately stopped by police and disproportionately searched when stopped; while obvious from the most basic statistics, the geographic scope and magnitude of this problem has been laid bare by new large-scale databases of traffic stops (Baumgartner et al., 2017; Pierson et al., 2020). Moreover, there is ample evidence that said gaps in stops are discriminatory. The racial gap in stops narrows after dark, when it is harder for the officer to see the driver (Grogger and Ridgeway, 2006). When drivers of color are searched, officers are less likely to find contraband, indicating a lower threshold for initiating the stop and search (Pierson et al., 2020).

Racial gaps in traffic stops, and the consequences therein, are of course one piece of a broader system of racialized policing and racism in the criminal justice system. Despite heightened public attention and the implementation of reforms aimed at reducing these disparities — ranging from broader racial representation in police forces, as well as the adoption of measures such as body cameras, de-escalation training, Department of Justice (DOJ) investigations, federal consent decrees, and external court monitoring – racial gaps in policing persist.

Ultimately, the impact of any of these policies depends on the degree of enforcement and, potentially, the simultaneous introduction of a suite of other (potentially unobserved) reforms. In US municipalities - oversight of police agencies ultimately occurs in city council and mayors' offices. We therefore step back from specific policies and ask: how does diverse racial representation amongst elected officials in a municipality (especially, city council) impact racial disparities in policing outcomes? The outcomes we primarily focus on in this paper are traffic stops (for reasons like, for example, speed limit or vehicle equipment violations) and the actions taken by officers conditional on stopping a driver (e.g., whether a citation vs. a verbal warning is issued) – both of which are actions where officers have substantial discretion.

We draw on data on police traffic stops from North Carolina throughout the 2010s. North Carolina is a setting that provides a unique opportunity to explore our research questions. First, there is a rich (nearly) statewide dataset on the universe of traffic stops. Second, unique features of North Carolina's voter registration files facilitate identifying the race of city council candidates at a statewide level – that is, for municipalities small and large. This would not be as readily possible in other states.

In our analysis, we first document large baseline disparities in whether an officer issues citation or arrests a driver conditional on making a stop. This is true in the simplest specification and in a rich specification including an array of fixed effects and controls, including officer fixed effects. Black drivers are 1.5-3.8 percentage points more likely than white drivers to receive fines or face arrest following a traffic stop. This gap widens to 11.1-14.3 percentage points for Latino drivers.

We then adopt a difference-in-differences approach, combined with intuition from a regression discontinuity approach, to provide causal evidence on the impacts of racial representation in city council in reducing the baseline disparities we document. We find that one additional (narrowly elected) non-white member of city council leads to a reduction in the number of Black drivers stopped by officers and - conditional on being stopped - a 2.2-2.8 percentage point reduction in the Black-white disparity in the likelihood of facing a citation or arrest. We find minimal impacts of a narrowly elected non-white councilmember on Latino stops or post-stop action; we note though that the vast majority of non-white councilmembers we observe are Black.

Our work is linked to two literatures: one on the impacts of racial and ethnic representation in elected office and a second on racial disparities in policing and traffic stops. Several recent papers draw on quasi-experimental methods to provide causal evidence on how representation of Black and Latino elected officials in local government impacts disparate racial/ethnic outcomes in K-12 education (Kogan et al., 2021; Fischer, 2023) and investment in neighborhoods, as proxied by housing prices (Beach et al., 2018). Our paper uses similar methods to explore whether racial representation impacts policing outcomes. On the one hand, policing is one of the central policy areas under the purview of municipal governments and as such one might expect that similar results - a reduction in disparate outcomes - would materialize in the context of policing. On the other hand, as noted above, racial disparities in policing are notoriously persistent. Moreover, Facchini et al. (2020) document that while the expansion of the franchise to Black Americans following the Voting Rights Act of 1965 impacted arrests made by Sheriff's departments – whose chief officer is *directly* elected –, there was no change in arrests made by municipal police departments – where the chief office is appointed by city government. They argue that *direct* political accountability was the difference.

There has also been a body of work documenting correlational relationships between representation and policing policy and outcomes. For example, Sharp (2014) finds that cities with Black representation in elected office have a higher share of Black officers; that effect does not, however, translate into changes in racial arrest patterns. Marschall and Shah (2007) find that cities with Black representation in elected office, especially in council, are more likely to have Civilian Review Board and Community Policing initiatives. Christiani et al. (2022) find that a majority Black council is associated with lower search rates across both Black and white drivers conditional on being stopped.

There *is* substantial *causal* evidence on other factors that may reduce racial disparities in policing, including - for instance - officer demographics (Ba et al., 2021; Hoekstra and Sloan, 2022), and also on documenting racial bias as a driver of disparities in who is stopped and/or searched (Grogger and Ridgeway, 2006; Antonovics and Knight, 2009).

