Black Mayors and Crime

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Abstract

Local elections are often contested on the grounds of public safety, but do elected officials have any power to curb crime? Black mayors have particular interest in the issue because Black communities are victimized by high levels of crime and fragile police-community relations. Using data on elections of first-time Black mayors, I find that police forces add more Black officers, a finding that is especially true for mayors with executive authority. Officers arrest 48 fewer potential Black offenders per 10,000 Black residents for crimes where they have the ability to exercise discretion, a finding that is commensurate with the overall reduction in crime. This effect is not visible for similar white arrests. Using changes in the levels of arrests and officers induced by pivotal Black elections, I then estimate the correlation of an additional officer on race-specific arrests. An additional Black officer is related to large reductions in discretionary Black arrests, perhaps suggesting increasing the presence and visibility of Black officers may offer a solution to the "over-policing, under-policing" problem Black communities tend to face.

Keywords: Mayor, Race, Police, Crime, Local Government
JEL Classification Codes: J15, K42, H76

1 Introduction

Black communities tend to face a dual public safety problem referred to as "over- and under-policing." Black individuals are victimized by crime at high rates (Harvey and Mattia 2022). Efforts to reduce the cost of violent crime by the police lead to increased interactions with civilians. These increased interactions sometimes lead to unfortunate and visible encounters which serve to further erode community trust. Because of victimization by criminals and a precarious reliance on law enforcement, Black residents have called for reform, starting with increased representation. A natural question is then: what might Black policymakers do that other policymakers might not commit to?

Identifying the effects of Black candidates has been of interest to scholars for decades. In interracial elections, "race and crime" have been the "totemic issues" underlying the election.[1] However, identifying this effect has been difficult for multiple reasons. Arrests data are voluntarily reported and can be missing for many consecutive years. From an empirical perspective, elections involving Black candidates are high-information (in the political economy sense), highly polarizing events. This works to the disadvantage of traditional regression discontinuity designs, which require quasi-randomness at the 50 percent vote-share threshold. Additionally, while Black mayors may want to improve the lives of Black Americans, this may involve different policy mandates across time as the existence of temporal heterogeneity may confound the effects of Black mayors as attitudes toward particular offenses or incarceration change.

With these challenges in mind and taking advantage of recent advances in difference-in-differences (DD) estimation (Callaway and Sant'Anna (2021)), this paper uses a DD design leveraging the timing of when a Black candidate is elected mayor for the first time to show that Black mayors have an impact on the diversity of the police force and that this translates into reductions in quality of life arrests and violent arrests for Black Americans. Quality of

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[1]See https://www.nytimes.com/1992/09/20/nyregion/dinkins-and-the-police-a-campaign-issue.html?searchResultPosition=25

life arrests are a set of offenses where one would expect officers to exercise some measure of discretion in executing an arrest. I compare the set of cities that elect a Black candidate in the same year against a set of cities that did not yet elect a Black candidate at the time period in question.[2]

I begin by examining whether Black mayoralties change arrest rates by race and according to crime severity. Across a range of crimes and crime groupings, Black Americans are arrested less after a Black election absolutely and relative to white Americans when comparing the same offense. There are 559 fewer Black arrests annually for quality of life offenses where officers may exercise some discretion. Violent arrests for Blacks display a similar pattern. When I estimate changes to the number of offenses known to police, a similar pattern arises whereby across most crime categories, the number of offenses falls. For example, total index crime falls by slightly more than 3,000 offenses, paralleling arrest findings. With this information in hand, I investigate public finance and public safety outcomes that may influence my findings. Interestingly, the share of expenditure devoted to police protection is reduced by 1.3 percentage points in the 10 years following a breakthrough election; however, this is not due to changes in the amount spent on police protection per person, indicating that additional expenditure is being devoted to other resources. I further observe positive, small, and statistically imprecise changes in the number of officers after a breakthrough election but this masks changes to the number of Black officers once a Black candidate assumes office. Using data that tabulates officer counts by race and by sex (but not their intersection consistently), I show that breakthrough elections increase the number of Black officers by 2.75 per 10,000 Black residents. Taken at the average Black population for treatment cities, this implies an increase of 32 Black officers. Importantly, the effect is larger and more precise when limiting the analysis to the set of cities where the mayor has power over the police department.

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[2]For example, I can compare outcomes in cities that elected a Black candidate in 1990 (Chesapeake (VA), New York (NY), New Haven (CT), Seattle (WA), and Trenton (NJ)) to outcomes in cities that did not yet elect a Black candidate by 1991 for the year 1991, and repeat that same procedure for every year in the data.

I conclude by determining the relationship between size of the police force or diversity of the police force and quality of life and violent arrests. I estimate the mayor-induced changes to the total number of race-specific arrests and the total number of officers, then to Black, female, and white officers, for each treatment city in the data. I then fit a line through those estimates and find that increasing the number of officers is related to reductions in violent arrests and homicide arrests but the relationship to quality of life arrests is tenuous, marking a slight difference from earlier work (Chalfin et al. 2022).[3] I find a strong negative relationship between increasing Black officers and the number of quality of life arrests, suggesting a dimension of officer discrimination. An increase of one Black officer leads to an imprecise reduction of one violent Black arrest. When combined with the increase in Black officers, this implies that Black officers are associated with a reduction of 63 violent crimes on average. These correlations suggest that a particular "non-costly" solution to police-community relations may be hiring a more diverse slate of cadets though this may be tougher for candidates who belong to ethnic groups outside of the one a law enforcement agency may be recruiting.

A principal policy of Black mayors is policing, crime prevention, and community relations (Bailey 1990). Black mayors often appoint Black police chiefs, hire more Black police officers, and institute policies to reduce discriminatory behavior and deter crime in Black and other disadvantaged neighborhoods (see Donohue and Levitt (2001)). However, less is known as to whether these changes are effective at achieving their goals. It is theoretically unclear how these policy changes affect Black residents and even white residents. Additional Black officers might decrease discriminatory behavior against Black residents, lowering the probability of arrest where officer discretion is involved, but the increase in officers and the change in policing tactics increases the probability of detecting a crime in the spirit of Becker (1968). From a political perspective, mayors are elected for a broad range of policies (only one being

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[3]Throughout I use murder and homicide interchangeably, although homicide encapsulates other non-intentional and justified acts. These cases are excluded.

policing) and often appoint police chiefs. This creates a layer of accountability between law enforcement and the voting public that may lead to divergence from the politically desirable outcome.

My research adds to the literature on management quality, especially as it relates to policing. Ornaghi (2019) demonstrates that the implementation of a merit system as opposed to a spoils system reduces crime rates by limiting political influence. This might be unattractive in this setting for two reasons. Merit systems in police hiring tended to negatively impact Black applicants, and if Black officers are focal in reducing excessive discretionary arrests, then this would be a negative feature. Second, if politicians are able to influence behavior in the sense that white officers also exercise greater caution due to perceived changes in discipline, this may also improve police–community relations. Kapustin, Neumann, and Ludwig (2022) show that changes to police leadership in Chicago generate sizable effects on policing outcomes. In this context, the management changes are at the commander level, but mayoral elections likely have their own independent effect on downstream managerial outcomes. Other work on management quality focuses on the mayor specifically. Colombo and Tojerow (2020) find that improving mayoral accountability decreases crime, whereas Akhtari, Moreira, and Trucco (2022) find that political influences can reduce the quality of the provision of public goods through the link of political and bureaucratic turnover.

A second literature deals directly with police composition and effectiveness. Chalfin et al. (2022) find that increases in police manpower cause a decrease in arrests for violent crimes, generating a "double dividend" of lower crime and fewer incarcerations; however, this effect wanes when the Black population increases, supporting the hypothesis that Black communities are "under- and over-policed." This work buttresses that finding but notes that changes to department diversity in places with significant Black populations have the ability to restore the double dividend consistent with the findings of Ba et al. (2021), Harvey and Mattia (2022), and Miller and Segal (2019). Donohue and Levitt (2001) examine the relationship between the racial composition of the police force and race-specific patterns

of arrests. They include a covariate for having a Black mayor and find 30 fewer non-white arrests per 1000 non-White residents and a reduction of 10 white arrests per 1000 white residents, though one should not assign a causal interpretation to the estimate. Other work, such as Fryer (2019), shows that minority interactions with the police lead to uses of force more often than interactions involving white civilians suggesting a particular dimension in which diversity is important.

Lastly, this work adds to the literature on Black representation and political economy. Closely related, Hopkins and McCabe (2012) find that the election of Black candidates for mayor does not lead to public finance outcomes different than a counterfactual white candidate, with a noted exception of and increase in Black officers. This work is able to replicate this finding, noting that mayors elected more recently tend to mute the effects found from earlier elections, and then extends Hopkins and McCabe's work by determining if these changes lead to better outcomes within the populace. Other work notes particular cases where Black mayors came to power and often finds similar conclusions, uniformly highlighting the heightened criminal justice aspect of Black elections (see Reft (2008), George (2004), and Browning, Marshall, and Tabb (2021)). Police and public safety have been pivotal issues long before Black populations commanded significant political power. Derenoncourt (2022) shows that before any Black candidates became mayors in the modern period,[4] larger Black populations caused by the Second Great Migration tended to be met with increases in police manpower, a reason why the issue is a pivotal topic in breakthrough elections.

The rest of the paper proceeds as follows. In Section 2 I provide information about the powers available to mayors to change policing outcomes and why changing departmental representation is a primary focus. In Section 3 I describe the data. Section 4 highlights my empirical approach. Section 5 describes the results, and in Section 6 I give some concluding remarks.

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[4]As opposed to Reconstruction; see Logan (2020) for more.

2 Background

2.1 Institutional Powers

Mayors walk no beats, carry no cuffs, and investigate no crimes. So how might they influence policing patterns and incentives? The candidate analog of theory proposed by Facchini, Knight, and Testa (2020) provides some intuition. They found that Black arrest rates fell in counties covered by the Voting Rights Act (VRA). In their model, the VRA shock to voter preferences driven by the size of th Black population induces the election of chief law enforcement officers (CLEOs) whose policies improve the treatment of Black residents. Importantly, in their context, this is driven by elected CLEOs and not appointed ones. While they note that electing policymakers such as a mayor is, in effect, a selection of a bundle of goods, we may still expect to see changes in the treatment of Black residents if the elected candidate is Black.

In many counties and smaller jurisdictions, the sheriff is the most powerful elected person, similar to the mayor in the general municipal context. We can, for example, observe the Los Angeles municipal code to understand the mayor's role in overseeing law enforcement. Among a host of powers, the mayor can "(a) exercise management authority over all departments, agencies, and appointed offices, ..., (c) appoint chief administrative officers...members of the boards of commissioners...[and] (e) remove from office any chief administrative officer or commissioner."[5] However, all mayors are not created equally. The enterprising reader can sleuth through the Vallejo (CA) muncipal code, for example, to find the one line of additional authority granted to its mayor: "The Mayor shall preside at meetings of the Council, shall have a vote in all matters before the Council, and shall be recognized as the head of the City government for all ceremonial purposes."[6] Thus some mayors are powerful by rules and

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[5]https://codelibrary.amlegal.com/codes/los_angeles/latest/laac/0-0-0-571. This is among perhaps many other pertinent powers, including the issuance of executive directives.

[6]https://library.municode.com/ca/vallejo/codes/municipal_code?nodeId=CH_ARTIIIMACO_S318PODUMA

others only have the potential to be powerful through influence.[7]

The rest of this section will focus on mayors who serve as chief executives to highlight policies Black mayors may follow and specific bureaucratic impediments that serve to limit their authority. Beginning with the latter, the same rules that empower mayors often serve as roadblocks to implementing their most preferred policy. Returning to Los Angeles as an example, suppose that the mayor wants to appoint a CLEO who aligns with the mayor's policing ideology. Then the mayor can appoint members to the Board of Police Commissioners, who would then recommend candidates to the mayor for appointment, and the mayor would then offer a recommendation to the city council. A similar stepwise process is required for the removal of a police chief, an action that can be thwarted by a two-thirds vote of the council. Such a situation played out when Tom Bradley—Los Angeles' first Black mayor, a mayor of 18 years at the time, and a former cop himself—attempted to remove then-police chief Daryl Gates in the wake of the Rodney King incident.[8]

2.2   Officers

Improving the diversity of the police force can lead to improvements in community relations (e.g., see Miller and Segal (2019), Harvey and Mattia (2022), and Ba et al. (2021)). Figure 1 shows changes to the shares of Black and female officers beginning in 1987 using the Law Enforcement Management and Administrative Statistics (LEMAS) for cities with populations of at least 75,000 and a 4 percent Black population in 1970. Both series have increased over time, but the share of Black officers has stagnated since 1993. Increases in each series can be attributed to affirmative action litigation that began decades earlier, with the explicit goals of increasing diversity to prevent riots and improving police–community relations (Cox, Cunningham, and Ortega 2022; McCrary 2007). Settlement of affirmative action litigation

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[7]In situations of low information, the mayor may be the only recognizable public figure, even when the mayor has similar powers to other council members. In this case, the mayor likely exerts influence on downstream elections.

[8]Presumably, the mayor would have been able to install all of the members on the Police Board of Commissioners, leaving minimal friction in decision making.

sometimes coincided with the election of a Black candidate. For instance, the city of New Orleans settled a 10-year-old lawsuit during the Ernest Morial administration to correct representation gaps in the city's police department supervisory ranks, which was 2.5 percent at the time (Piliawsky 1985).

An important point is that because the mayors themselves are Black, they are likely or have been subject to police-specific discrimination.[9] This means they have (in general, stated) an interest in changing department behavior to improve their constituents' satisfaction, and by extension, decreased or less combative officer interactions improve long-term outcomes for residents.

3 Data

In this paper, I study the relationship between the election of a city's first Black mayor and racial patterns of law enforcement by exploiting the variation in election timing and geography. I begin by restricting my inquiry to cities whose populations were greater than 75,000 in 1970 and that had a Black population of at least 4 percent in 1970. This leaves 63 cities that elected a Black candidate and 92 cities that did not elect a Black candidate but meet the population threshold. Figure 2 illustrates the timing of when Black candidates assumed office from 1974 to 2018 grouped into decadal intervals, with each block representing one year of the sample. Both within and across decades we observe variation by geography and Black population. For instance. in the 1970s, we observe cities with Black population shares of less than a quarter (e.g. Los Angeles and Raleigh) and cities with near-Black majorities (Atlanta), and these cities cover all four census regions. Boxes in bold indicate being within or after the term of a Black mayor.[10] Treatment is defined as an absorbing state; so once a

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[9]Wellington Webb commented during an interview, "Racism in Colorado and Denver specifically has always been more covert than overt. There is rarely a black male who has not had some bad experience with a police officer, and being called a "n——" doesn't qualify because particularly in my day that was as common as a salad before dinner." His day as mayor of Denver began in 1991.

[10]Figure 8 in the Appendix displays the set of cities that have elected a Black candidate since 1970.

city elects a Black candidate, the city remains treated throughout the sample. For example, Clarence Lightner assumes the mayoralty of Raleigh at the end of 1973, serves for two years, and is not re-elected, but Raleigh's treatment status from 1974 onward is "treated." Also note that some cities have boxes that are always bold. This indicates that their proportion of treatment status is one; so they will not serve as treatment or controls here due to a lack of pre-treatment observations.

