

THE ECONOMIC & SOCIAL CONSEQUENCES OF BLACK MAYORALTIES

By

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*For Merrill and Betty*

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## Chapter 1

### Introduction

Turning back the clock to one year before the Civil Rights Act, Black and White Americans were asked if “Black people have as good a chance as White people in your community to get any kind of job for which they are qualified?”<sup>1</sup> White respondents responded in the affirmative 41 percent and Black respondents were at 23 percent. When asked the same question in June 2021, Whites increased to 60 percent and Blacks increased to a still low 31 percent. The high watermark for either race was the first year of the Obama administration, the United States’ first Black president. Both White and Black Americans attributed some aspiration to the color of Obama’s skin despite him running a relatively race-neutral campaign, but how much, if any, can a Black candidate do for Black people?

This dissertation examines executive representation of Black Americans in U.S. cities. The guiding question is do Black mayors affect the economic and social outcomes of Black Americans? How do they? Underlying the exploration is that place, period, and institutions all serve to bolster or mitigate mayoral impact. The effect Black mayors have on municipal policy especially as it involves how it affects Black residents has been of interest to academics since the first Black candidates—post-Reconstruction—were elected in 1967. Since then, Black Americans have made large gains in municipal government, but mayors are still more likely to be White and male (Kirkland 2017), and the economic and social progress Black Americans have made relative to White Americans has been small or transient. The Black–White gap in unemployment remains unchanged since 1970, the gap in wages has been stagnant for 20 years, and the subject of police discrimination and brutality against Black Americans remain a contentious political and social topic despite policy efforts to address all of these.

Whether increasing descriptive representation reduces racial disparities is the subject of a broad inquiry. Among the first to address the issue, R. Browning, Marshall, and Tabb (1999) find few changes after the election of a Black candidate when analyzing a sample of 10 large cities in California, two of which elect a Black candidate in the seventies. Hajnal (2001) summarizes the early work which finds Black mayors do “not greatly improve the well-being of the black community.” This work relied on smaller samples and could not examine effects that varied within the city. Hopkins and McCabe (2012) improve on this by using a larger sample of breakthrough elections but find that breakthrough mayors do not govern differently in observable ways from White mayors using both difference-in-differences and regression discontinuity designs. They do find changes in the composition of the police force, but otherwise find breakthrough mayors are institutionally constrained. Nye, Rainer, and Stratmann (2014) is among the first to examine the intra-city impacts of Black mayors. In a sample of 17 cities (33 MSAs), 12 (22) of which elect Black candidates, they find that Black employment increases with pronounced gains in municipal government using a triple-difference design with a lagged explanatory variable.

My dissertation pushes our understanding forward by overcoming three methodological issues. First, regression discontinuity is the traditional design for analyzing the effects of descriptive representation; however, Vogl (2014) demonstrates bunching at the 50 percent vote-share threshold in favor of Black candidates nullifying the validity of the design. Alternatively, the validity of difference-in-differences designs have been questioned because Black mayors are not randomly assigned and election timing is not purely random (see Aghion, Alesina, and Trebbi (2008)). Second, few studies have examined the intra-city impacts of Black mayors. Black mayors are elected with almost 100 percent of the Black vote with expectations of improving the Black community, therefore allowing for heterogeneous effects by race is crucial to understanding how descriptive representation affects Black Americans’ social standing. Third, effects are not only likely to be heterogeneous by race but also across space and time. Previous research has recognized this source of differentiation (Hopkins and McCabe (2012); Thompson 2006) but not analyzed this potential avenue of heterogeneity. I use a difference-in-differences design with variation in treatment timing which

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<sup>1</sup>[Gallup polls on racial relations](#)

obviates the challenge of endogenous campaign intensity and allows me to estimate both racial, spatial, and temporal heterogeneous treatment effects. I assuage concerns of potential violations of the difference-in-differences design by estimating event studies allowing me to inspect trends before the election of Black candidates. My findings suggest that Black mayors can improve the welfare of Black residents when they are endowed with powers to effect policy.