Finally, there is relatively less evidence on racial disparities in the most frequent outcomes of traffic stops (warnings and citations) than on stops themselves and searches. There are some critical exceptions. For example, drawing on data from Massachusetts, Makowsky and Stratmann (2009) find no evidence of a race difference in the likelihood that a stopped driver receives a citation. Seguino and Brooks (2021), drawing on data from Vermont, and Baumgartner et al. (2020), drawing on data from North Carolina, on the other hand, both find that Black and Latino drivers are more likely to receive a citation following a traffic stop.[1]

The primary contribution of our paper is to present new causal evidence on the question of how racial representation in elected office shapes policing behavior and outcomes, and especially racial disparities therein. We consider an outcome that impacts a large number of individuals: traffic stops and the most typical outcomes of those traffic stops. (Indeed, roughly eight percent of the driving population in the US in a given year has contact with law enforcement via a traffic stop.)[2] In doing so, we also present new evidence on baseline disparities in these outcomes.

2 Institutional Details, Methods, and Data

We use a difference-in-differences specification – within a narrow band of marginal candidates' margin of victory – comparing outcomes of traffic stops before vs. after a newly elected non-white councilmember, compared to the election of a white councilmember.

Our analysis therefore requires data that identify (close) contests between one white and one non-white candidate for city council and links that to municipal police agency policing outcomes. We draw on data from two main sources: the "Open Data Policing" platform and North Carolina administrative election records.

[1] Also, see Graham and Makowsky (2021) for a review of work on local government's financial reliance on criminal justice revenue and how that shapes outcomes like traffic citations.

[2] bjs.ojp.gov/content/pub/pdf/cbpp18st.pdf

2.1 Institutional Details

We first briefly outline relevant institutional details of municipal council elections in North Carolina to provide context for our discussion of methods. Many municipal council contests in the state aim to fill multiple seats. For instance, two seats might be open; in such a case, the top two vote-getters would be elected to office. This is important for our empirical design; we focus on the "marginal candidates": the lowest ranked winner and the highest ranked loser. If filling two seats, that would be the second and third ranked candidates. Multiple seats are filled in a single contest when elections are on an at-large, rather than district-based, basis. In North Carolina, that is true of roughly 86 percent of municipalities. The modal and (roughly) average and median number of members in a municipal council is five. Members serve two- or, more frequently, four-year terms.[3] Nearly all contests are non-partisan.

2.2 Data

2.2.1 Policing Data

As described on their website, "Open Data Policing aggregates, visualizes, and publishes public records related to all known traffic stops to have occurred in North Carolina since Jan 01, 2002. Data is available for most North Carolina departments and officers serving populations greater than 10,000." That last note is important as it is one of two reasons, revisited shortly, as to why we do not observe the universe of municipalities in the state.

The data report, for each traffic stop, the race, gender, and age of the individual stopped; the agency of the officer who made the stop; the date and time of the stop; the reason for the stop; and the action taken as a result of the stop. The data also include an anonymized identifier for the officer who made the stop, which will allow for inclusion of officer fixed effects in our analysis.[4] There are ten distinct reasons listed for stops. The three most common, accounting for over two-thirds of stops are: Speed Limit Violation, Vehicle Regulatory Violation, and Vehicle Equipment Violation.[5] There are five possible actions (listed here in order of frequency): Citation Issued, Verbal Warning, Written Warning, No Action Taken, and On-View Arrest. "Citation Issued" and "Verbal Warning" are the most common, together accounting for more than two-thirds of outcomes. Arrests are

[3]Many of these details are drawn from University of North Carolina School of Government's database on municipal form in North Carolina: https://www.sog.unc.edu/forms-of-government-paged .

[4]We do not observe any demographic information about the officers.

[5]The remaining reasons, in order of frequency, are: Investigation, Seat Belt Violation, Safe Movement Violation, Stop Light/Sign Violation, Other Motor Vehicle Violation, Checkpoint, and Driving While Impaired.

very rare (less than five percent). (Appendix Table A.3 reports frequencies of stops, stop purpose, and resulting action, overall and by race.) For our analysis, we create a dummy variable – called "Action Taken" – equal to one if the outcome is "Citation Issued" or "On-View Arrest" and equal to zero otherwise. That is our main outcome variable.

Because our focus is on city councils, we restrict the Open Policing data to stops made by officers from municipal police agencies, which city councils have jurisdiction over; in other words, we drop stops by officers from Sheriff's Departments, State Highway Patrol, and college/university police agencies.

In instances with multiple passengers, the data lists the race/ethnicity of each passenger, but without identifying the race/ethnicity of the driver in particular. To simplify our analysis and avoid ambiguity on this front, we restrict our data to observations where only a single race/ethnicity (and therefore single driver) is listed. We further restrict our data to only white, Black, or Latino individuals. Other race/ethnic groups are listed in the data, but form a very small share of the overall sample.

Finally, it is worth noting that whether a traffic stop escalates to a violent or fatal encounter with the police is also a critically important piece in documenting and understanding racism in policing (Knox et al., 2020); those outcomes, however, are not in our dataset and as such are not explored in this study.