I rely on various sources to conduct my analysis. Information on elections mainly comes from Vogl (2014), the most comprehensive source of Black mayors of medium-sized and large cities. Vogl (2014) contains the winner and runner-up in elections for all cities with at least 50,000 residents in 1960 and a Black population share of at least 4 percent. In addition, his work includes both candidates' race, vote shares, and party affiliation. Much of those data are derived from Ferreira and Gyourko (2009), who mailed surveys to all cities with a population greater than 25,000 that directly elect a mayor. The Vogl (2014) data end in 2010; so I supplement this with election return information from various sources, including newspaper archives that explicitly mention a candidate being the first elected Black mayor; local, county, and state election websites; and the website ourcampaigns.com "whose mission is to collect and make available information about all official elections, historical, current and ongoing." The website often contains photographs of candidates, but I then further confirm a candidate's racial identity through web search or explicit mention. In other words, the photograph assists in determining a candidate's race but the ultimate factor is whether the candidate or external sources identify the candidate as Black. My main concern for this analysis is the race of the winning candidate, but I confirm the race of the runner-up whenever possible.

I obtain local government expenditure and revenue figures from the Annual Survey of Local Government Finances (ASG) and police and total employment figures from the Annual Survey of Public Employment and Payroll (ASPEP). The surveys are mailed yearly to local governments. Large governments, such as the ones being studied, have probabilities of being

surveyed near one. Reporting on the number of sworn police officers begins in 1977, but figures on total employees dedicated to police protection begin earlier. ASPEP was not collected in 1996 due to a change in reporting date; so data for that year are interpolated from the adjacent years.

My main outcomes are arrests by race and offenses known to law enforcement. I use Kaplan's (2020) concatenated files for the Uniform Crime Reporting (UCR) arrests by age, sex, and race, 1974-2018, and the UCR program data for offenses known and clearances by arrest. The data contain arrest information by age and juvenile status for over 40 different crime categories. Because some crimes are not crimes in other jurisdictions, and there are different reporting patterns across agencies, I construct two additional arrest variables which I denote as arrests for violent crimes and arrests for quality of life crimes. Violent arrests are the annual sum of aggravated assault, non-negligent homicide, rape, and robbery. Quality of life arrests are the annual sum of suspicion,[11] vagrancy, vandalism, gambling, prostitution, liquor violations, curfew violations, loitering, and drunkenness.[12] I only consider municipal police departments and exclude county, state, and other agencies[13] that may have overlapping jurisdictions but different duties and are not, most importantly, under the authority of police chiefs appointed by the mayor, city council, and/or city manager. Notably, this also excludes sheriff's departments whose leader is generally popularly elected separately from the local executive. In addition, I exclude agencies that are reorganized over the sample period, since reorganization may involve being absorbed by a county agency or dissolved altogether. UCR data are voluntarily self-reported by each agency; however, each agency has the incentive to accurately report arrests because they are considered in federal funding and grant requests.

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[11]The FBI defines 'suspicion' as an arrest "for no specific offense and released without formal charges being placed."

[12]Chalfin et al. (2022) construct a similar "quality of life" offense including many similar crimes. My initial construction notably excludes drug possession. I do this because we may believe that election of a Black candidate should induce fewer quality of life arrests, but there may be substitution at the possession–sale threshold whereby we may conclude that possession arrests for Black residents increase in the wake of a breakthrough election. I observe in table 2 that sale of cannabis does decrease; however identifying the effects of possession is more difficult because collection of the outcome happens later.

[13]For example, airline and university police

Most agencies submit crime reports to a state UCR program.[14] The state UCR program forwards the data to the FBI after aggregating offenses to uniform definitions.

The offenses known to law enforcement and clearance by arrest data are reported beginning in 1960. It is a compilation of offenses reported to law enforcement agencies and should not be considered as the totality of crime as many crimes go unreported according to victimization data. As with the arrests data, I restrict my attention to municipal police departments and offense records beginning in 1974. The offense data contain reports for "index" crimes, which are the sum of violent crimes—homicide, forcible rape, robbery, and aggravated assault—or property crimes—arson, burglary, larceny-theft, and motor vehicle theft. Because they are the sum of crimes, these indexes are heavily influenced by the crimes that are most commonly committed in each reporting category (robbery for violent and larceny for property). Offense data are reported using the hierarchy rule, which means that if a series of crimes occur at once, only the most serious crime is reported. The offenses known data are all part 1 crimes, which are considered more serious crimes and reliably reported by most law enforcement agencies.

There are some drawbacks to the arrest and offense numbers. First, crimes are recorded using the "hierarchy rule," meaning that only the most severe crime is reported. For example, if an assailant murders an individual during a robbery then only the murder is recorded. However, only one crime is committed in 85 percent of cases. Also, while much effort is made to confirm the accuracy and consistency of reporting through quality assurance reviews, the data still contain some discrepancies and missing information. First, I look for instances of within municipality "jumps" that are not explained by changes in reporting. For instance, arrest figures in Alabama for disorderly conduct decrease 7-fold in Birmingham after 1990 and 15-fold in Montgomery after 1991, a decrease that cannot be explained by changes in

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[14]Mississippi does not have a state reporting agency, and agencies in Ohio that use the Summary Reporting System as opposed to the National Incident-Based Reporting system do not report to the Ohio state reporting agency. In both cases, these agencies submit their data directly to the FBI.

reporting patterns but appears to be related to the method of reporting "other" arrests.[15] Because of this particular issue within each city, I flag these jumps and alter the values to missing. Then I follow Mello (2019) and Evans and Owens (2007) to identify outliers and errors, which are then recoded as missing. Then through backward/forward imputation and interpolation, I fill in the missing data.

Figure 3a shows how a range of pretreatment characteristics affect the likelihood of electing a Black candidate, and Figure 3b describes how those same characteristics affect the timing of electing a Black candidate among the treated. A city's Black population strongly predicts the likelihood of electing a Black candidate, whereas it has less predictive power for indicating when one will be elected. A one standard deviation increase in Black population share increases a Black candidate's probability of wining by 22 percentage points which is double the standard deviation of 11.4 percent; a similar standard deviation increase moves up election of a Black candidate about one election cycle at best. In addition, the characteristics do not jointly predict election timing but they do predict likelihood. The main analysis will focus on the sample of treated units to obviate this concern about selection. The next section will lay out my empirical strategy and concerns to identification.

4 Empirical Strategy

To determine the relationship between race-specific law enforcement patterns and the election of breakthrough Black candidates, I exploit the timing of when a Black candidate assumes office in each city, then I stratify the sample by the specific powers granted to the office. I aim to compare cities that directly elect a Black candidate for the first time in a specific year relative to cities that had not yet elected a Black candidate by that year. Because Black candidates are not randomly assigned to cities, it is important to assess trends in arrest patterns before a candidate takes office to ensure that they are not predictive of election. A

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[15]This feature is in general exclusive to cities in Alabama and Florida, indicating actions taken by their state reporting agencies.

primary concern is that patterns for Black or white arrests are changing before the election of a breakthrough Black candidate differentially relative to cities where elections do not occur. For example, the total level of offenses could be declining, which may remove the most effective political cudgel an opposing candidate can use in an interracial election.[16] A complementary concern is that while we may expect Black candidates to improve outcomes for Black residents, the way in which this arises could lead to different effects across periods, and these effects may grow over time. Restated, we may fail to uncover the true effect of Black mayors on public safety outcomes if the effects evolve or are reinforcing over time. We must also be cognizant of treatment effect heterogeneity that arises from differences in timing group size (i.e. in some years four Black candidates might assume office, whereas in other/most years, it is one) and when units are treated in the panel. Fortunately, numerous scholars have identified these issues in difference-in-differences settings and proposed solutions (Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021; Borusyak, Jaravel, and Spiess 2021).

I follow Callaway and Sant'Anna (2021) and leverage variation in the timing of breakthrough Black elections to estimate group-time average treatment effects. My data run from 1974 to 2018 (i.e., $t = 1974, 1978, ..., 2018$). Across the sample period, 63 Black candidates assume office in 33 periods beginning in 1975, which I index $g = 1975, 1978, 1980, ..., 2017$. I estimate the group–time average treatment effects on the treated for each group g in each time period t by comparing units in g to units that were not yet treated in t. I then summarize the effect of electing a Black candidate on different public safety outcomes by aggregating these different treatment effects. In the main text, I focus on the sample of treated units that ever elect a Black candidate for expedience in estimation as there are no observable characteristics that strongly influence election timing as figure 3b illustrates.

Under a parallel trends assumption, the group–time average treatment effect on the

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[16]Offenses and not arrests, which may be more salient to voters, but they are presumably related, holding officer capacity and effectiveness fixed.

treated for each group g at time t with no anticipation is

$$ATT(g,t) = E[Y_t - Y_{g-1}|G_g = 1] - E[Y_t - Y_{g-1}|D_t = 0] \tag{1}$$

This process yields many $\widehat{ATT(g,t)}$, most of which are identified from relatively few observations; so instead of interpreting each group-time ATT, I combine these effects into more intuitive aggregate effects.[17]

First, I report the average effect for cities where a Black candidate takes office in each $\tilde{g}$,

$$\hat{\theta}(\tilde{g}) = \frac{1}{2019 - \tilde{g}} \sum_{t=\tilde{g}}^{2018} \widehat{ATT(\tilde{g},t)} \tag{3}$$

which takes for each group $\tilde{g}$ the group-time ATT, sums over every period greater than or equal to $\tilde{g}$, and equally weights each post-treatment period to obtain a group-specific treatment effect. Then, each group-specific effect is aggregated and weighted according to the probability of belonging to group g,

$$\hat{\theta}_O = \sum_{g \in \mathcal{G}} \hat{\theta}(g) P(G = g). \tag{4}$$

Equation 4 reports an intuitive summary average treatment effect on the treated. While important, equation 4 obscures the possibility that policy may operate on a lag, and some

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[17]The doubly-robust version used in the Appendix in the extended sample is,

$$ATT(g,t) = \left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{p_{g,t}(X)(1-D_{g,t})(1-G_g)/(1-p_{g,t}(X))}{\mathbb{E}[p_{g,t}(X)(1-D_{g,t})(1-G_g)/(1-p_{g,t}(X))]} \right) (\Delta Y_t - m_{g,t}(X)) \right] \tag{2}$$

where ΔY_t is the average difference of a policing outcome at time t relative to $g-1$ for the treated units, G_g is a dummy variable equal to 1 for units in timing group g, D_t is a dummy equal to zero for units not-yet-treated at time t, $p_{g,t}(X)$ is the probability of being a member of g at time t given covariates X, and $m_{g,t}(X)$ is the average difference in the outcome of interest for the comparison units at time t relative to $g-1$ given characteristics X. The propensity score is estimated using a logistic working model and $m_{g,t}(X)$ is estimated via linear regression as described in Sant'anna and Zhao (2020). I estimate each $ATT(g,t)$ with its sample analog, $\widehat{ATT(g,t)}$ using the doubly-robust methods outlined in equation 2, which yields consistent estimates if at least one of the propensity score model or outcome evolution of the control group is correctly specified. Estimation procedures done using the `did` package from Callaway and Sant'Anna (2022).

crimes, operational choices, or budget decisions may be more easily influenced. In order to illustrate this and provide some evidence of parallel trends I also estimate an "event-study" specification. For each relative period e, I estimate

$$\hat{\theta}_D(e) = \sum_{t=1974}^{2018} \sum_{g \in \mathcal{G}} \mathbf{1}\{t - g = e\} P(G = g | t - g = e) \widehat{ATT(g, t)}, \qquad (5)$$

which is the group-time aggregation equivalent of the canonical event study. For periods $g \leq t$, the comparison is the long difference of t to $g - 1$, similar to the traditional event study where the relative periods are compared to an omitted period, usually the $-1th$ period. For periods $g > t$, the "short-difference" is computed where subsequent pre-periods are compared to one another. Because the ATTs in the pre-period are assumed to be zero, this is a similar test to comparing the leads to the omitted group in the canonical case. Identification relies on the usual parallel trends assumption, which states that in the absence of treatment, outcomes for the breakthrough election group would have evolved similarly to outcomes for cities that had not elected a Black candidate in the year being estimated. Simultaneous confidence bands clustered at the city (agency) level with 95 percent coverage are constructed via multiplier bootstrap proposed by Callaway and Sant'Anna (2021); these bands are robust to multiple-testing problems to which traditional pointwise bands are not.

The main data set consists of 63 cities that elect a Black candidate after 1974 and contains data on arrests, offenses, public finances, and 1970 demographics. The next section provides brief descriptive measures and then turns to the main analysis of the impact of Black mayor.

5 Results

5.1 Descriptive Stats

Figures 4a and 4b depict trends in the outcomes of interest by race (blue line for Black outcomes and red line for white outcomes) and by municipal organization (solid line for mayor-council and dashed line for council-manager). The y-axis is arrest per 10,000 residents of the same race; that is, the Black figure is Black arrests divided by Black residents, then multiplied by 10,000, though arrests in the jurisdiction are not necessarily arrests of inhabitants. Outcomes used in the analysis are defined similarly. There is a clear level difference between Black and white arrest rates in both quality of life and violent crimes. Quality of life arrests across race are declining across the sample period, perhaps reflecting rising average income, improving employment prospects, or progressing racial attitudes. Only minor, transient differences appear in arrest rates across municipal organization, which resolve by the end of the sample period, suggesting minimal differences in police behavior given the functional head of government.

Trends in violent crime reveal a perhaps more interesting pattern. First, Black residents are at least twice as likely to be arrested for violent crimes than white residents throughout the period. Unlike quality of life arrests, violent crime increases for all groups until roughly 1993, then declines thereafter, coinciding with the "great American crime decline." This has been attributed to increases in income and employment as well as increases in police manpower, mandatory minimum sentencing, and a general increase in the carceral state (Zimring 2006).[18]

Table 1 describes pre-treatment characteristics for the elected sample. Black residents account for one-quarter of residents, and white residents constitute the remaining portion.[19]

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[18]While violent arrests increase until the early 1990s and offenses known to law enforcement exhibit the same pattern, victimization rates are generally flat over that period and begin to decline at the same point. The pattern perhaps reflects changes in reporting practices by the public to the police.

[19]We should treat this as a roughly accurate measure of Black (white) residents. In 1970, respondents were asked to record the race they most closely identified with and used their father's race if they were unsure.

The sample is split evenly by mayor-council and council-manager governments. Cities are growing on average from 1967 to 1971 but this is highly variable. Municipalities spend 10.5 percent of their budget on police protection and maintain large police forces, though this number contains employees with non-arrest powers as well. Black offenders are arrested more than 70 percent more often than white offenders for quality of life offenses and nearly 7 times more often for violent offenses. The ratio of officers to murders is 31.43, but this number is skewed by a small number of cities with few murders. Appendix Figure 9 illustrates the mean and median ratios of officers to murders from 1987 to 2016, and we observe that most departments sit below 25 across the period. O'Flaherty and Sethi (2019) indicate that increasing the police force to above 25 officers for every murder produces reductions in mortality risk greater than the cost of employment; so many agencies are understaffed by this metric. I also test to see if administrations are specifically responding to murders when considering manpower.

In the next section, I begin by describing the impact of Black mayors on race-specific arrests. I probe agency and public finance outcomes that may shed light on the methods that policymakers use to affect outcomes. Then, I estimate changes in department size and department composition by race and gender and conclude the section by estimating correlations of officers and arrests.