## **1.1 Chapter Summaries**

### **1.1.1 Black Mayors & Black Communities**

Do Black communities economically benefit from the election of a Black mayor? Leveraging elections where a Black candidate wins mayor for the first time and that Black Americans often reside near each other, I use a stacked difference-in-differences strategy allowing for heterogeneous treatment effects by ZIP-code racial composition to estimate the effect breakthrough Black mayors have on location-specific economic activity. I find majority-Black ZIP codes experience gains in all areas of economic activity relative to non-Black communities. Gains to the number of businesses in majority-Black zip codes are found in industries where the mayor and department heads exert influence over city contracts suggesting these figures are influencing the distribution of city funds. To provide evidence these gains are experienced by Black residents, I turn to the decennial Census and American Community Survey and find a 27 percent reduction in the Black–White self-employment gap on average. However, there exists a positive relationship between the Black–White self employment gap and relative self-employment gains suggesting institutional and historical factors may limit Black economic progress in areas of high disparity. These findings suggest a role for the government to reduce Black–White disparities in entrepreneurship by eliminating informational and structural barriers to participation.

### **1.1.2 Black Mayors & Race-Specific Arrests**

A central and recurring issue for city residents is public safety and treatment by the police. Given the repetitive nature of the issue a natural question is do elected officials have any power to curb crime or poor police behavior. Elected Black candidates have particular interest in the issue because Black communities are victimized by high levels of crime and poor police–community relations. Using data on elections of first time Black mayors, I show that mayors may be limited in their ability to affect race-specific arrest patterns. I find that after pivotal elections, police forces become more diverse in race and gender which is especially true for mayors with executive authority. Arrests for discretionary offenses tend to decrease qualitatively for Black Americans in the post period which is not similarly visible for White Americans. I conclude my analysis by documenting the relationship between police force manpower and diversity on arrests. Increasing the number of Black officers tends to reduce the number of violent arrests of Black Americans suggesting increasing police diversity may reduce particularly costly crimes.

### **1.1.3 A Historical Perspective Of Black Mayors**

After the Civil Rights Act, Black candidates began assuming positions of power in large municipal governments for the first time since the end of Reconstruction. While it is granted that their central goal was to improve welfare of Black residents, a less understood aspect of Black mayors' approach is differences of governing strategy caused by the period in which they're elected. Drawing on evidence from four Alabama cities —Birmingham, Mobile, Montgomery, and Huntsville —three that elect a Black candidate and one that does not, I shed light on differences in resident outcomes driven by election timing with particular emphasis on outcomes that are econometrically difficult to identify.

## Chapter 2

### Black Mayors & Black Communities

Across a range of economic outcomes, Black Americans are drifting further apart from White Americans.<sup>2</sup> Economic gaps are exacerbated because Black Americans by history, preference, or policy live next to each other (see Frey (2021)). The relative lack in communal resources leads to lower wealth accumulation, lower rates of entrepreneurship (Hipple and Hammond 2016), and higher rates of downward mobility (Chetty et al. 2020).

This raises the question of whether targeted policies might address those gaps. At the city level, policymakers may employ various policy levers from localized infrastructure investment, business relief to depressed communities, minority purchasing programs, and entrepreneurial support for disadvantaged groups. Because policies implemented will vary across jurisdictions, examining the political actors who will represent race-specific voter interests assists in our understanding if economic gaps can be addressed by political means. Representation for disadvantaged and underrepresented groups has received renewed focus from policymakers, media figures, academics, and voters alike. It is unclear, however, if electing policymakers that reflect the characteristics of their constituents translates to better outcomes. A central question is, then, does the election of Black candidates lead to better economic outcomes for Black Americans and their communities and to what extent, if any, do they gain relative to White Americans.

In this paper, I examine the role Black mayors play in influencing the distribution of economic activity across a city. The difficulty in identifying the effect of Black mayors on Black outcomes are manifold. Annual, individual-level data were only available for the largest cities and at the metropolitan statistical area which leads to three problems. First, mismeasurement arises from unaffected residents who live in suburbs and or in another city that shares the same MSA. Second, if Black candidates produce material gains for Black residents specifically, then effects are obscured by observing citywide outcomes. Third, identifying Black-specific effects are difficult using surveys such as the CPS which collected data from 60,000 households monthly (compared to 295,000 for the American Community Survey) meaning the likelihood of collecting sufficient data for all but the largest cities was impossible. Additionally, Black candidates are not randomly assigned to govern, so the conditions that lead to their elections also require scrutiny.