2.2.2 Elections Data

Our elections data are administrative records obtained from the North Carolina State Board of Elections website. We draw on three forms of administrative elections data: election returns, candidate filing, and voter registration files.

The election returns data, most obviously, are essential for identifying when and where close city council elections occurred. They report candidate-level vote totals for every election in North Carolina, with candidates' names and the office they run for. We extract from these the candidates for city council from the years 2011-2019.

The candidate-level data does not include any personal data on the candidates, namely: race, gender, or partisan affiliation. Thus, we cannot immediately use the elections data to identify close elections *between white and nonwhite candidates*. Instead, we turn to voter registration files to fill in those details. In North Carolina, voter registration files include voters' race, ethnicity, gender, partisan affiliation, address, and more. One solution to attaching demographic information to our city council candidates would be to attempt to match council candidates from election returns to voter files by candidate name and city. A drawback of that approach is that candidates with more common names may match to multiple individuals in the voter registration files within a given city. To overcome that, we draw in a third dataset: candidate filings data. These data report the universe of candidates for public office in North Carolina each election year and the home address of these candidates. We therefore first match candidates by name and city to the candidate filings data to uncover their address. We can then match candidates by name and some more specific geography (we use zipcode) to the election returns, to minimize duplicate matches. It is worth noting that we do still fail to match some candidates; this can happen, for instance, as a result of discrepancies in how candidates' names are spelled in elections return data vs. the voter files (e.g., "Dan Jones" vs. "D. Brady Jones" or "Daniel B. Jones").

The candidate filings data are also important to our data construction, as they include some additional detail on the election itself – e.g., the number of seats being filled.[6]

2.2.3 Analysis Data

The resulting elections dataset includes the set of city council elections from 2011 to 2019 where one of the two marginal candidates (last winner and first loser) was white and the other not white. We identify 207 unique contests in 149 unique municipalities that fit this description. Amongst "close" contests (a margin of victory of less than ten percentage points), we observe 99 unique contests in 84 unique municipalities. Merging the elections data with the policing data, we lose some municipalities that do not manage their own police agencies (contracting to neighboring municipalities or under the jurisdiction of the sheriff's department) and also the small number of municipalities not covered by the policing data. That results in 60 total municipalities and 83 total elections, and 31 municipalities and 38 elections in contests decided by ten percentage points or less.

In merging election data with policing data, we create a panel around each election. Specifically, for each election, we include four years of policing data before the new councilmembers take office and the four years after; however, due to ambiguity in when councilmembers take office and due to shifts in police spending and policy in elections years (McCrary, 2002; Baicker and Jacobson, 2007), we omit policing data from election years.

2.3 Methods

Our main methodological approach is a difference-in-differences design, paired with the intuition of a regression discontinuity approach. We do not use a more typical cross-

[6]That is important because the elections data does not include an indicator variable for being elected in all years in our sample. In city council elections, often, multiple seats are being filled simultaneously, with the top N vote-getters filling the N seats. Thus, in the absence of an indicator for winning, knowing the number of seats being filled is critical for us to identify the "marginal" candidates.

sectional regression discontinuity design due to the relatively low number of municipalities and elections in our final dataset. The canonical close-elections regression discontinuity design relies solely on post-election variation and fits lines to candidates' margins of victory on either side of the victory threshold; doing so ideally leverages a large number of observations on either side of the cutoff to reliably fit lines to the data and identify the discontinuity at the cutoff. A difference-in-differences approach, comparing averages before and after an event, imposes less structure and as such is a better fit for a scenario with a smaller number of observations on either side of the cutoff.

As such, noting again that we construct a panel around each relevant election event, we take advantage of observing both pre- and post- election observations of each agency. Our difference-in-differences setup therefore compares changes in policing outcomes before vs. after an election between a white and non-white candidate in municipalities where the non-white candidate won (relative to cities where the non-white candidate did not).

We do, however, still draw on the regression discontinuity intuition that outcomes in narrowly contested elections are more plausibly exogenous than in the full range of contests. As such, while our estimating equation is a difference-in-differences specification, we restrict our attention to relatively narrowly decided contests (with margins of victory of less than 10 percentage points).[7]

The simplest representation of our estimating equation is:

$$\text{Action Taken}_{eit} = \beta_1(\text{Non-White Win}_i \times Post_t) + \beta_2\,\text{Post}_t + \theta_i + \tau_t \quad (1)$$

In the equation, e indexes police encounters, i indexes police agencies or municipalities, and t indexes time periods (relative to the election year). Action Taken$_{eit}$ is a dummy variable indicating whether a police encounter resulted in an "action taken" as defined above (citation or arrest). We include municipality-election (θ_i) and year fixed effects (τ_t).[8] β_1 is our coefficient of interest and captures the impact of a non-white candidate winning on police behavior.

For added robustness, we do also present results where we interact "post", "nonwhite win", and the interaction of those variables with the nonwhite candidate's margin of vic-

[7]A bandwidth of 10 percentage points was determined by collapsing the data to one observation per agency, measure post vs. pre changes in residualized outcomes (residualizing out the main set of fixed effects and controls used in our analysis). We then use the Calonico et al. (2014) method, positing a polynomial of degree zero (to match that our analysis does not fit lines to either side of the cutoff), to identify the optimal bandwidth. Our appendix documents robustness to other bandwidths.