5.2   Are Black Americans Arrested Less and for What Kinds of Offenses?

Table 2 displays estimates of equation 4 on race-specific arrest outcomes. Columns 1 and 3 include no pre-treatment characteristics, and columns 2 and 4 control for each city's Black population share in 1970. Comparisons between the conditional and unconditional specifications are not one-to-one because estimation for each treatment group is only possible

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Methods to record race have changed across decades. For example, 1980 was the first to include a question about Hispanic heritage in a two-step method and recorded the respondent's mother's race if they marked more than one. For more, see Race and multi-racial Americans in the US census.

for time periods when there is a comparable control group, i.e., when there is overlap in the probability of electing a Black candidate between treatment and control. This implies that for some treatment groups, the group-specific effect is not the full treatment horizon. It is still useful to compare estimation stability nonetheless. The first two columns are all treated cities, and the final two restrict the sample to cities where the mayor serves as chief executive.

Two observations are immediate: nearly all estimates are negative, perhaps suggesting a policy priority for Black mayors, and the estimates suggest that mayors can exert some control over their law enforcement agencies. Most notably, reductions in arrests for potential Black offenders is sharpest in QoL arrests. In those cities where the mayor serves as chief executive, there were 48 fewer arrests per 10,000 Black residents, and I can rule out all but a negligible positive effect at a 95 percent level. The effect is quite similar and more precise when conditioning on the Black population share where I can rule out any positive effect at a similar level. QoL arrests for whites displays a smaller, noisier reduction, which may indicate a change in departmental prerogative overall. Violent arrests display a less consistent pattern, changing signs when I condition on the Black population share for white violent arrests. I cannot rule out changes in either direction for either race except for violent Black arrests in column (3). Homicide arrests are negative and imprecise, a finding that should be expected since the homicide level is erratic year-to-year. The reduction is larger, yet still imprecise in the chief executive cases for Black offenders.

The final rows report changes to the sale of cannabis. Arrests for the sale of cannabis decrease for both white and Black potential offenders. Across both specifications and samples, the estimates are qualitatively similar except less precisely measured when conditioned on the Black population. Black offenders are arrested 1.87 fewer times per 10,000 Black residents and there are 0.73 fewer white arrests per 10,000 white residents. It may be of interest to compare the estimates for both possession and sale of cannabis. On the one hand, we might expect the number of possession arrests to decline if there is an element of discretion in arresting or executing a search warrant for cannabis; on the other hand, if there is an element

of manipulation at the possession–sale threshold, then one could plausibly see an increase in possession arrests and a decrease in sale arrests. This particular investigation does not have the data to inspect manipulation at the threshold both because many agencies do not begin faithfully reporting possession arrests until the next decade, and I lack the data on the arrestable quantity (or weight) of cannabis at time of arrest.

Figure 5 restricts attention to QoL and violent arrests and examines the change in a 15-year event window. The figure displays 10 years of pre-treatment to analyze pre-trends where the entire sample is displayed in Figure 5a and the mayor-council sample is displayed in Figure 5b. I focus most of my discussion to the mayor-council sample. Violent arrests for both Black and white persons declined in the post-period, and both estimates decline almost continuously. By the tenth year post-assumption (year "9" on the x-axis), Black arrests have declined by slightly fewer than 20 and white arrests have declined by two showing that Black elections may be particularly important for deterring or detecting potential Black crime. Another interpretation, of course, is that other opportunities, employment for instance, have improved for Black residents specifically. We observe similar declines in Black and white QoL arrests, but once again the difference in scales belies the differences in magnitude. Black (white) arrests have declined by a statistically imprecise 25(5) arrests approximately. The notable difference between the types of arrests is that it takes much longer to reduce QoL arrests (in a statistical sense); so this may be a consequence of trying to reduce violent crimes and not necessarily the proximate policy target. I also cannot rule out composition effects for the longer lags, as this may reflect differences in treatment effects between earlier- and later-treated groups, though I find no evidence of this when observing the group-specific effects (that is, the estimates returned from equation 3).

In general the number of arrests declines. This could be the result of crime levels declining via some method of deterrence or improving welfare; it could be caused by Black or female cops making fewer arrests (without commenting on whether or not this is a net positive). Departments may be shrinking, labor market prospects may be improving, or

police quality may be declining. Table 3 shows estimates of equation 4 on offenses known to law enforcement. The results are broadly consistent with the estimates in Table 2 in that declines in offenses are commensurate with arrest declines. The mayor-council estimate indicates a level reduction of 5,100 offenses reported to law enforcement, and is larger when conditioning on the Black population. Reports of murder and violent crime are all negative, but murder is only statistically significant in the conditioned case, possibly indicating a short-run reduction in homicide. The average reduction in this case would be 15 lives, which is a significant reduction in any event. Property and total index crimes both exhibit similar, statistically significant reductions, a finding consistent with the reported finding of the total level of crimes falling. The results provide support for the conclusion that elections of Black candidates have an effect on the level of crime, and the results do not reflect solely a change in quality or behavior of the law enforcement agency.

5.3   Changes to Spending and Department Size

Chief executives have control over the distribution of the budget. Having the power to line item veto gives them considerable influence to make marginal moves across many departments either through additional funds or increased staffing. Even when mayors serve as council persons, they may indirectly influence budget appropriations, depending on the particular policy they hold paramount or their standing within the council. Table 4 reports results from the estimation of equation 4 on a set of public finance and public safety outcomes with standard errors reported in parentheses to determine if this helps explain the reductions in crime and arrests. The number of police relative to the population does not change after a Black election, though in column (3), the range of outcomes has limited overlap with small, negative changes to officers per capita; however, the share of police expenditure relative to total expenditures falls. In column (3), the reduction ranges from a negligible tenth of a percent to an approximate 2.5 percentage point reallocation of the municipal budget. Importantly the reduction appears to be from a reordering of priorities and not an absolute

reduction in the police budget, as police expenditure per capita does not change or is slightly increased. In column (3), expenditure increases by slightly less than $10M. Staffing levels also do not appear to change, as they are highly variable across jurisdictions, and the police share of total employment is positive in magnitude across all specifications but imprecisely measured. Sylvera (n.d.) demonstrates that Black mayoral elections also lead to increased economic activity in majority Black communities. Given the highly localized nature of crime and that Black communities experience a disproportionate share of violent crime, increased economic opportunity and stronger links to the local government may play a stronger role in reducing crime than increases in detection and deterrence.

5.4 Are There More Black Officers? Evidence from the Law Enforcement Management and Administrative Statistics (1987-2016)

While the size of the police department does not appreciably change, the racial and gender composition of the department might. This in turn may shift the behavior of the average officer or may work to mitigate harmful culture within agencies. In earlier periods, many departments were beset by affirmative action litigation requiring departments to increase their numbers in one or both dimensions. In the later periods, criminal justice activists and constituents alike called for changes in department structure to address accusations of continued discriminatory behavior. I use the Law Enforcement Management and Administrative Statistics (LEMAS) to provide evidence as to whether department composition changes in reference to race and gender, and then I examine whether the number of law enforcement officers increases by race and gender as well. LEMAS is a periodic survey sent to departments that asks questions about agency staffing, diversity, policing initiatives, operations, salaries, and other line items pertinent to law enforcement agencies. The survey was first collected in 1987, then was recorded in 1990, 1993, 1997, 2000, 2003, 2007, 2013, and 2016. I merge these together to

construct a panel that answers whether agency diversity increases after election of a Black candidate.

I report estimates of equation 4 in Table 5, which shows changes to the shares of Black or female officers in panels A and B and their per capita change in panels C and D, respectively. These estimates will capture changes in departments in cities where a Black candidate assumes office after 1987 as that is the first year in which the survey was taken. We might assume that we are capturing a lower estimate for changes to Black officers and department composition (see Fig. 1) because much of the change in Black officers seems to come by 1993; however, if this is a policy area common to Black mayors on average —and many do indeed make this an important policy goal —we should presume to observe a positive effect.

Panel A contains results for the full sample and Panel B restricts estimation to the set of treatment cities where the mayor holds executive power. In both cases, there is no appreciable change in the share of Black or female officers. Agencies on average have 1.8 p.p. more Black officers as a share of the police department after election but the confidence interval indicates that even moderate decreases in the share of Black officers are possible. This could be indicative of difficulties in recruiting or just a shortcoming of the available data (by the time we're able to observe departments again and their relevant comparison groups, short-term gains may have been closed). The share of white officers, however, declines 4 percentage points in panel B, with the range implying small net losses in the share of white officers to a nearly 6 percentage point decline in the share of white officers. The share of female officers remains unchanged in both specifications.

The number of Black officers in per capita terms increases after election, and the estimates are larger and more precise when restricting estimation to chief executive cities. Evaluating the chief executive estimates at the population average implies a total increase of 32 Black officers after electing a Black candidate. Changes to other demographic groups are noisy and sometimes switch signs. The last two columns in panels C and D test whether the

22

gap between Black and white officers is reduced and whether the officers to murders ratio increases. The Black-white officer gap is reduced on average across both specifications, and this gap reduction is significant at a 10-percent level. The upper end of the interval implies a reduction of 46 percent in the gap, and the lower end would imply a reduction of 4 percent; so departments exhibit a wide range of outcomes. Finally, it does not appear that departments add officers in relation to the number of murders that occur. I now turn to analyzing whether elections of Black candidates lead to changes in arrest patterns and if there might be a pattern between the results in this section and the next.

5.5   Are Particular Interventions More Effective?

In order to estimate the relationship of particular kinds of police manpower on QoL or violent arrests, I leverage an estimation strategy proposed by Deshpande and Li (2019) and Cengiz et al. (2019) where I separate each breakthrough election e into its own data set and estimate an event-specific treatment effect. I estimate

$$Y_{ect} = \alpha_{ec} + \delta_{et} + \tau_e Black\ Mayor_{ect} + \beta_e \mathbf{X} + \varepsilon_{ect} \tag{6}$$

where Y_{ect} is a crime outcome (QoL or violent arrests) in its level or police manpower outcome (total officers, Black officers, or female officers) for event/election e in city c in year t. The effect is the level change induced from electing a Black mayor. I relate the two outcomes (crime and manpower) to each by fitting a line via OLS to determine the effect of police manpower on violent or QoL crime. I re-estimate the relationship excluding New York because its department is far larger than any other police force, and OLS leads to overfitting that point. Because these objects are related through the estimation procedure, I report bootstrapped standard errors for each. The slope of the relationship is given by the simple linear regression coefficient,

$$\bar{\tau} = \frac{\text{cov}(\widehat{arrests}, \widehat{officers})}{\text{var}(\widehat{officers})} \tag{7}$$

Figures 6-7 plot the change in police on the x-axis and the change for QoL or violent crime on the y-axis. The OLS fit is then plotted on top of the data. Figure 6 is consistent with work on the effect of police on crime. Each point represents a point estimate of equation 6. The best-fit line represents estimation of equation 7. Because Black mayors can leverage many policy levers besides increasing the number of officers, or Black, female, and white officers, the estimates represent a non-causal correlation between officers and arrests. Nonetheless, the investigation is informative of policies that may be associated with desirable outcomes. While changes in QoL arrests are roughly the same by race, changes to the number of officers are associated with a reduction in Black violent crime. Increasing police manpower is correlated with decreased arrests for QoL crimes and decreased arrests for Black violent crimes more than white violent crimes.

Table 6 consolidates the results from each figure and reports the slope of a one Black/female/white officer, or officer change on arrests for QoL offenses, violent offenses, and murder by race with bootstrapped standard errors. The top panel includes New York, and the bottom panel excludes the city. I focus on the lower panel to abstract from the problems caused by OLS in the presence of an outlier. The number of observations at the bottom of the table includes New York. An increase in Black officers is associated with a reduction in the number of QoL Black arrests by 7.35 on average and a reduction in the number of QoL white arrests by 4.71, though the latter estimate is not statistically significant. The relationship between Black officers and other arrests is imprecise. Female officers are associated with large, imprecise reductions in QoL arrests for both races. In addition, a one female officer increase is associated with a large reduction in Black violent arrests which may be partially driven by the limited variation in female officer changes, though earlier work has found that additional female officers are effective in reducing certain types of violent crime (Miller

24

and Segal 2019). An additional white officer is associated with increases in arrests of Black offenders for murder, and their relationship with QoL crimes is null but suggestive of a range of relatively large increases or reductions. The one officer (overall manpower) relationship shows how in the context of Black mayoralties, this is not equivalent to an addition of one white officer, relationships that in other contexts are more strongly linked. A one officer increase is negatively associated with violent Black arrests and Black arrests for murder, though given the imprecise estimate, I cannot fully reject the over-policing, under-policing narrative.

6   Discussion and Conclusion

Across time and space, crime and public safety are one of the most oft-cited concerns in the run-up to breakthrough elections. However the relationship between this public safety fervor and translation into one particular public safety outcome is loose. Why is that? First and perhaps most importantly, crime is only one element of public safety. Environmental standards, property standards, sewerage, street repair, and a litany of other components combine to create a sense of safety and community trust. In this sense, my investigation only covers a small portion of how a mayor may cause or influence public safety outcomes. Efforts to improve these complementary elements of public safety likely improve faith in the government overall, and the added faith has the ability to increase bilateral communication. Communication and faith are important components to the policing aspect of public safety; so policy likely needs to be broader, which makes efficacy more difficult. Experiences also play a factor in proposed policy. In addition, some mayors come from policing backgrounds, others from criminal defense or prosecution, and even others from business.[20] Adding these hitherto "unobserved" elements into the analysis will provide further clarity as to how politics lead to policy.

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[20]See Appendix C for more on this point, and Kirkland (2017) for more on mayoral characteristics.

Noted shortcomings notwithstanding, the election of breakthrough Black candidates tends to lead to more Black officers and large reductions in crime citywide. In turn, breakthrough elections lead to decreases in violent arrests without increases in quality of life arrests for Black persons, which means that a potential path to correcting the under-policing/over-policing outcome is through increased political representation and perhaps improving officer diversity. What remains to be investigated is the nature of victimization. Crime and arrests are reduced, but do Black residents disproportionately benefit from committing fewer crimes and being victimized less in the process? In addition, investigating any differences in managerial tactics to achieve these goals is of principal importance. The municipal budget is finite, and investing the marginal dollar in the most efficient, least discriminatory practices should be seen as one of the largest goals of any effective policymaker. I leave these questions for future work.