I overcome these challenges by leveraging the fact that Black Americans often reside in the same areas so, I begin my exploration by focusing on economic outcomes by ZIP code that vary by racial composition. While ZIP codes are collections of postal routes, residents understand the demographic context of their ZIP code better than self-defined measures of community (Velez and Wong 2017). Focusing on majority-Black ZIP codes has two other advantages as well. Black Americans in majority-Black communities participate in elections at high levels despite longer wait times at the polls and directing policy towards blocs of voters is practical (BlackFuturesLab 2019; Ray and Whitlock 2019). Further, Black residents live in majority-Black and majority-minority ZIP codes more often than they do in majority-White ZIP codes (Farrell, Wheat, and Grandet 2019; Frey 2021), so focusing the analysis on communities<sup>3</sup> is an intuitive choice.

I study elections of Black candidates who become a city's first directly elected Black mayor — hence, breakthrough — between the years 2000 and 2010 to determine the effect descriptive representation has on race-specific community development. Breakthrough elections data are paired with the ZIP Codes Business Patterns (ZBP) data that measures the local employment, establishments, and payroll level. In order to determine the differential effect between Black and White communities, I use a difference-in-differences (DD) design where I allow for heterogeneous effects between communities by racial composition. Therefore, I can identify the effect breakthrough Black mayors

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<sup>2</sup>For the Black–White wage gap see [B-W Wage Gap: Economic Policy Institute](#). For B–W homeownership gap see [B-W homeownership gap: Urban Institute](#). For the B-W unemployment gap see [B-W unemployment gap: Center For American Progress](#). For a glimpse at representation in the most powerful positions in America across government, industry, and academia see [NYT: Faces Of Power](#).

<sup>3</sup>When referring to “communities,” I am referring to ZIP codes, the smallest spatial unit in my analysis.

have at the city level, absolutely for Black and non-Black communities, and the relative difference in economic activity between Black and non-Black communities.

I compare economic activity in cities that elected a Black candidate for the first time to cities that observed at least one interracial election after 1994 or elected a breakthrough Black candidate more than 8 years after the focal breakthrough. Validity of the design requires that the evolution in economic activity between treatment and control would have evolved in parallel absent the breakthrough. I examine the validity of the design by estimating a “stacked” event study for community-specific effects where each breakthrough election is its own dataset composed of the six years before a breakthrough mayor assumes offices and the eight years following.<sup>4</sup> I then summarize my findings into a single post-breakthrough election treatment effect.

Relative to non-Black communities, after a breakthrough election, I find gains of approximately 1000 workers (6.3 percent) and 40 businesses (4.6 percent) in Black communities. Relative gains persist eight years after the breakthrough election. These gains appear to operate through a redistribution channel as non-Black communities experience declines in both categories. In absolute terms, Black communities experience small, but imprecise, positive gains in employees per resident. Establishments per residents grow the first four years but return to baseline by the eighth year, but payroll remains elevated. Non-Black communities experience small and statistically imprecise declines in employment and establishments per resident four years after a breakthrough election which continues after the first term. In the aggregate, cities experience a 2.2 percent decline in employees per resident. Establishment gains are found in cities where the mayor serves as chief executive or was re-elected, and there are no gains in cities where an interracial election was held but the White candidate won suggesting a causal role of the mayor in redistribution.

I then turn to examine whether establishment gains are experienced in industries plausibly affected by municipal contracting, an area where the mayor exerts direct influence. Because we might expect a shift in city contracts granted from Black firms to White firms, we should expect increased competition and entry in industries affected by municipal contracting. I find some evidence to support this conclusion; Black communities observe an 8 percent relative gain in the number of construction firms, but otherwise no gains in other industries plausibly affected. Establishments in non-Black communities do not experience gains across all industries. In addition, I find economically and statistically significant evidence indicating the election of a breakthrough candidate induces relative and absolute gains in businesses with fewer than 10 employees consistent with a Black mayor and their administration incorporating smaller, lower capitalized businesses into purchasing programs. Businesses with fewer than 10 employees in non-majority-Black communities experience declines over the eight-year period consistent with the mayor redistributing governmental resources from non-Black to Black communities.

Because I cannot rule out that gains observed in majority-Black communities are being experienced by non-Black residents, I turn to the decennial census and American Community Survey to provide evidence that gains in self-employment are only experienced by Black residents even after including a set of controls for income, age, and education. I compare the self-employment results with estimates for the city-specific Black–White self-employment gap, and find a positive relationship between relative gains in self-employment and the B–W self-employment gap suggesting historical or institutional factors may limit the mayor’s ability to forward Black economic progress. On average, I find a reduction in the B–W gap in self employment of 27 percent in the decade after a breakthrough election. My findings suggest that electing candidates committed to the reduction of Black–White disparities can have tangible effects.