[8]Note that "Non-White Win" itself is absorbed into municipality-election specific fixed effects. Because "post" is defined with respect to election timing, it is not absorbed into (calendar) year fixed effects.

tory (or loss), pushing the specification closer to a regression discontinuity, but still within the panel framework. In those specifications, as with standard regression discontinuity, identification occurs at the margin – that is, for cities with the closest election outcomes. In the results section, we refer to this as our RD-DiD approach.

Of course, the above equation would only estimate overall impacts of a new non-white councilmember on police behavior. Our interest in this paper is whether there is a differential impact on encounters involving Black and Latino drivers. As such, for our main specifications, we instead fully interact both Non-White $\text{Win}_i \times Post_t$ and $Post_t$ with dummy variables indicating whether the driver was Black or Latino.

In our main specifications, in addition to municipality-by-election and year fixed effects, we also include fixed effects for hour of the stop and month. Additional specifications add driver demographics (age and sex), stop purpose fixed effects, and officer fixed effects.

Only the richest specification includes officer fixed effects. A comparison of specifications with vs. without officer fixed effects will help us consider the mechanism driving our results. If any results that we observe in specification without officer fixed effects go away with the inclusion of officer fixed effects, that would indicate that effects are not occurring *within* officers and instead may be driven by shifts in composition of a police force. If effects survive in specifications with officer fixed effects, that suggests that the impacts of representation in city council impacts individual officer behavior.

3 Results

3.1 Descriptive Analysis: Baseline Racial/Ethnic Gaps in Outcomes of Traffic Stops

Before turning to our analysis of the impacts of city council representation, we begin by examining the overall racial disparities in policing behaviors, especially, the action taken after a stop.

This is already partially summarized in Appendix Table A.3, which documents that Black drivers account of roughly 42% of stops in our sample (nearly twice the Black share of the population), Latino drivers account for 9% (close to their share of the population), and white drivers account for 49%. It also reports differences across groups in action taken conditional on a stop, but because there are substantial differences in the reasons these groups are stopped (also shown in that table), we turn to regression analysis to assess baseline differences.

Table 1 presents the baseline racial disparities in the actions taken following stops for Black drivers and Latino drivers, respectively. Within the whole dataset of stops (that is, without restricting to our close elections sample), Black drivers are approximately 1.5 to 3.8 percentage points more likely to have actions taken against them after a stop compared to white drivers after controlling the core fixed effects[9]; the Latino-white disparity is greater than Black-white gap, amounting to approximately 11.1 to 14.3 percentage points. The coefficients vary based on the inclusion of various fixed effects, the demographics of the drivers, and the specific data sample that we used. Of course, as noted, Latino drivers represent the smallest share of those pulled over and so the measurement may be noisier.[10]

3.2 Effects of Non-white Council Representation

In light of the substantial racial and ethnic gaps in stops and "Action Taken" after a stop, we now examine whether increased representation of non-white councilmembers can help reduce this disparity.

3.2.1 Impacts on Composition of Drivers Stopped

Table 2 shows the effect of a victory by nonwhite council candidates on the number of stops executed by police officers. We conduct analysis using data collapsed at the officer level. After the election of nonwhite council members, there is no significant change in the overall number of stops. However, police officers made fewer stops involving Black drivers. Our preferred estimate in Table 2 is Column 4 (with officer-level fixed effects), which suggests a 16.5% decrease in the number of stops made against Black drivers. On average, each officer stops 10 fewer Black motorists after a narrow victory of a minority council member. We find no impact on Latino drivers. Within our sample from North Carolina, nonwhite candidates are most often Black. Therefore, our finding is that the election of (typically) a Black council member influenced police behavior towards Black drivers.

[9] They include month, agency, hour of stop, and election contest fixed effects.

[10] It may also be that disparities are significantly larger for the Latino population as a result of language differences; from American Community Survey data, roughly half of adult North Carolinians who speak Spanish report speaking English "less than very well".

3.2.2 Impacts on Outcomes of Traffic Stops

Understanding that having a Black council member reduces the number of stops involving Black drivers, we now turn to an analysis on outcomes of these stops. Table 3 displays the differential effects of a victory by a non-white council candidate on the actions taken post-stop for Black and Latino drivers, as compared to the baseline group of white drivers. As a reminder, "action taken" here is defined as the driver receiving a citation or being arrested as opposed to receiving a warning or even no action.

The Black-white gap in likelihood of action taken shrinks by 2.2 to 2.8 percentage points following the election of a nonwhite council member – large effects relative to the baseline gaps documented in Table 1. The estimates in Columns 2 and 3 (without vs. with officer fixed effects) are quite close; that suggests that the main effects are derived from individual officer behavioral changes, rather than shifts in the composition of the force via hiring and firing. In the appendix, we test for robustness across different bandwidths. Table A.1 shows that the estimates are quite similar across a narrower or wider bandwidth. Column 4 of Table 3 reports the results of the combined RD-DiD approach, where all DiD variables are further interacted with candidates' margin of victory. The result, at least for the Black-white gap, is remarkably similar to Column 3.