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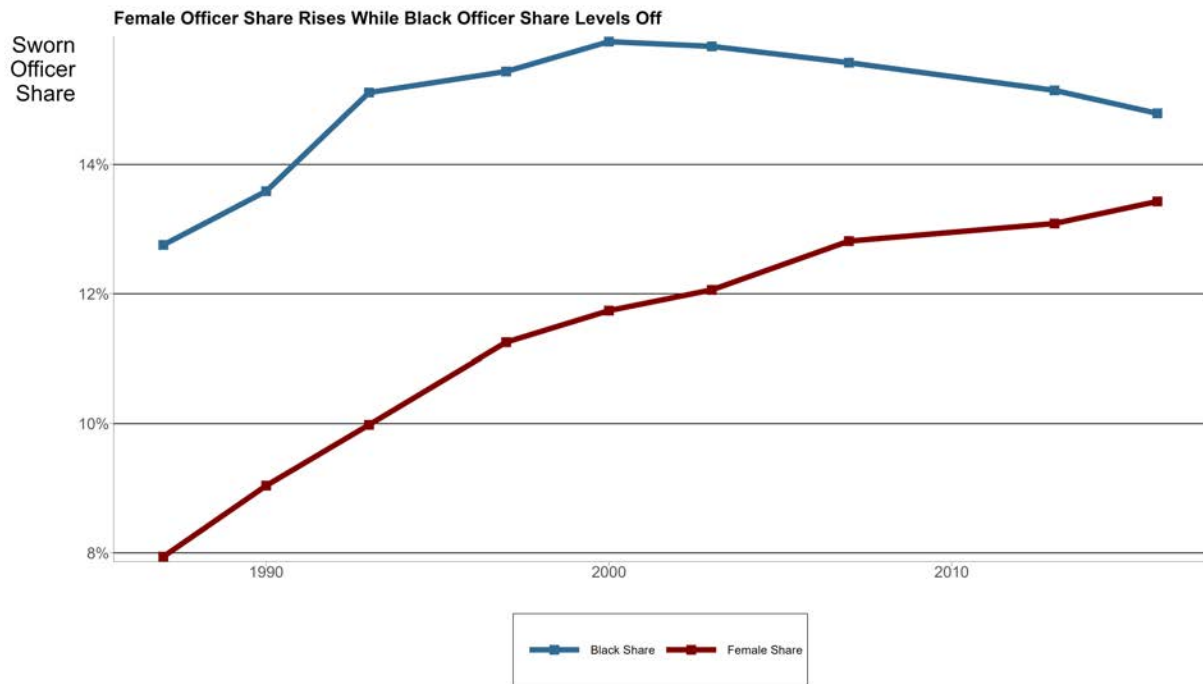
Figure 1: Share of Black and Female Officers, 1987-2016

Source: *Law Enforcement Management and Administrative Statistics.* The average share of law enforcement agency that is Black or female is calculated for each year of survey data within the sample.
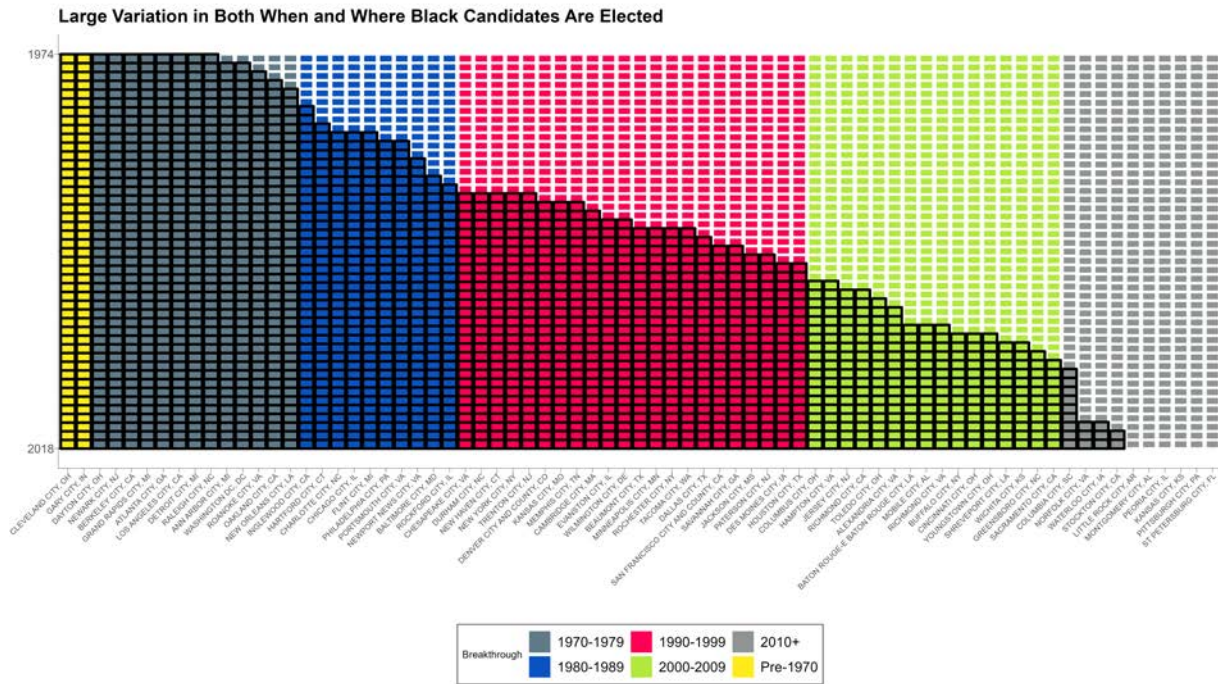
Figure 2: Breakthrough Election Timing: 1974-2018

Figure describes the distribution of breakthrough elections across the sample. Then, it is categorized by the decade in which the mayor assumed officed. The boxes denote years, and each bold box denotes a treatment year. Note, for any city in which every box is bold, the city elects a candidate in or before 1974 and will be excluded from the analysis.
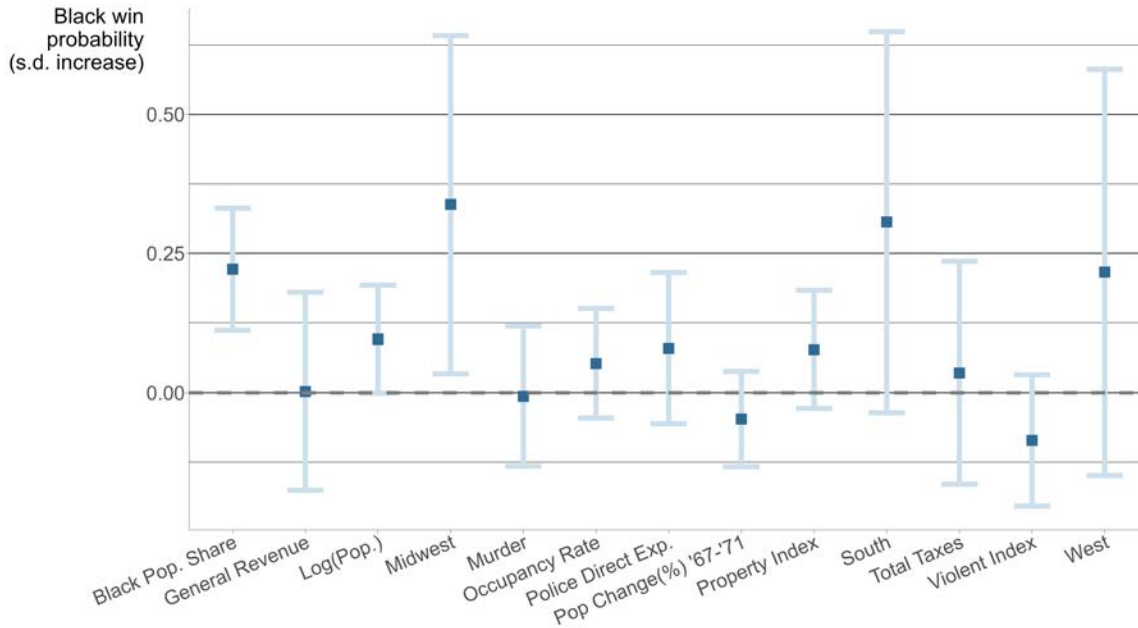
(a) 1970 Black Population Share Strongly Predicts Black Election
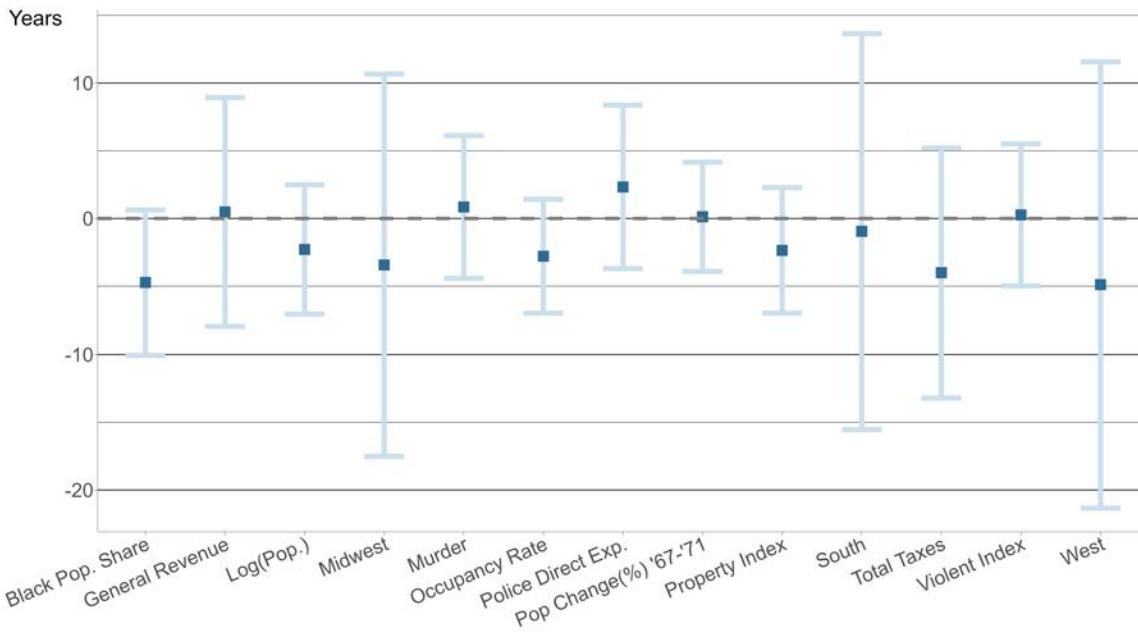
(b) 1970 Black Population Share Less Predictive of Timing

Figure 3: Coefficient for Prediction on Treatment and Timing

Panel 3a displays point estimates and 95% confidence intervals from the regression, $EverBlackMayor_c = \mathbf{X}'_c\beta + \varepsilon_c$. The F-statistic for joint significance is 5.26. Panel 3b displays point estimates and 95% confidence intervals from the regression, $FirstBlackMayorYear_c = \mathbf{X}'_c\beta + \varepsilon_c$. The F-statistic for joint significance is 0.99. All continuous predictors are standardized so that estimates reflect the effect of a one standard deviation increase. Results where predictors are in per capita terms reflect the same pattern.
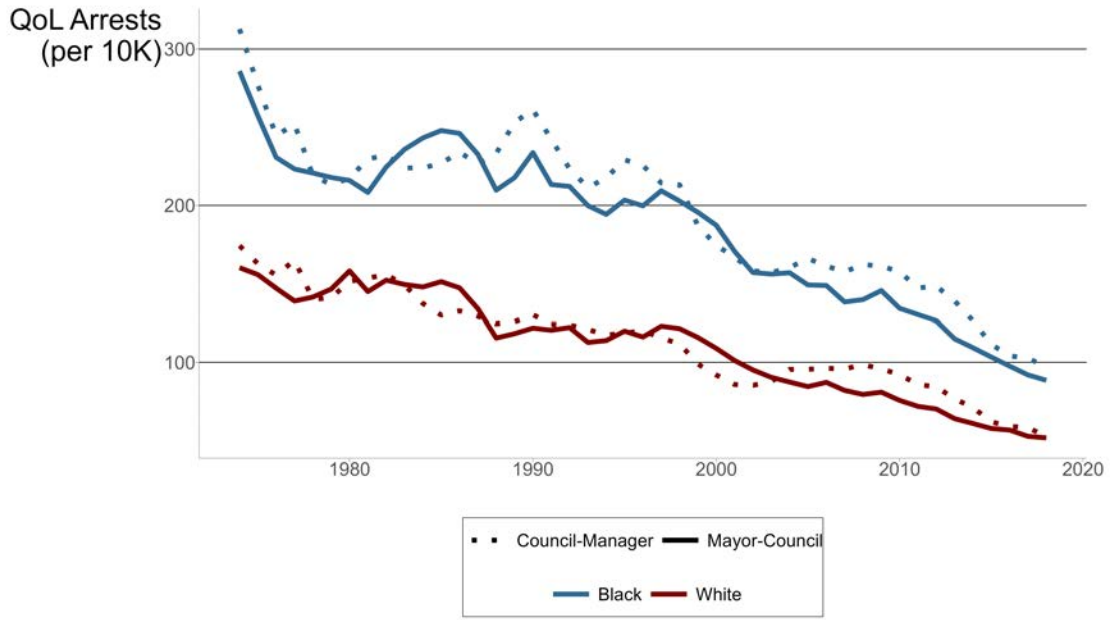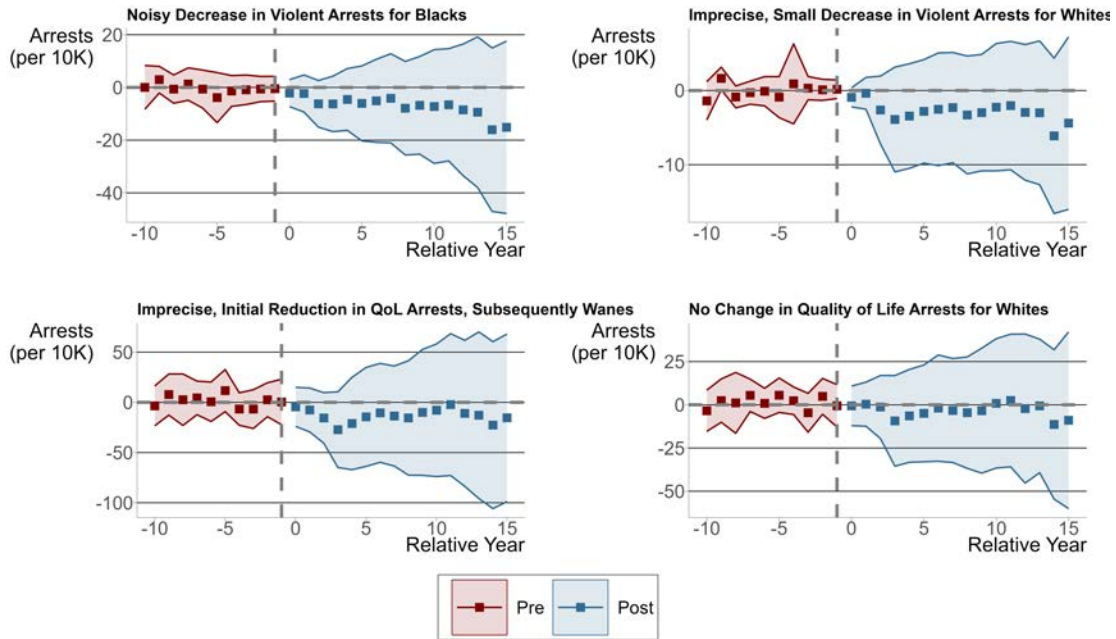
(a) Trends in Quality of Life Arrests by Race & Municipal Organization
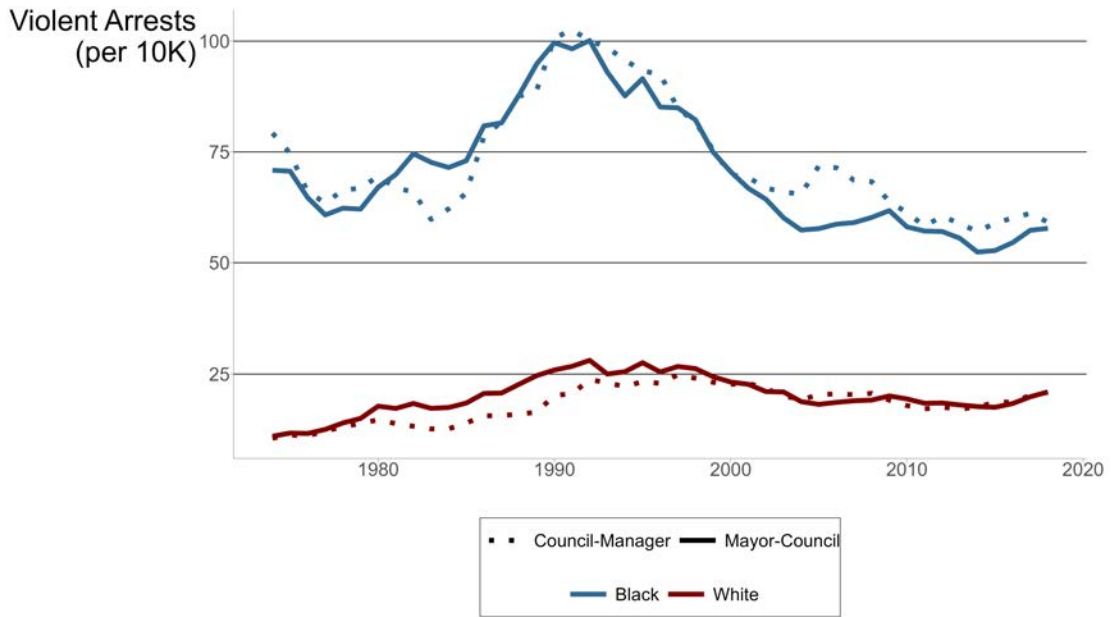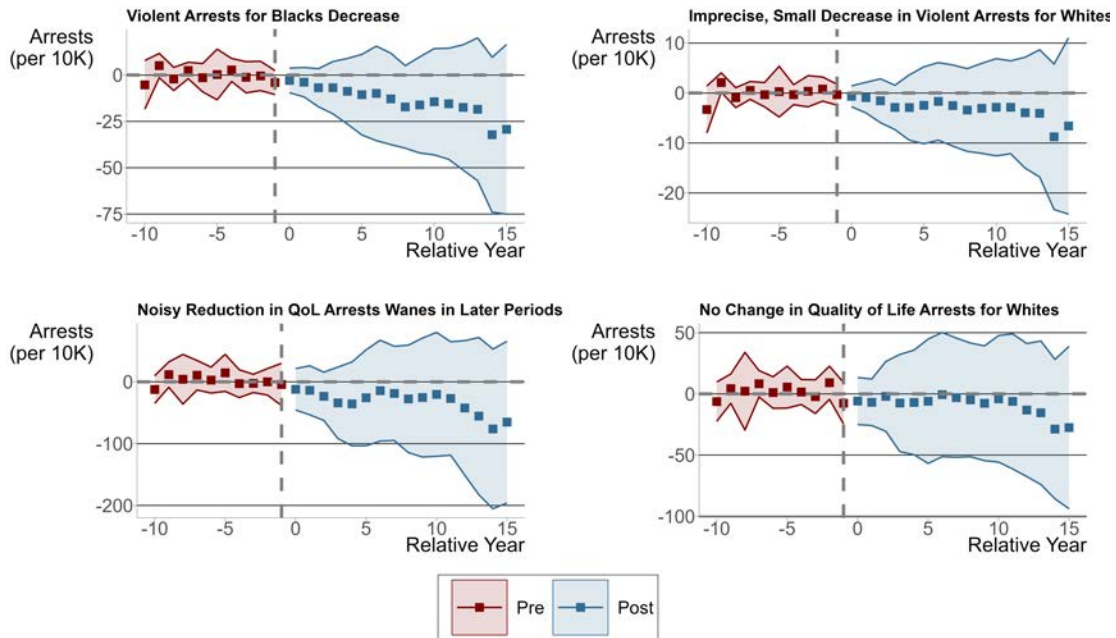