Previous work has recognized the importance of examining Black policymaker impact on Black constituent outcomes and racial inequality; however, my work is the first to leverage where Black Americans live to provide understanding on how Black policymakers influence the distribution of economic activity. Nye, Rainer, and Stratmann (2014) address a similar question. Using a sample of Black mayoral elections from large cities from 1973 to 2004, the

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<sup>4</sup>Stacked estimation is robust to pitfalls from OLS using conventional two-way fixed effects such as heterogeneous treatment effects. For more see Section 3.4.

authors find increased employment for Black Americans relative to White Americans and these gains are especially pronounced within municipal government. Logan (2020), in the context of Reconstruction, demonstrates that Black policymakers had diverging policy goals from White lawmakers, and they were effective at increasing tax revenues to improve literacy and school enrollment for African Americans. Hopkins and McCabe (2012), using both difference-in-differences and regression discontinuity, find that the election of a Black mayor does not change municipal revenues or expenditure; however, the municipal workforce becomes more diverse, namely the police force<sup>5</sup>. Earlier work from Hajnal (2001) and R. Browning, Marshall, and Tabb (1999) buttress the conclusion that Black mayors govern no differently than Whites but could not speak to intra-city variation in race-specific outcomes.

I build and improve upon this work in four ways. First, recent innovations in difference-in-differences methodology have called into question results from the estimation of traditional panel two way fixed effects model in the presence of treatment effect heterogeneity. Second, gains in municipal employment are not a likely channel in the present context as Black Americans are overrepresented in public employment, and while the authors suggest Black mayors may introduce policies that boost Black private employment, they do not examine how race-specific policy changes likely boost Black entrepreneurship. Third, previous work has described the effect of a subset of Black mayors elected in the seventies and eighties that inherited cities with eroding tax bases and transitioning employment-sector composition (e.g. manufacturing base to service base). Documenting the impact of these figures is of large historical interest and has received much scholarship; however, we should expect the effect of Black candidates currently elected to differ from those in earlier periods which motivates my focus on breakthrough mayors elected from 2000 to 2010.<sup>6</sup> Fourth on the presumption that Black mayors do effect the distribution of resources, then identifying the causal effect of subsequent Black mayors is confounded by the initial breakthrough if race-specific trends in economic outcomes are changed.<sup>7 8</sup>

My findings add to a broader literature on candidate identity. Beach et al. (2019) use close elections for city council in California to show that victories by non-White candidates improve housing values in majority-ethnic neighborhoods suggesting localized improvements in minority neighborhoods which are capitalized into housing. Ferreira and Gyourko (2014) discuss the role gender plays in politics, but they find no role for gender in influencing policy or the size and scope of government. Similar to Logan (2020), I find that the identity of individual candidates can produce meaningful changes when considering the institutional context that grants the candidate authority.<sup>9 10</sup>

This work bridges the literature examining the economic and political effects of the VRA and the Great Migration at the middle of the 20th century. This paper shows that decades after the enfranchisement of millions of Black

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<sup>5</sup>A similar result is found by McCrary (2007) who discusses the effect of court-ordered affirmative action mandates on the composition of the police force. In numerous cases (e.g. New Orleans, Cincinnati, Philadelphia), Black mayors are acquiescing to the injunctions that were previously ignored by White mayors.

<sup>6</sup>Black mayors, and to a larger extent, breakthrough Black mayors have been categorized into three groups by the timing of the first election: civil rights mayors, technocrats/coalition builders, and “trans-racial appealists,” breakthrough mayors whose “appeal goes beyond racial boundaries” (Saunders 2014). Saunders roughly categorizes the time periods as before 1975, 1975-1990, and 1990 to the present. Importantly, he notes “Taking a long historical view, it’s clear that the people who became first African-American mayors beginning in the late ’60s and continuing through today held different views, developed different paths to victory and methods of governance, and had differing perceptions of their skills among their constituents.”