Figure 1 employs an event study approach to our data. We modify a version of equation (1), substituting the "Post" indicator with a series of variables indicating the number of years before/after the election the action was taken following a stop. Panel (a) reports Black-white gaps; Panel (c) reports Latino-white gaps. Panels (b) and (d) adopt a similar event study approach to the RD-DiD specifications – where all relevant variables are interacted with margin of victory such that the main variables are identified only for very close elections.

In short, across the four panels, we find no evidence of pre-trends. The post-period results largely mirror what we have already documented: Black-white gaps decline after the election of a nonwhite (most often Black) candidate, Latino-white gaps do not. Appendix Figures A.1 and A.2 further report event studies for additional bandwidths and again lead to the same conclusions.

To gain a deeper understanding of the decrease in racial disparities in actions taken post-stop, we examined all five possible outcomes following stops, rather than simply grouping outcomes into "action taken" or not. Results are reported in Table A.2. Most notably, we find that the reduction in "action taken" amongst Black drivers appears to be a substitution away from officers issuing citations and towards verbal warnings.

4 Conclusion

From prior research, there is ample evidence of racial/ethnic disparities in the likelihood of being stopped in traffic and also in the likelihood of being searched conditional on being stopped (Baumgartner et al., 2017). Disparities in the likelihood of being searched are especially problematic in that they may open a path to further disparities in broader contact with the criminal justice system, or, tragically, violent or fatal encounters with the police. And, yet, per Baumgartner et al. (2017) searches occur for roughly three percent of traffic stops. A much larger share of stops result in some form of consequence based on the purported reason for the stop: e.g., a verbal warning or a citation. These outcomes are the focus of our paper.

Even for "routine" stops that do not end in a search or violence, any disparities in the likelihood of being issued a citation are further magnified by the different downstream impacts this may have on different groups in a broader system of racism. Baumgartner et al. (2018) and Goffman (2014) note how a traffic encounter for individuals, for instance, with prior contact with the criminal justice system can lead to job loss, eviction, or jail time. As such, it is critical to document the scope of disparities in traffic citations – especially in light of potential disparate downstream impacts – and understand paths towards reducing them. Our paper documents one such path: greater racial representation in elected office.

Specifically, we draw on administrative data on the outcomes of traffic stops from municipal police agencies in North Carolina during the 2010's. We first document large baseline disparities. Black drivers are 1.5 to 3.8 percentage points more likely to suffer material consequence of the stop (citation or, less frequently, arrest), depending on the set of controls included. Latino-white gaps are even larger, ranging from 11.1 to 14.3 percentage points.

We then turn our attention to our main focus: can the political process play a role in reducing these gaps? Specifically, because municipal police agencies are supervised by municipal government (mayors and city councils), we ask: does increased nonwhite representation on a city council impact racial/ethnic gaps in traffic stops and traffic stop outcomes? With respect to the Black-white disparities in traffic stops, the answer is a very clear "yes". The reduction in the Black-white gap in the likelihood that a traffic stop results in a citation or arrest is almost large enough to eliminate the estimated baseline gap altogether. In particular, conditional on a wide array of controls, Black drivers are less likely to receive a citation and more likely to instead receive a "verbal warning". These effects could occur either through shifts in the composition of the police force or via changes in individual officer-level behavior. We find evidence that results are largely driven by the latter.

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Table 1: Baseline Disparities in Action Taken After Stop, Relative to White Drivers

VARIABLES	(1) Action Taken	(2) Action Taken	(3) Action Taken	(4) Action Taken	(5) Action Taken	(6) Action Taken
Black Driver	0.015**	0.028***	0.021***	0.029***	0.038***	0.028***
	(0.007)	(0.005)	(0.005)	(0.003)	(0.009)	(0.003)
Lat. Driver	0.136***	0.143***	0.126***	0.128***	0.111***	0.124***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)
Observations	6,367,686	6,367,686	6,309,597	6,297,601	4,954,573	1,288,024
R-squared	0.078	0.148	0.159	0.269	0.266	0.287
Core FEs	Yes	Yes	Yes	Yes	Yes	Yes
Driver Demos.	No	No	Yes	Yes	Yes	Yes
Purpose FEs	No	Yes	Yes	Yes	Yes	Yes
Officer FEs	No	No	No	Yes	Yes	Yes
Election Sample					Yes	Yes
Close Election Samp.						Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Impacts of Non-White Council Candidate Win on Officer-level Stops

VARIABLES	(1) Total Stops	(2) Total Stops	(3) Black Stops	(4) Black Stops	(5) Lat. Stops	(6) Lat. Stops
Post X NW Win	-14.035	-8.719	-8.890*	-10.123**	-1.104	-0.540
	(9.771)	(7.117)	(4.494)	(4.192)	(0.742)	(0.621)
Observations	21,239	17,857	21,239	17,857	21,239	17,857
R-squared	0.031	0.683	0.062	0.673	0.052	0.656
Officer FE's	No	Yes	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Average number of total stops at officer level in estimation sample is 61.