(b) Trends in Violent Arrests by Race & Municipal Organization

Figure 4: Arrests Time Series

(a) Entire Treated Sample

(b) Only Chief Executive Cities

Figure 5: Race-Specific Arrest Patterns After Black Election

Notes: Results reflect estimation of equation 5 where the dependent variables are arrest rate by race∈{black, white} and by type of offense ∈{quality of life, violent}. Blue points and lines reflect post-treatment period where each relative period e reflects the weighted average of each group-specific ATT e years after treatment relative to the year before treatment. The red points and lines reflect pre-intervention effects, which test if there are significant differences between each period and the preceding one. Simultaneous 95% confidence bands displayed.
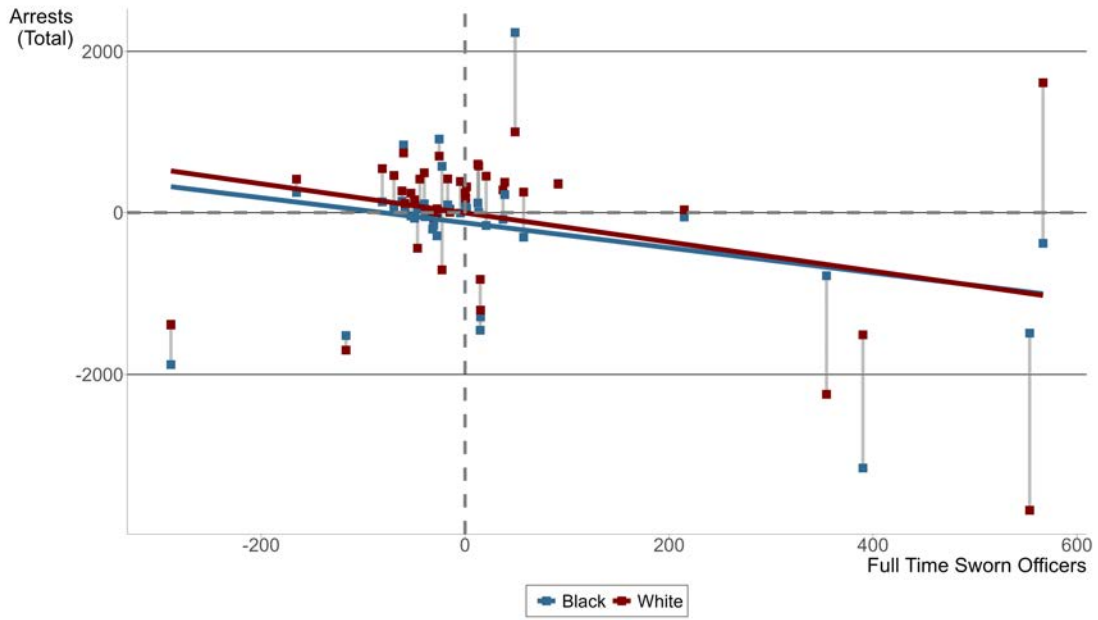
(a) Quality of Life
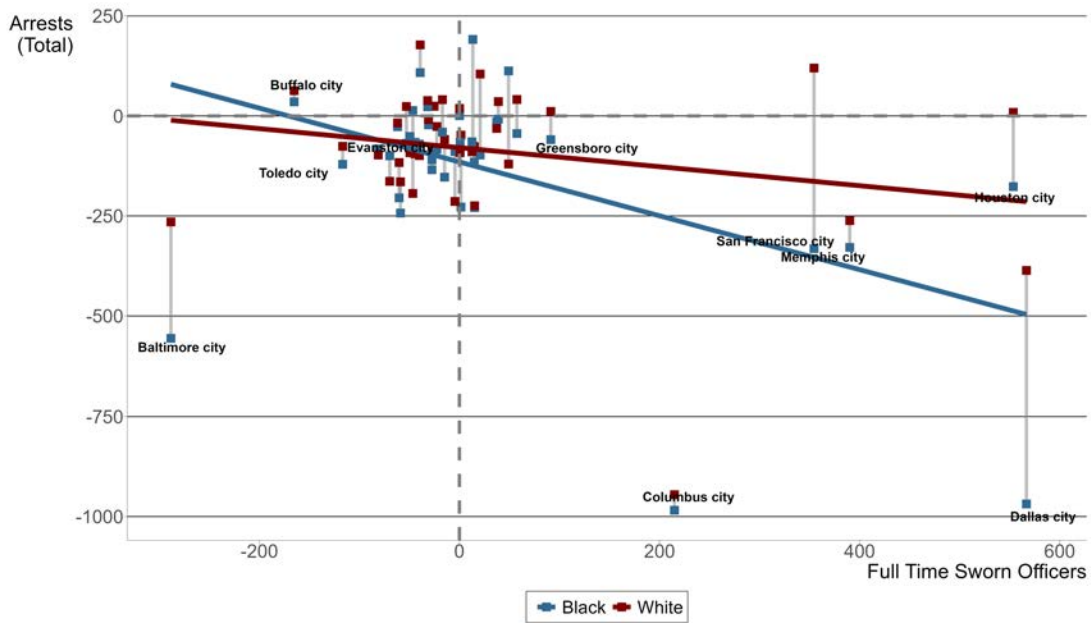
(b) Violent

Figure 6: Relationship Between Changes in Force Size and Arrests

Figures 6a and 6b display estimates of equations 6 and 7. Each point represents a single regression for each treatment city for Black (blue points) and white (red points) offenses and change in officer level. The best-fit lines represents the race-specific correlation between the set of results on either axis.

37
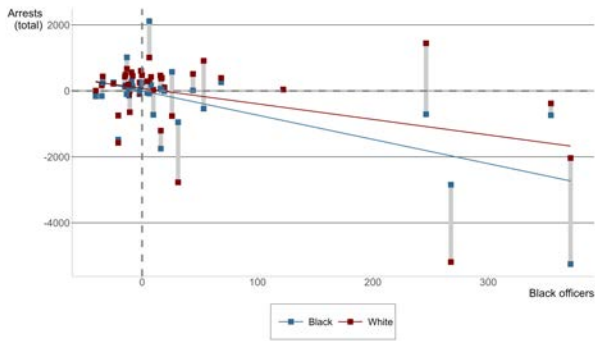
(a) Black QoL
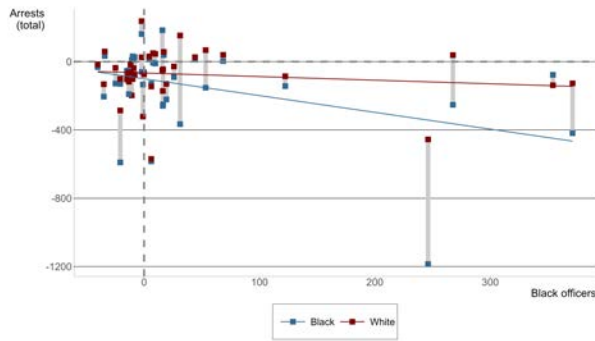
(b) Black Violent

(c) Female QoL
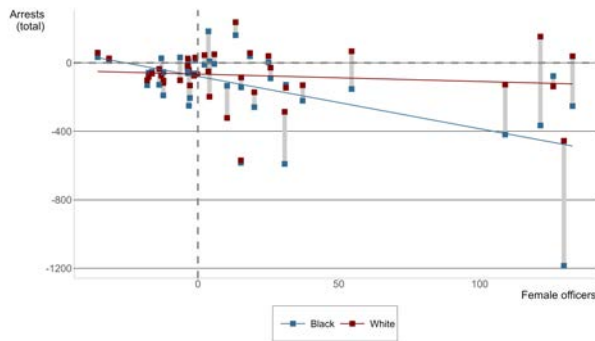
(d) Female Violent
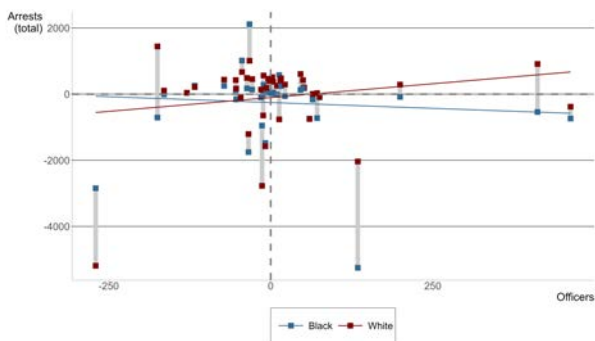
(e) White QoL
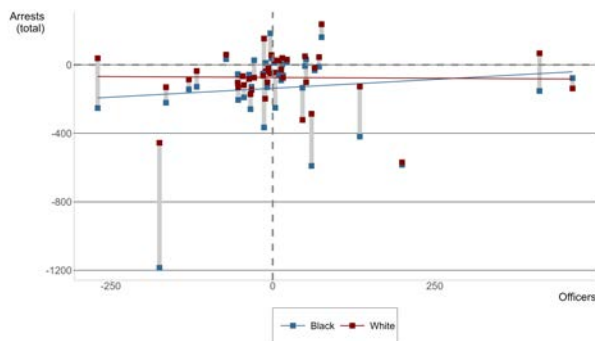
(f) White Violent

Figure 7: Correlation Between One Female Officer Increase And Arrests
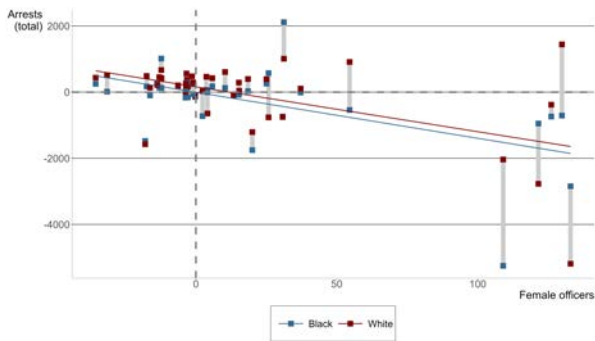
Figures 7a-7f display estimates of equations 6 and 7. Each point represents a single regression for each treatment city for Black (blue points) and white (red points) offenses and change in officer level. The best-fit lines represent the race-specific correlation between the set of results on either axis.

Table 1: Pre-Treatment City Characteristics

	Mean	SD		Mean	SD
Arrests ('74)			*Pub Fin & Demographics*		
QoL Black	285.4	(176.01)	Percent Chief Executive	54.50	(50.17)
QoL White	162.66	(128.13)	Population	476,473	(1,058,042)
Violent Black	71.01	(29.94)	Percent Black	24.39	(11.46)
Violent White	10.94	(6.97)	Percent White	74.18	(11.06)
Murder Black	4.47	(2.09)	Occupation Rate	94.58	(1.65)
Murder White	0.7	(0.47)	Percent Population Change	7.45	(15.97)
			Police Expenditure	147.89	(58.75)
			Police Expenditure Share	10.54	(4.39)
			General Revenue	1,395.17	(857.71)
			General Expenditure	1,483.79	(891.00)
			Total Taxes	715.44	(413.47)
	Mean	SD	Full Time Employees	12,215	(44,920)
Offenses			Full Time Police	1,774	(4,896)
Murder	1.4	(0.75)			$\underline{N}$
Violent	67.05	(36.51)	Elected Cities		66
Property	615.21	(172.09)	Mayor Council Cities		37
Officers / Murders	31.43	(43.20)			

Sources: *Uniform Crime Reporting Program: Arrests by Age, Sex, and Race, Uniform Crime Reporting Program: Offenses Known to Law Enforcement*, and the *Annual Survey of Governments*.