<sup>7</sup>For more on changes in trends in post-breakthrough cities, see Appendix 2.8.1.3

<sup>8</sup>Historical perspectives underscore how Black mayors direct public resources toward Black residents and businesses and provide in-depth, city specific context to challenges Black mayors faced. e.g. Kruse (2005) discusses Atlanta’s early Black mayors’ political goals describing how Maynard Jackson “forcefully tried to ‘integrate the money,’” and Andrew Young “never met a building permit he didn’t like.” Sugrue (1996) documents the same relationship in Detroit where Coleman Young “used city employment and city contracts to reward loyal supporters.” Piliawsky (1985) examines community-related development after the election of Ernest Morial in New Orleans and documents that rewards went to Black middle- and upper-class residents while low-income residents saw fewer returns.

<sup>9</sup>For other work on descriptive representation see Beach and Jones (2017), Pande (2003), and Sances and You (2017).

<sup>10</sup>Much of this work and recent work relies on regression discontinuity designs to identify the effects of racial or ethnic identity on minority outcomes; however, Vogl (2014) analyzes the validity of regression discontinuity design for interracial mayoral elections involving Black candidates and finds evidence of endogenous campaign intensity in interracial elections in the South nullifying the use of regression discontinuity in this setting. For more see 2.4.1

Americans, representation still produces benefits beyond what incorporation at the ballot box does. Important to this work is Derenoncourt (2022) who studies how changes in commuting zone racial composition during Great Migration reduced long-run mobility. Derenoncourt finds that the Great Migration reduced intergenerational mobility for low-income families which are driven by neighborhood factors such as changes in local policy. This work builds upon the fact that these neighborhood factors remained, to a large extent, unaddressed. Black policymakers recognize these deprivations and implement policies to economically improve these areas.

## 2.2 Background

### 2.2.1 How Powerful Is The Mayor?

The role Black policymakers, perhaps more importantly barrier-breaking candidates, play in altering the trajectory of the communities they govern is unclear. It is clear their presence is of symbolic importance as their appearance on the ballot drives numerous disaffected voters —Black or otherwise—to the ballot box (Vogl 2014; Washington 2006). Precisely because Black candidates hold such symbolic and nebulous significance, their effect on the local economy remains understudied; however, any impact a policymaker may have is filtered through their position of power and the institutions which grant them authority. Following I briefly outline the governance structures of local government, and the powers or constraints mayors may face therein.

Municipal governments are “creatures of the state,” so cities derive their powers from state constitutions. States follow various principles of local government authority which provides legislative latitude for municipalities; however, there are two general forms of municipal government: mayor–council and council–manager<sup>11</sup>. In mayor–council governments, the mayor serves as the chief executive of the government. The mayor appoints department heads, hires executive staff, proposes the budget, and may serve as a member of planning and/or development boards (sometimes in an *ex officio* status). In council–manager governments, the mayor is a council person whose powers in general do not differ from other council persons. Mayors in this form preside over council meetings and may have veto power, though not common. The executive department and day-to-day operations are left to a hired professional city manager.<sup>12</sup> This suggests we should expect mayors in mayor–council governments to exert more influence over municipal policy. Other institutional factors may work to limit mayoral impact; for example, a typical term length is four years, but some terms may be as short as two which may limit mayoral effectiveness due to the frequency of the campaign and election cycle.

Besides institutional constraints, political actors militate against mayoral agendas that are deemed far reaching. The mayoral agenda relies on majority support from the city council and only in rare cases are a majority of city councillors persons of color when the first Black candidate. In council–manager forms, this problem is amplified when each council person represents a specific district. In Tiebout (1956) models of government expenditure, local governments compete for taxpayers, and taxpayers can “vote with their feet.” If subject to an increase in taxes, firms may decide to relocate reducing employment opportunities and increasing the difficulty of starting a business for already undercapitalized minority entrepreneurs. Voters or businesses with limited political power alone form interest and civic groups, such as unions, chambers of commerce, political committees, and neighborhood associations, whose support the mayor relies on to communicate the benefits of their proposed policies to group members. Thus, a Black mayor’s policy preferences are constrained by competing political interests, voter and business responsiveness to tax changes, or resource reallocation from one community to another (also see (Rose-Ackerman 1979; Peterson 1981)). Rae (2003) echoes this constraint commenting upon his time in the John C. Daniels administration (New Haven’s first Black mayor) “[we] were (to say the least) less than effective in wielding expectations of African-American supporters to the policy options and employment opportunities available from the administration,” whereby he describes difficulty bargaining with the New Haven Democratic Town Committee and factions within the administration.

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<sup>11</sup>County commissions are a variant.

<sup>12</sup>Think Chris Traeger from *Parks And Rec*.