Table 3: Impacts of Non-White Council Candidate Win on Post-Stop Police Action

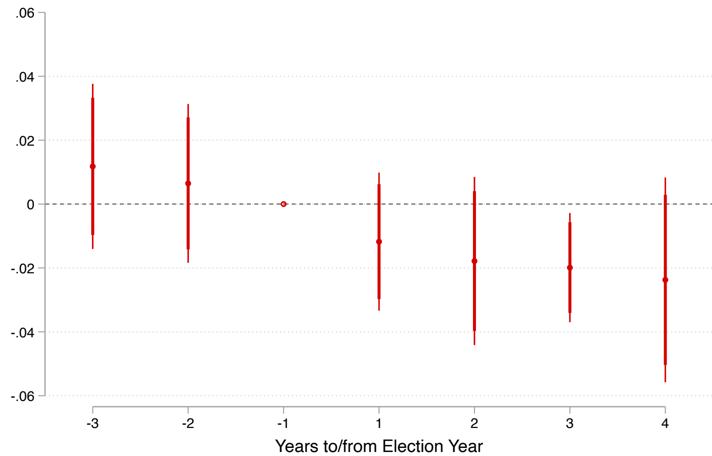
VARIABLES	(1) Action Taken	(2) Action Taken	(3) Action Taken	(4) Action Taken
Post X NW Win	0.053	0.029	0.007	-0.036
	(0.032)	(0.029)	(0.025)	(0.039)
Post X NW Win X Black Driver	-0.026*	-0.028**	-0.022***	-0.022***
	(0.013)	(0.011)	(0.006)	(0.006)
Post X NW Win X Lat. Driver	-0.013	0.003	0.014	0.025
	(0.029)	(0.028)	(0.021)	(0.027)
Observations	1,301,439	1,289,805	1,288,024	1,288,024
R-squared	0.073	0.154	0.288	0.288
Core FE's	Yes	Yes	Yes	Yes
Stop Purpose FE's	No	Yes	Yes	Yes
Driver Demos.	No	Yes	Yes	Yes
Officer FE's	No	No	Yes	Yes
Method	DiD	DiD	DiD	RD-DiD

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

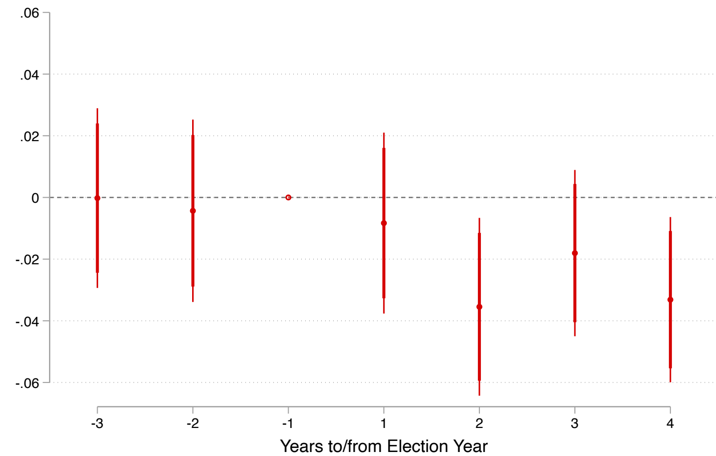
Notes: "Action Taken" is a binary variable equal to one if drivers are given a citation or arrested; equal to zero if no action is taken or drivers receive a verbal or written warning. "Core FE's" includes month, agency, hour of stop, and election contest fixed effects. "Driver Demos." includes sex and age (which enters as a second degree polynomial).

Figure 1: Event Studies: Action Taken, Black Drivers

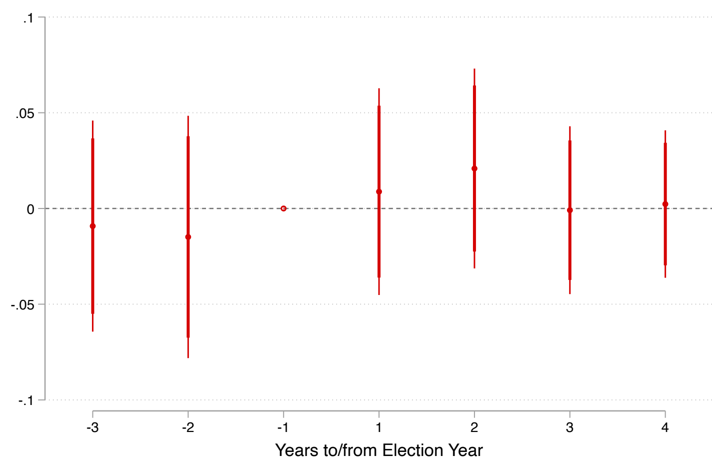
(a) Black-White: DiD within margin of 10pp.