Table 2: Impact of Black Mayor on Race-Specific Arrests by Offense Type

Dep. Var	Treated (1)	(2)	Treated - Mayor-Council (3)	(4)
QoL Black	-22.54	-16.27	-48.07*	-50.50**
	(20.52)	(15.92)	(28.16)	(22.11)
QoL White	-10.75	-4.14	-20.98	-16.86
	(11.27)	(8.35)	(14.94)	(11.21)
Violent Black	-11.13	1.24	-18.95*	-6.5
	(8.15)	(6.31)	(10.88)	(7.81)
Violent White	-3.76	0.49	-4.59	0.47
	(3.01)	(2.02)	(4.07)	(3.10)
Murder Black	-0.14	-0.17	-0.45	-0.41
	(0.35)	(0.23)	(0.49)	(0.29)
Murder White	-0.08	-0.03	-0.13	-0.02
	(0.13)	(0.06)	(0.16)	(0.08)
Sale Black	-1.64*	-1.06	-1.87*	-1.48
	(0.88)	(0.78)	(0.96)	(0.92)
Sale White	-0.69*	-0.74*	-0.73*	-0.65
	(0.39)	-0.42	(0.40)	(0.51)
Black Share		X		X
Cities	63	63	37	37
Treated Groups	33	33	21	21

***$p < 0.01$;** $p < 0.05$;* $p < 0.1$

Notes: Results reflect estimation of equation 4. The dependent variable is listed in the first column and reflects various types of arrest offenses. Columns (1) and (2) display the estimates from the sample of elections involving a Black victory, and columns (3) and (4) are the subset of victories where the local government has a mayor-council organization. Columns (2) and (4) reflect estimates conditioned on the Black population share in 1970. Multiplier bootstrap standard errors are reported in parentheses and are clustered at the city level.

Table 3: Impact of Black Mayor on Offenses Known to Law Enforcement

	Treated		Treated - Mayor-Council	
Dep. Var	(1)	(2)	(3)	(4)
All Crimes	-71.35***	-99.70***	-109.23***	-173.86***
	(29.15)	(25.21)	(33.80)	(19.30)
Murder	-0.08	-0.16*	-0.07	-0.32**
	(0.09)	(0.09)	(0.19)	(0.16)
Violent Index	-5.80	-5.81	-6.04	-10.53**
	(6.97)	(5.09)	(8.69)	(4.97)
Property Index	-35.45**	-47.40**	-57.88***	-89.24***
	(15.79)	(20.46)	(18.87)	(18.63)
Total Index	-41.56**	-53.38**	-64.05***	-99.81***
	(20.97)	(23.04)	(23.88)	(20.41)
Black Share		X		X
Cities	63	63	37	37
Treated Groups	33	33	21	21

***$p < 0.01$;** $p < 0.05$;* $p < 0.1$

Notes: Results reflect estimation of equation 4. The dependent variable is listed in the first column and reflects measures of offenses known to law enforcement. Columns (1) and (2) display the estimates from the sample of elections involving a Black victory, and columns (3) and (4) are the subset of victories where the local government has a mayor-council organization. Columns (2) and (4) reflect estimates conditioned on the Black population share in 1970. Multiplier bootstrap standard errors are reported in parentheses and are clustered at the city level.

Table 4: Impact of Black Mayor on Officers and Expenditure

	Treated		Treated - Mayor-Council	
Dep. Var	(1)	(2)	(3)	(4)
Police (per 10K)	1.17	0.15	1.64	-0.13
	(0.98)	(1.05)	(1.09)	(0.84)
FT Employees (per 10K)	-0.99	-1.70	-1.84	1.39
	(5.77)	(5.35)	(7.38)	(6.59)
Police Employment Share	0.84	0.51	0.59	0.85
	(0.70)	(0.45)	(0.86)	(1.22)
Police Expenditure	11.53	8.07	20.72*	12.03
	(8.95)	(9.47)	(12.03)	(14.56)
Police Expenditure Share	-0.99**	-1.36***	-1.29**	-2.51***
	(0.49)	(0.39)	(0.61)	(0.35)
Black Share		X		X
Cities	63	63	37	37
Treated Groups	33	33	21	21

$^{***}p < 0.01;^{**} p < 0.05;^{*} p < 0.1$

Notes: Results reflect estimation of equation 4. The dependent variable is listed in the first column and reflects measures of public finance as they relate to law enforcement and the size of the local government. Columns (1) and (2) display the estimates from the sample of elections involving a Black victory, and columns (3) and (4) are the subset of victories where the local government has a mayor-council organization. Columns (2) and (4) reflect estimates conditioned on the Black population share in 1970. Multiplier bootstrap standard errors are reported in parentheses and are clustered at the city level.

Table 5: Changes in Department After Election of Black Candidate

Panel A: Full Sample

	Black Share	White Share	Hispanic Share	Male Share	Female Share
BM × Post	0.003	−0.014	0.009	0.001	−0.001
	(0.014)	(0.016)	(0.010)	(0.010)	(0.011)
Cities	50	50	50	50	50

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Panel B: Chief Executive Cities

	Black Share	White Share	Hispanic Share	Male Share	Female Share
BM × Post	0.018	−0.041**	0.019**	0.002	−0.002
	(0.020)	(0.018)	(0.008)	(0.010)	(0.009)
Cities	31	31	31	31	31

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Panel C: Full Sample

	Black Per Capita	White Per Capita	Male Per Capita	Female Per Capita	B-W Difference	Off Per Murder
BM × Post	1.568	−1.334	−0.250	0.065	−2.903*	−0.334
	(0.985)	(1.324)	(0.552)	(0.290)	(1.584)	(3.974)
Cities	50	50	50	50	50	50

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Panel D: Chief Executive Cities

	Black Per Capita	White Per Capita	Male Per Capita	Female Per Capita	B-W Difference	Off Per Murder
BM × Post	2.751**	−1.015	0.157	0.203	−3.766*	1.313
	(1.277)	(2.143)	(0.628)	(0.312)	(2.236)	(4.337)
Cities	31	31	31	31	31	31

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Results reflect estimation of equation 4. The dependent variables are listed at the top of each panel and reflect changes to the racial or gender composition of law enforcement agencies after election of a Black candidate in panels A and B. Panels C and D reflect changes to the number of sworn officers as a rate of the race- or gender-specfic population within the LEA. The final two columns in Panels C and D show the change in the representation gap between Black and white officers (Column (1) - Columns (2)), and the change in the ratio of police officers to murders after election of a Black candidate. Panels A and C display the estimates from the sample of elections involving a Black victory, and Panels B and D are the subset of victories where the local government has a mayor-council organization. Estimates are derived from the Law Enforcement Management and Administrative Statistics. Multiplier bootstrap standard errors are reported in parentheses.

Table 6: Relationship Between Officers and Arrests Induced by Election of Black Mayor

	QoL		Violent		Murder	
	(1)	(2)	(3)	(4)	(5)	(6)
	Black	**White**	**Black**	**White**	**Black**	**White**
Officer	0.09	0.56	-0.3	-0.1	-0.01***	-0.01**
	(5.02)	(8.40)	(0.35)	(2.43)	(0.00)	(0.01)
Black Officer	-11.17	0.32	-5.54	-1.98***	-0.11	-0.06
	(13.19)	(2.54)	(9.19)	(0.18)	(0.09)	(0.10)
Female Officer	-8.07	0.64	-4.29***	-1.54***	(0.18)	-0.05
	(28.04)	(8.54)	(1.68)	(1.26)	(0.01)	(0.07)
White Officer	-8.8	1.38	-4.63	-1.7	-0.07	-0.05
	(16.4)	(2.30)	(9.85)	(0.2)***	(0.06)	(0.08)
	Exclude NYC					
Officer	-1.55	-1.80	-0.67*	-0.24	-0.08***	-0.02
	(10.39)	(2.35)	(0.38)	(0.30)	(0.03)	(0.02)
Black Officer	-7.35***	-4.71	-0.98	-0.21	-0.00	-0.02
	(2.35)	(3.24)	(0.97)	(0.55)	(0.18)	(0.10)
Female Officer	-13.91	-13.57	-3.06**	-0.43	-0.03	-0.07
	(8.99)	(8.36)	(1.28)	(1.15)	(0.32)	(0.09)
White Officer	-0.71	1.67	0.21	-0.02	0.08*	0.01
	(5.33)	(2.31)	(0.85)	(0.53)	(0.05)	(0.06)
N (w/ NYC)	**42**	**42**	**42**	**42**	**42**	**42**

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Notes: Results reflect estimation of equation 6 for changes in officers and repeating the process for race- and offense-specific arrests. From there I obtain the estimates in each cell through estimation of equation 7 which estimates the correlation between officers and arrests. The first panel contains OLS estimates including the city of New York, and the bottom panel excludes the city from estimation. Bootstrapped standard errors are reported in parentheses.

Appendix

A Data

Table 7 describes the annual variation in the first time a Black candidate assumes the mayoralty. Because effect at initial treatment is not the parameter I am most focused on estimating, I make no attempt to try to adjust each assumption date relative to the date of reporting, though this may add some noise to the group-specific treatment estimates. Instead, mayors are assigned their assumption year according to the first year in which they were the mayor for the majority. That is, mayors who assume office before July are assigned the year of initial assumption and those who assume afterward are assigned the following year. This also assigns timing group according to when the first Black candidate is directly elected. In some cases, Black candidates have been selected from within the city council or by being the leading vote-getter from a citywide council election. Because by hypothesis, one might assume these forms of election are correlated with limited mayoral power, I assign "breakthrough" as previously described. See Hopkins and McCabe (2012) for a method of assignment that aligns with the former.

B Additional Figures and Tables

C Black Mayor and a Cop: A Comparative Case Study of Houston Mayor, Lee Brown

On January 2, 1998, Lee Brown was inaugurated as the first Black mayor of Houston. He also had the distinction of becoming the second Black mayor of a large city with police

experience after Tom Bradley (Los Angeles).[21] The main text is devoted to determining how a principal characteristic, namely, Blackness, affects downstream public finance and public safety outcomes. Naturally, auxiliary characteristics shape behavior as well. The question, then, is: how salient are other features of identity outside of race to shaping decision-making, or does race dominate other ways in which people might identify themselves? Unfortunately, the data limit many ways in which we may probe this question, and in many cases, it is *ex ante* unclear how other characteristics might shape policymakers' public safety decisions. However, some relevant jobs might have higher a likelihood of shaping these decisions because they directly intersect public safety outcomes. I'll highlight two occupations that do not strongly intersect with Blackness: police officer and district attorney, though it is not entirely rare for Black persons to work in either occupation.

Police officers interact with offenders daily and, depending on assignment, may uniformly deal with a certain type of criminal, e.g. an officer assigned to narcotics chiefly interacts with suspects involved with illicit drugs. District attorneys whose job is to prosecute potential offenders serve as a complement to officers, and their job prospects depend on maintaining high prosecution rates. Similar to officers, prosecutors can specialize in particular criminal areas that expose them to a subset of potential offenders. Judges also bear mentioning, though it is somewhat less clear how the job affects decision-making, for instance, considering how a being a civil court judge affects one's opinion on the treatment of potential offenders at the street level. Further, a large literature already exploits judge heterogeneity in sentencing, suggesting other factors play a larger role. One factor is party affiliation, especially as it pertains to being a member of the Republican party. In my sample, three mayors belong to the Republican party, but this section (or paper) will not explore that dimension, though it

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[21]A couple of caveats merit some clarity. First, there may have been other Black mayors with policing experience in cities outside of the sample, but obtaining reliable data — for instance, whether the election was direct or within council — becomes somewhat onerous for cities below a certain size threshold. Second, other mayors between Bradley and Brown have what can be described as "public safety" experience. Sharon Sayles Belton (Minneapolis) served as a parole officer and Edward Vincent Jr. (Inglewood) worked in the Los Angeles County probation department. I make a distinction between these, as the day-to-day interactions between offenders are likely different and likely shape perceptions differently.

warrants further research.

Here, I discuss Lee Brown. First, I will briefly lay out some biographical details that should illuminate the quantitative analysis that will follow. Before the analysis, I also highlight particular characteristics of Brown that serve to situate my findings more intuitively, discuss why Brown presents particular challenges to interpretations of results in the main text, and give a brief overview of the estimation methodology.

Before Lee Brown became the mayor of Houston, his life was devoted to the study and practice of criminology. Though I cannot confirm this, Dr. Brown may perhaps be the most well read mayor in criminology having earned a doctorate in criminology from the University of California, Berkeley during his time as a patrolman.[22] The 112 linear feet of boxes that chronicle Mayor Brown's life contain innumerable articles on public safety, e.g. "Anatomy of Police: A Case for Community Control," "Establishing a Police-Community Relations Program," and "Law and the Black Man." These are a small sliver of the pieces he wrote more than 20 years before becoming mayor. While being the mayor is more public-facing and by necessity offers less time for academic pondering, the pieces he wrote during his tenure were still largely focused on public safety.[23]

Mayor Brown's pre-mayoral history limits not only the interpretation of Houston, but also perhaps how one should understand the mayoral channel. One potential channel is through that of appointments such as the head of the police department. Sometimes, this does not exactly line up. For instance, Mayor Brown served as the head of the Houston force 15 years before assuming the mayoralty. He encountered resistance from rank-and-file officers especially because at the time the Houston agency was considered one of the worst in the country. Given that we take that assessment to be true, then having an even marginal effect on race-specific arrests might seem likely. This complicates our understanding of what it

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[22]I have intentionally not called him Dr. Brown hitherto for "artistic" purposes that hopefully become clear.

[23]Of the seven during-administration articles contained in his professional papers, three were explicitly related to policing, while another concerned treatment for those with alcohol and drug dependencies.

47

means to be a "clean treatment."