### 2.2.2 Neighborhood Segregation & Its Economic Implications

A historical feature of American society has been that the average Black American does not live near many White Americans. The definition of “near” has been challenged and amended, and segregation (and its resultant measures) are multidimensional. The conclusion that America is a segregated society, especially for Black Americans, is unchanged. Reviewing segregation in 1990, Massey, White, and Phua (1996) conclude, “Among all groups, Blacks are uniquely segregated in U.S. urban areas.” Another way to view segregation which has implications for my investigation is to ask what is the neighborhood context in which the average Black American lives. The average Black resident in 2000 lived in neighborhoods that were composed of more than half of Black residents (Frey 2021). Segregation has both social and economic implications for Black Americans. For concision, I will focus on the economic implications.

There are two particular constraints faced by Black Americans and their communities. First, businesses in Black communities suffer from operating in economies parallel to businesses in White communities (Baradaran 2017). Businesses in majority-Black communities have liquidity constraints and small profit margins which is partially driven by low levels of wealth and assets held by Black Americans; however, businesses in Black communities, regardless of owner race, perform worse than comparable and lower-rated businesses (Farrell, Wheat, and Grandet 2019). A. Perry, Rothwell, and Harshbarger (2020) find that highly rated businesses in Black communities experience lower revenue growth than poorly rated businesses in neighborhoods with nearly no Black residents which they conclude leads to billions of dollars in lost wealth. Second, while it is true that Black and White business owners are penalized for their geography, Black entrepreneurs receive an additional penalty as a consequence of their race. Fairlie, Robb, and Robinson (2020) examine racial disparities in access to capital for startups and find Black-owned startups are smaller than their White counterparts and remain that way. This is true of Black- versus White-owned businesses operating in the same ZIP code controlling for a range of factors such as credit-worthiness, income, and education.

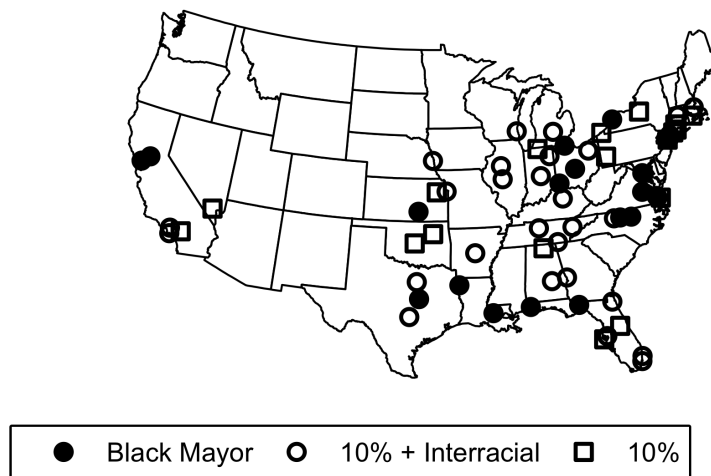


Figure 2.1: Sample Cities By Breakthrough Election

Sources: Vogl (2014), Ferreira (2009)

## 2.3 Data

### 2.3.1 Mayoral Data

Data on elections come from two primary sources. The first is source is from Vogl (2014) which contains elections from medium and large U.S. cities beginning in 1965. The data include candidate race and party, year of election, city, and margin of victory. I supplement this data with data from Ferreira and Gyourko (2009) which provide similar information except for candidate race. For relevant elections not included in either file and to extend the included cities further than 2010, I use state or local elections websites<sup>13</sup> and ourcampaigns.com, a crowdsourced “political data resource whose mission is to collect and make available information about all official elections, historical, current and ongoing.” The website often contains photographs of candidates as well as other resources to help identify candidate race. I verify this information by searching for news sources that explicitly mention the candidate is the first Black mayor or the runner-up is Black. Figure 2.1 displays the set of cities under consideration for this exercise separated by treatment status. The sample of cities are all cities with populations greater than 100,000 in 1990 whose Black population share is at least 10 percent. For my main analysis, I restrict the set of control cities to those that also experience an election where a Black candidate finishes as the runner-up to reflect cities that *could have* elected a Black candidate. Because electoral rules can be used to limit or block the supply of minority candidates, I show in the appendix results where I relax this restriction (Aghion, Alesina, and Trebbi 2008; Shah 2014). I exclude all cities that elected a Black mayor before 2000. If a breakthrough mayor assumed office at the end of the year, they are coded as assuming office in the next year. The sample window contains 18 breakthrough elections that cover all four census regions plotted with solid circles. The South is responsible for 10 elections, followed by the Midwest with four; the North and West each contain two breakthrough elections respectively. Control cities are plotted using hollow shapes. Cities that observe at least one interracial election after 1994 are depicted with hollow circles, and cities that did not but met the 10 percent Black population criteria are depicted with hollow squares.