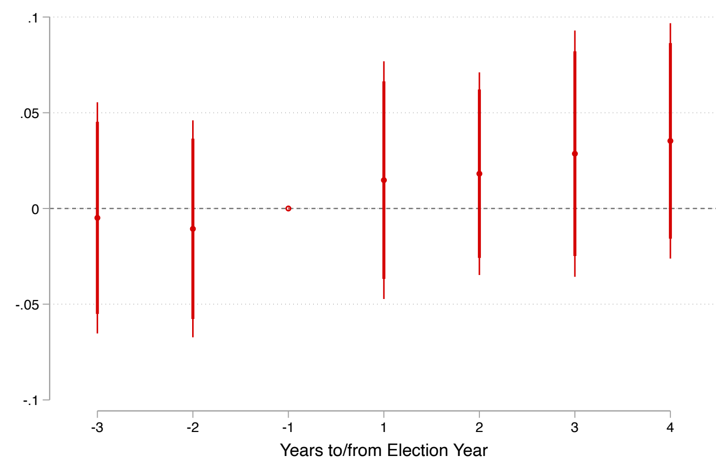
(b) Black-White: RD-DiD within margin of 10pp.



(c) Lat.-White: DiD within margin of 10pp.



(d) Lat.-White: RD-DiD within margin of 10pp.



A Appendix: Additional Results

Table A.1: Impacts of Non-White Council Candidate Win on Post-Stop Police Action, Different Bandwidths

VARIABLES	(1) BW=0.05	(2) BW=0.10	(3) BW=0.2
Post X NW Win	0.004	0.007	0.020
	(0.035)	(0.025)	(0.015)
Post X NW Win X Black Driver	-0.018***	-0.022***	-0.015**
	(0.006)	(0.006)	(0.007)
Post X NW Win X Lat. Driver	0.007	0.014	-0.004
	(0.027)	(0.021)	(0.017)
Observations	1,076,235	1,288,024	2,391,568
R-squared	0.296	0.288	0.284
Core FE's	Yes	Yes	Yes
Stop Purpose FE's	Yes	Yes	Yes
Driver Demos.	Yes	Yes	Yes
Officer FE's	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "Action Taken" is a binary variable equal to one if drivers are given a citation or arrested; equal to zero if no action is taken or drivers receive a verbal or written warning. "Core FE's" includes month, agency, hour of stop, and election contest fixed effects. "Driver Demos." includes sex and age (which enters as a second degree polynomial).

Table A.2: Impacts of Non-White Council Candidate Win on Post-Stop Police Action, All Possible Outcomes

VARIABLES	(1) None	(2) Verbal Warn.	(3) Written Warn.	(4) Citation	(5) Arrest
Post X NW Win	0.004	-0.008	-0.003	0.008	-0.002
	(0.003)	(0.015)	(0.022)	(0.024)	(0.002)
Post X NW Win X Black Driver	0.001	0.030***	-0.010	-0.022***	0.001
	(0.002)	(0.010)	(0.009)	(0.006)	(0.002)
Post X NW Win X Lat. Driver	-0.008*	-0.003	-0.003	0.011	0.003
	(0.004)	(0.025)	(0.017)	(0.022)	(0.003)
Observations	1,288,024	1,288,024	1,288,024	1,288,024	1,288,024
R-squared	0.151	0.340	0.235	0.297	0.091
Core FE's	Yes	Yes	Yes	Yes	Yes
Stop Purpose FE's	Yes	Yes	Yes	Yes	Yes
Driver Demos.	Yes	Yes	Yes	Yes	Yes
Officer FE's	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

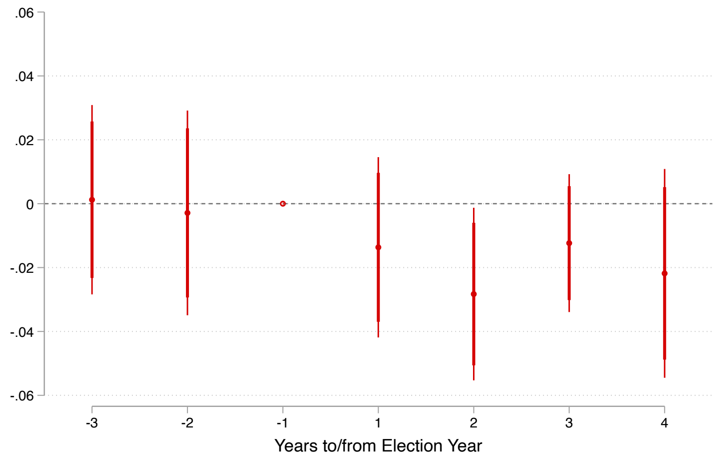
Notes: "Action Taken" is a binary variable equal to one if drivers are given a citation or arrested; equal to zero if no action is taken or drivers receive a verbal or written warning. "Core FE's" includes month, agency, hour of stop, and election contest fixed effects. "Driver Demos." includes sex and age (which enters as a second degree polynomial).