In the second case, Mayor Brown served (somewhat briefly) as the commissioner for another Black mayor, David Dinkins of New York. Both Dinkins and Brown were able to advocate for greatly increasing the size of the police force and in turn the racial representation therein. However, Brown and, to a lesser extent, Dinkins receive less credit for the reduction in crime that would begin on their watch but peak in the tenures of Mayor Giuliani and Commissioner Kelly. Part of the justification for this exploration lies in the fact that being Black and having police experience is not the usual pre-election conditions that arise. However, I am somewhat limited in following this logic deeper due to the inability to reliably track managerial changes within each law enforcement agency.[24]

C.1 Constructing a Synthetic Houston

A natural comparison in this context may be nearby Dallas, which is also a large city with a similar Black population share. However, Ron Kirk became the first Black mayor of Dallas three years before Lee Brown would assume office. The happenstance somewhat muddles how we would understand the differences between cities, even though Kirk lacks any public safety experience. Because no city roughly approximates Houston quite as well, one approach is to construct a "synthetic" version of Houston that uses a combination of cities with similar observable pre-election characteristics that are relevant to the outcome. I follow Abadie, Diamond, and Hainmuller (2010) and construct a synthetic version of Houston. To begin, I limit the relevant "donor" cities to those that elected a Black candidate after 1998. In some sense, we may believe it valid to restrict the choice only to cities that elect a Black candidate after the treatment window (in this setting 2008); however, because I am interested in the effect of Black and police experience, I also allow for the possibility of election within the

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[24]This also arises again in Houston where the second Black police chief, Clarence Bradford, was appointed the year before Brown took office though he would serve almost entirely during the Brown administration.

window. That being said, results are nearly unchanged if I make this additional restriction.[25] For both the crimes and arrests data used and discussed within the main text, I fit the pre-election data using the average police expenditure, tax collection, share of municipal expenditure devoted to policing, and murder rate. In addition, I use the lagged values of the dependent variable for the years, '82, '85, '88, '91, '95, and '98. While selecting different, fewer, or more years does not substantially change the results, the lagged years do the best at minimizing the in-sample prediction error, though even that comes with caution. The synthetic control estimator performs well when the donor cities are within the convex hull of the intervention city, but Houston is an outlier in violent arrests for Black offenders, and some crimes (murder specifically) are volatile, making both estimation and interpretation precarious. I will show the results for all relevant outcomes but will mostly abstain from making strong conclusions in these cases.[26]

Besides being useful in this single-treatment case, the synthetic control method has a number of benefits. The weights derived from the comparison units and predictors are explicit and between zero and one, which in the case of OLS is not necessarily true. In addition, the post-treatment outcomes are not used to construct the counterfactual, which "limits" the researcher's ability to manipulate the results.

C.2   Results

Figures 11a and 11b display synthetic control estimates for arrests by race and severity of crime and the corresponding weights attributable to each donor and variable. The dashed vertical lines delineate the three terms Brown was in office. We can observe that the fit

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[25]I drop Jersey City (NJ) from the sample because Glenn Cunningham (2002) also has law enforcement experience. It might be of interest to repeat this experiment with Jersey City and compare both results with the ones in the main text. Unfortunately, Mayor Cunningham died during his first term in office; so I refrain from such an exercise.

[26]Another method is to split the pre-treatment data, train a portion of the data, obtain the weights from this set, and use those weights on the other portion of the data to determine fit (see Abadie, Diamond, and Hainmuller (2015) for more). Because of the volatility in arrests, I opt to use all of the pre-treatment data to obtain the weights.

for violent arrests for Blacks and quality of life arrests for whites is poor. In the case of violent arrests for Blacks, Houston's observable arrests lie outside those of the other cities, and quality of life arrests for whites has a spike year in 1988, which alters the fit otherwise. Violent arrests for whites and QoL arrests for Blacks generally have a good fit, and by that I mean the difference between observed and synthetic is small. QoL arrests for Black offenders is unchanged in comparison to a synthetic Houston until two years after Brown leaves office, and then there is a notable jump relative to synthetic Houston when his succesor is in office. Violent arrests for whites are marginally higher during the Brown administration, but it is important to note the difference in magnitude for the white figure. During the Brown administration, white arrests were between 1-3 arrests higher per 10,000 white residents, a number that is dwarfed by the level for QoL arrests and the level for violent Black arrests. Variable weights indicate that the lags of the dependent variable regularly received the most weight and a subset of cities were used as donors more often, such as Columbia, South Carolina, and Cincinnati, Ohio for, example.

Figures 12a and 12b repeat the exercise but report estimates and weights for non-negligent homicide, violent index crime, property index crime, and all crime. Except for homicide, the synthetic version of Houston follows the observed pre-election Houston well. Violent index crime appears to increase, while property index crime decreased. These trends combined mute the overall effect on crimes; however, it is unclear if these changes are statistically meaningful. One way to determine if this differs from random chance is to look at the ratio of the post-period and pre-period mean squared predictive error. We can then iterate across all donor cities and compare those ratios. For the case of violent index and property index crime, the ratio of the pre- and post-election MSPE far surpasses 1 (which would indicate the MSPE in the post-period roughly equaled that of the data it was intended to minimize) and surpasses the ratio for all donor cities, suggesting there was a plausible effect. Because the effects are countervailing, the effect on total crime is roughly nil despite the pre-election fit. We can also observe that Kansas City (KS) often receives the most weight of the controls

which in some sense is quite the coincidence because Kansas City's first Black mayor (2022), Tyrone Garner, has credentials similar to those of Mayor Brown.

C.3   Takeaways

Though any definitive conclusions perhaps lie outside of the scope of this aside, I have asked if there are other "identity" characteristics that countervail or intensify managerial decisions. In some sense, one should expect that other "contrasting," downstream identities are dominated by racial identity. Indeed, Brown's entire policing career seems to have been influenced by his Blackness and not the reverse. Long before he rose to any kind of power, most of his work was dominated by the question of community relations and the interactions of Blacks and the police. This leads to a secondary consideration: identifying and ordering identities. I've proceeded mostly as though Brown saw himself first as a Black man and second as a lawman, but outside of the privilege of asking Brown directly, it is an open question as to how Brown and, for that matter, other Black candidates view themselves. Brown is not with other credentials; he has a Ph.D. an uncommon credential for most mayors, let alone a Black one. One could look to campaign materials, but often both candidates offer banalities aimed to not offend voters. Nonetheless, we may be somewhat restricted in studying this from a purely econometric standpoint, but blending more qualitative insights with quantitative observations is certainly warranted to understand what voters might expect from candidates with similar identities.
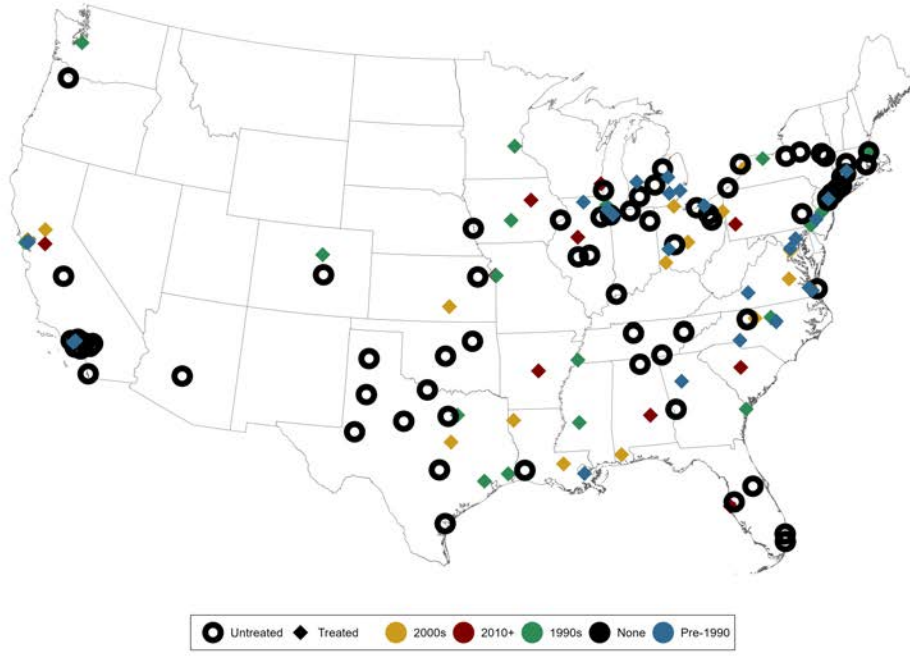
Figure 8: Treatment Map by Elected/Non-Elected for Large Cities in 1970

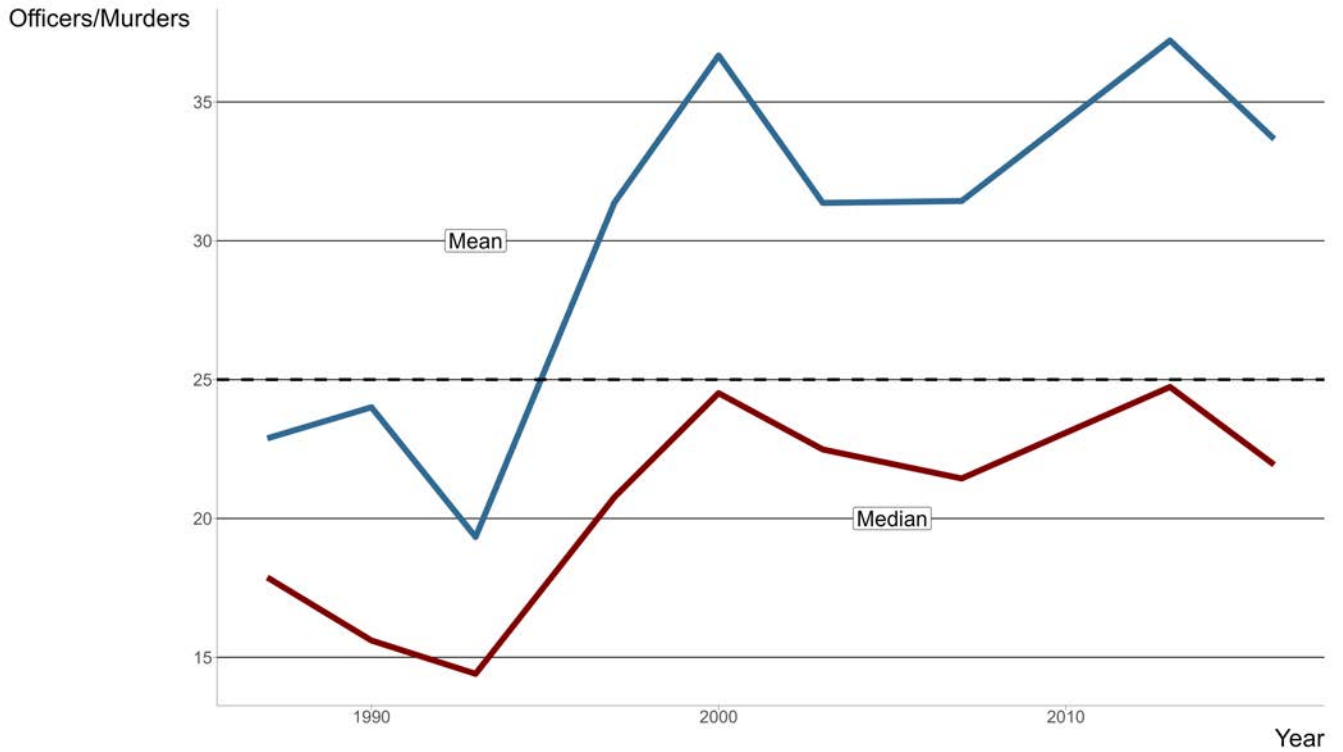Sources: Ferreira and Gyourko (2009), Vogl (2014), author

Figure 9: Mean and Median Officers Per Murder 1987-2016

Notes: Author's calculation from *Law Enforcement Management and Administrative Statistics* and UCR *Offenses Known to Police*. "Mean" refers to the cross-sectional average of the number of officers each agency recorded divided by the total number of homicides within jurisdiction. Median figure is similarly determined.
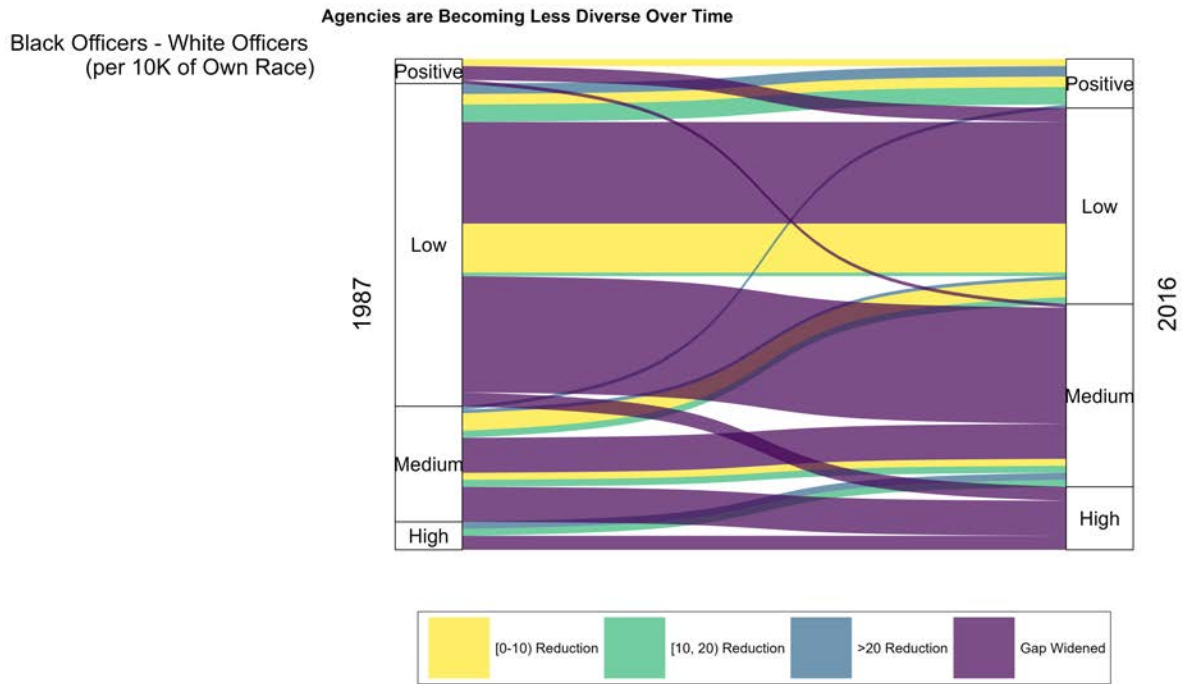
Figure 10: Changes in Officer-City Representation