Table A.3: Frequencies of Stops and Stop Outcomes by Race

	Race			
	Black	Latinx	White	Total
N	2,702,294 (42.4%)	545,682 (8.6%)	3,119,710 (49.0%)	6,367,686 (100.0%)
Purpose				
Checkpoint	29,160 (1.1%)	21,224 (3.9%)	27,486 (0.9%)	77,870 (1.2%)
Driving While Impaired	9,398 (0.3%)	4,883 (0.9%)	16,884 (0.5%)	31,165 (0.5%)
Investigation	113,417 (4.2%)	43,919 (8.0%)	99,986 (3.2%)	257,322 (4.0%)
Other Motor Vehicle Violation	116,636 (4.3%)	38,303 (7.0%)	110,641 (3.5%)	265,580 (4.2%)
Safe Movement Violation	174,532 (6.5%)	42,048 (7.7%)	235,622 (7.6%)	452,202 (7.1%)
Seat Belt Violation	79,086 (2.9%)	11,712 (2.1%)	99,198 (3.2%)	189,996 (3.0%)
Speed Limit Violation	710,036 (26.3%)	143,541 (26.3%)	1,115,246 (35.7%)	1,968,823 (30.9%)
Stop Light/Sign Violation	193,493 (7.2%)	42,423 (7.8%)	260,903 (8.4%)	496,819 (7.8%)
Vehicle Equipment Violation	408,486 (15.1%)	71,736 (13.1%)	371,343 (11.9%)	851,565 (13.4%)
Vehicle Regulatory Violation	868,050 (32.1%)	125,893 (23.1%)	782,401 (25.1%)	1,776,344 (27.9%)
Action				
Citation Issued	1,230,709 (45.5%)	320,060 (58.7%)	1,462,820 (46.9%)	3,013,589 (47.3%)
No Action Taken	90,676 (3.4%)	16,658 (3.1%)	83,667 (2.7%)	191,001 (3.0%)
On-View Arrest	68,772 (2.5%)	13,876 (2.5%)	53,368 (1.7%)	136,016 (2.1%)
Verbal Warning	967,812 (35.8%)	150,139 (27.5%)	1,040,241 (33.3%)	2,158,192 (33.9%)
Written Warning	344,325 (12.7%)	44,949 (8.2%)	479,614 (15.4%)	868,888 (13.6%)

Figure A.1: Event Studies: Action Taken, Black Drivers

(a) DiD within margin of 5pp.



(b) DiD within margin of 20pp.

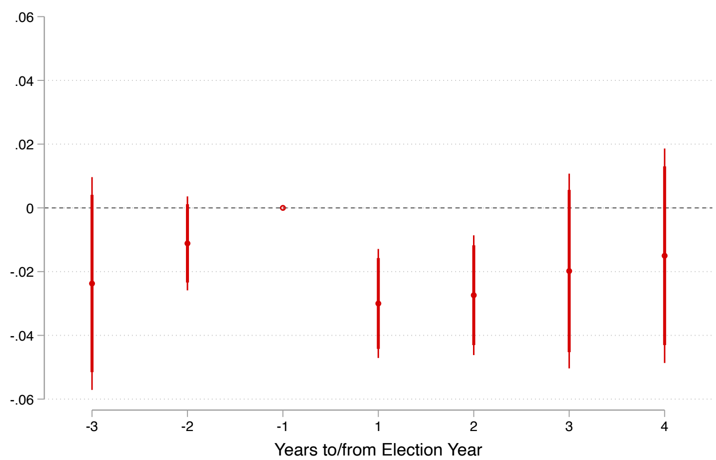
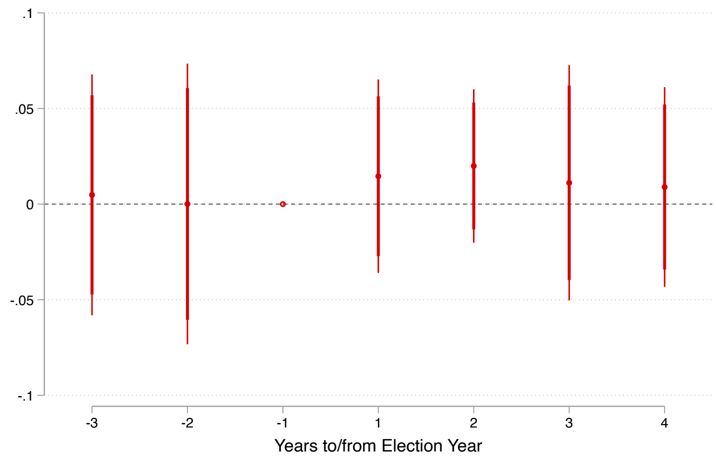


Figure A.2: Event Studies: Action Taken, Latino Drivers

(a) DiD within margin of 5pp.



(b) DiD within margin of 20pp.