Figure illustrates changes in racial representation within each department in the sample. The representation gap is calculated by taking the total number of Black officers and dividing it by the city's Black population and repeating it for white officers, then taking the difference between them. Categories are broken down by how the gap changed from 1987 to 2016: purple for a widening B-W gap(in other words, that number became more negative), yellow for a 0-10 officer gap reduction, green for a 10-20 officer gap reduction and blue for a greater than 20 officer gap reduction. On either side of the year figure, "Positive" implies the B-W gap was positive, "Low" implies it was between 0 and 10 officers, "Medium" implies 10-20 officers, and "High" is anything greater than 20 officers. Source: *LEMAS*.
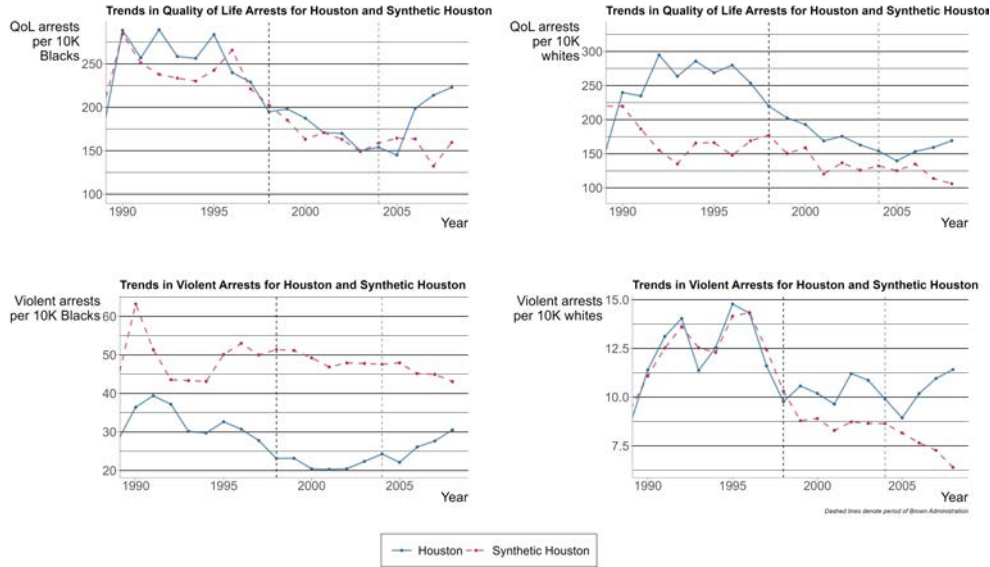
(a) Synthetic Control Results for Arrests in Houston
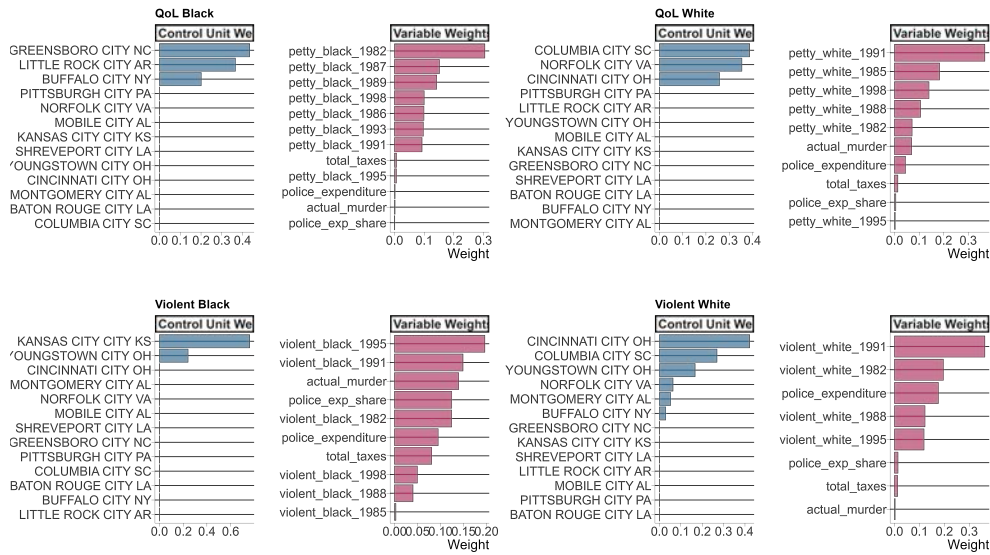
(b) Variable and Unit Weights for Arrest Outcomes

Figure 11: Arrest Outcomes Post-Lee Brown

Figure 11a reports synthetic control estimates for various arrest outcomes after the Lee Brown election. The blue line indicates the actual outcomes in Houston and the red line indicates a counterfactual Houston constructed from various donor cities, which can be viewed in Figure 11b. The blue bars describe the weight placed on each potential donor city, and the red bars describe the weights placed on each pre-treatment control. The controls consist of various lags of the outcome variable, and 8-year pre-period means of other related outcomes.
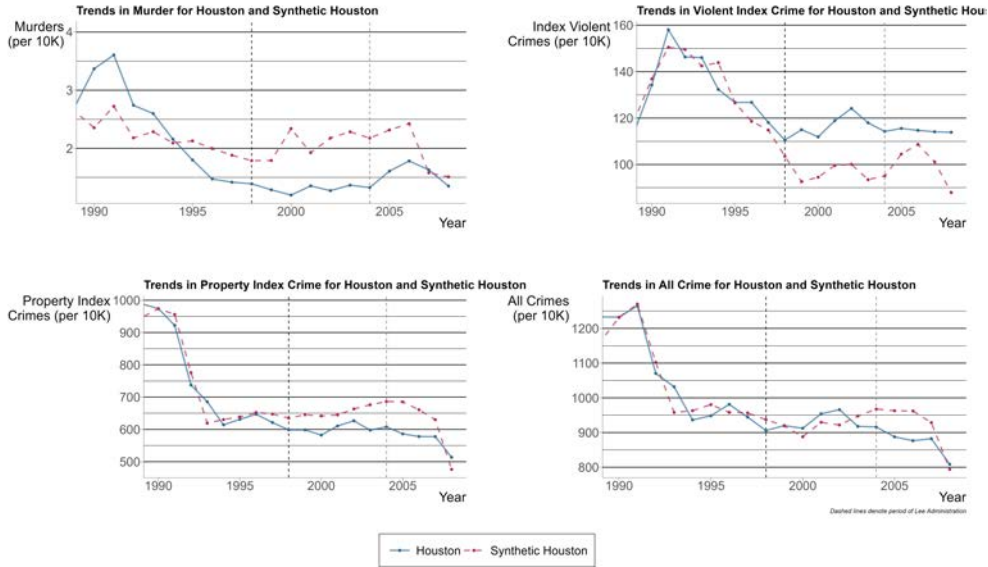
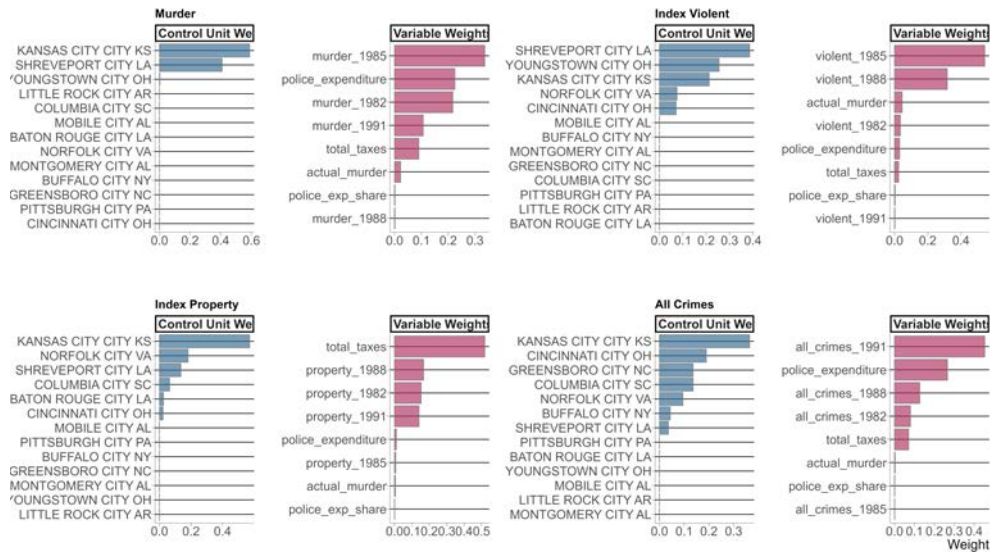(a) Synthetic Control Results for Offenses in Houston

(b) Variable and Unit Weights for Offense Outcomes

Figure 12: Crime Outcomes Post-Lee Brown

Figure 12a reports synthetic control estimates for various criminal offenses known to law enforcement after the Lee Brown election. The blue line indicates the actual outcomes in Houston and the red line indicates a counterfactual Houston constructed from various donor cities, which can be viewed in Figure 12b. The blue bars describe the weight placed on each potential donor city, and the red bars describe the weights placed on each pre-treatment control. The controls consist of various lags of the outcome variable, and 8-year pre-period means of other related outcomes.

56

Table 7: Treatment Cities

Timing Group Cities
1975 Ann Arbor (MI)
1976 Roanoke (VA)
1977 Oakland (CA)
1978 New Orleans (LA)
1980 Inglewood (CA)
1982 Hartford (CT)
1983 Chicago (IL), Flint (MI), Charlotte (NC)
1984 Philadelphia (PA), Portsmouth (VA)
1986 Newport News (VA)
1988 Baltimore (MD)
1989 Rockford (IL)
1990 New Haven (CT), Trenton (NJ), New York (NY), Durham (NC), Chesapeake (VA)
1991 Denver (CO), Kansas City (MO), Memphis (TN)
1992 Cambridge (MA)
1993 Wilmington (DE), Evanston(IL)
1994 Minneapolis (MN), Rochester (NY), Beaumont (TX), Tacoma (WA)
1995 Dallas (TX)
1996 San Francisco (CA), Savannah (GA)
1997 Jackson (MS), Paterson (NJ)
1998 Des Moines (IA), Houston (TX)
2000 Columbus (OH), Hampton (VA)
2001 Richmond (CA), Jersey City (NJ)
2002 Toledo (OH)
2003 Alexandria (VA)
2004 Waco (TX)
2005 Mobile (AL), Baton Rouge (LA), Richmond (VA)
2006 Buffalo (NY), Cincinnati (OH), Youngstown (OH)
2007 Wichita (KS), Shreveport (LA)
2008 Greensboro (NC)
2009 Sacramento (CA)
2010 Columbia (SC)
2014 San Antonio (TX)
2016 Waterloo (IA), Norfolk (VA)
2017 Stockton (CA)
2019 Little Rock (AR)
2020 Montgomery (AL)
2021 Peoria (IL)
2022 St. Petersburg (FL), Kansas City (KS), Pittsburgh (PA)

Sources: Vogl (2014), Ferreira and Gyourko 2009, author's own.

Table 8: Impact of Black Mayor on Race-Specific Arrests by Offense Type

Dep. Var	All Cities			All Cities - Mayor-Council		
	(1)	(2)	(3)	(4)	(5)	(6)
QoL Black	-19.63*	-31.03***	-25.26**	-36.00**	-51.23**	-36.11
	(10.41)	(11.24)	(12.42)	(18.03)	(20.37)	(26.64)
QoL White	-8.24	-18.72***	-10.27	-13.80	-26.24	-8.57
	(6.44)	(7.21)	(7.51)	(9.75)	(10.23)	(13.69)
Violent Black	-6.90*	-5.09	-4.74	-11.53***	-9.81***	-10.47**
	(3.87)	(3.96)	(3.61)	(3.76)	(3.60)	(4.43)
Violent White	-3.04**	-4.83***	-4.71***	-3.49***	-5.68***	-4.64***
	(1.23)	(1.57)	(1.65)	(1.31)	(1.68)	(1.68)
Murder Black	-0.20	0.16	-0.12	-0.32	-0.28	-0.24
	(0.15)	(0.15)	(0.17)	(0.25)	(0.25)	(0.30)
Murder White	-0.06	-0.18***	-0.10*	-0.05	-0.21***	0.00
	(0.05)	(0.07)	(0.06)	(0.05)	(0.06)	(0.06)
Sale Black	-1.42*	-1.76**	-2.15**	-1.30	-2.11***	-3.24***
	(0.80)	(0.82)	(0.96)	(0.83)	(0.70)	(0.84)
Sale White	0.78**	-1.25***	-1.22***	-0.61	-1.14**	0.96
	(0.34)	(0.41)	(0.46)	(0.44)	(0.49)	(0.65)
Black Share		X		X	X	
BS + X			X			X
Cities	137	137	137	110	110	110
Treated Groups	33	33	33	21	21	21

$^{***}p < 0.01;^{**}p < 0.05;^{*}p < 0.1$

Notes: Results reflect estimation of equation 4 where the ATT is estimated using equation 2. The dependent variable is listed in the first column and reflects various types of arrest offenses. Columns (1)-(3) display the estimates from the sample of elections involving a Black victory, and columns (4)-(6) are the subset of victories where the local government has mayor-council organization. Columns (2) and (5) reflect estimates conditioned on the Black population share in 1970, and columns (3) and (6) are also conditioned on the 1970 population. Multiplier bootstrap standard errors are reported in parentheses and are clustered at the city level.

Table 9: Impact of Black Mayor on Offenses Known to Law Enforcement

Dep. Var	All Cities			All Cities - Mayor-Council		
	(1)	(2)	(3)	(4)	(5)	(6)
All Crimes	-49.37**	-6.90	-19.53	-69.76***	-14.38	-38.22
	(23.84)	(27.82)	(27.87)	(21.23)	(22.53)	(28.87)
Murder	-0.02	0.01	-0.08	0.06	0.13	-0.19
	(0.07)	(0.06)	(0.08)	(0.16)	(0.14)	(0.16)
Violent Index	-3.00	11.64*	7.88	-2.71	14.77***	4.31
	(4.37)	(6.89)	(6.71)	(4.42)	(5.39)	(5.83)
Property Index	-26.40*	-0.68	-9.70	-36.98**	-1.76	-19.60
	(15.21)	(18.02)	(19.40)	(16.86)	(16.34)	(17.71)
Total Index	-29.20	11.16	-1.68	-39.42**	13.27	-14.51
	(18.37)	(23.54)	(26.07)	(19.49)	(20.76)	(21.52)
Black Share		X			X	
BS + X			X			X
Cities	137	137	137	110	110	110
Treated Groups	33	33	33	21	21	21

***$p < 0.01$;** $p < 0.05$;* $p < 0.1$

Notes: Results reflect estimation of equation 4 where the ATT is estimated using equation 2. The dependent variable is listed in the first column and reflects measures of offenses known to law enforcement. Columns (1)-(3) display the estimates from the sample of elections involving a Black victory, and columns (4)-(6) are the subset of victories where the local government has a mayor-council organization. Columns (2) and (5) reflect estimates conditioned on the Black population share in 1970, and columns (3) and (6) are also conditioned on 1970 population. Multiplier bootstrap standard errors are reported in parentheses and are clustered at the city level.

Table 10: Impact of Black Mayor on Officers and Expenditure

Dep. Var	All Cities			All Cities - Mayor-Council		
	(1)	(2)	(3)	(4)	(5)	(6)
Police (per 10K)	1.09**	0.51	0.85	1.92***	1.14*	2.04***
	(0.48)	(0.50)	(0.55)	(0.62)	(0.61)	(0.73)
FT Employees (per 10K)	9.50***	2.49	3.73	8.29	-2.09	3.11
	(3.33)	(4.66)	(4.84)	(5.79)	(7.77)	(6.50)
Police Employment Share	-0.11	0.24	0.26	-0.60*	-0.01	0.48
	(0.44)	(0.42)	(0.41)	(0.34)	(0.40)	(0.45)
Police Expenditure	8.24	0.10	6.75	19.88**	8.82	18.82
	(7.45)	(7.28)	(7.71)	(9.78)	(10.38)	(12.38)
Police Expenditure Share	-0.76***	-0.43	-0.29	-0.81***	-0.27	-0.26
	(0.22)	(0.30)	(0.33)	(0.22)	(0.48)	(0.47)
Black Share		X			X	
BS + X			X			X
Cities	137	137	137	110	110	110
Treated Groups	33	33	33	21	21	21

$^{***}p < 0.01;^{**}p < 0.05;^{*}p < 0.1$

Notes: Results reflect estimation of equation 4 where the ATT is estimated using equation 2. The dependent variable is listed in the first column and reflects measures of public finance as they relate to law enforcement and the size of the local government. Columns (1)-(3) display the estimates from the sample of elections for all cities whose populations exceeded 75,000 and had a Black population of 4 percent in 1970, and columns (4)-(6) are the subset of victories where the local government has a mayor-council organization. Columns (2) and (5) reflect estimates conditioned on the Black population share in 1970, and columns (3) and (6) are also conditioned on 1970 population. Multiplier bootstrap standard errors are reported in parentheses and are clustered at the city level.