### 2.3.2 ZIP Code Level Data

The ZIP Codes Business Patterns (ZBP) provides annual data on number of establishments, employment, and payroll beginning in 1994. Establishments are physical locations where business is conducted and may not be identical to an enterprise.<sup>14</sup> The establishment count represents the total number of locations with paid employees at any time during the year. Employment captures all full- and part-time employees on payroll in the pay period including March 12. Self-employed individuals, employees of private households, and most government employees are excluded. Some employment data are suppressed to avoid disclosing figures for individual establishments. Wherever possible, I interpolate the employment number using the two closest years. From 2007 forward, the Census began infusing these numbers with noise to obviate this problem. Approximately 90 percent of total employment is covered by the ZBP. ZBP data are not subject to sampling error as it is taken from the Business Register, a file of all known single- and multi-establishment companies. All ZIP codes with populations over 1000 residents that report positive employment for the year are retained for the analysis.

In addition to the total number of establishments and employees at the ZIP code level, the ZBP provide an industry-by-establishment-size breakdown of establishments. From 1994 to 1997, establishment industries were recorded using Standard Industrial Classification codes (SIC). After 1997, they were recorded using the North American Industry Classification System (NAICS). When a SIC code corresponds to one NAICS category, I match it to the corresponding NAICS category. I exclude instances where a SIC code corresponds to multiple categories or there is not a similar NAICS category.

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<sup>13</sup>e.g. Neither file contains information on elections for independent cities in Virginia, so I consulted the Virginia state elections website which contains all elections beginning in 2000.

<sup>14</sup>An enterprise is a business with one or more physical establishments, e.g. Walmart.



Demographic data at the zip-code level are taken from the integrated public use microdata (IPUMS) NHGIS which provide decadal data on population and housing at the zip code tabulation area (ZCTA) level (Manson et al. 2019). ZIP codes are not “true” areal units but instead a collection of postal routes. Because ZIP codes are used to tabulate data, the Census Bureau drew geographic boundaries for them in 2000, creating ZCTAs, to approximate the geographic extent of ZIP codes. I match the five-digit ZIP codes to the ZCTA. Most ZIP codes directly match the ZCTA but some ZIP codes are large post offices, individual buildings, or complexes. This may be the case for universities or government buildings that have intense postal activity. I exclude these instances from the analysis. I retain ZCTAs where more than 50 percent of the ZCTA population is contained within the target city. I show in the appendix that my results are not sensitive to more restrictive definitions of ZCTA. Moving forward, I use the term “zip code” to refer to both ZIP codes and ZCTAs<sup>15</sup>.

The zip code data are merged to the data on mayoral elections where the target of analysis is zip codes in cities that elected a Black mayor for the first time between 2000 to 2010 inclusive. The set of control cities is all cities that experienced an interracial election starting in 1994 with Black populations above 10 percent. This set also includes cities that elected a Black candidate after 2010 as well. The sample includes 18 treatment cities and 31 control cities or 874 zip codes, 129 of which are majority Black.

### 2.3.3 Summary Statistics

Table 2.1 separates the sample by treatment status and reports balance between treatment and control. The final column contains the difference between treatment and control and prints the t-stat for significant differences between the groups below the difference. The first panel presents results at the city level for 1990. If treatment cities collect more revenue than control cities then this allows them to more effectively implement policies or provide government services which contributes to local productivity. Examining revenue measures, treated cities collect more sales taxes, though not statistically different than untreated cities; however, treatment and control cities are no different when comparing property taxes per capita and, more importantly, total revenue collected. Untreated cities have more outstanding debt and spend more on police than treated cities on average, but the standard deviations are large and neither of these differences are significant. Treated cities have higher Black populations and, in turn, lower White populations which is to be expected in this context. Important to the design is that there are not differential trends in Black population growth between treated and control. I show in the appendix that in the six years before pivotal elections, the Black population share is stable so changes to the Black population are not causing Black candidates to be elected. Cities are balanced in both their institutional arrangement and dissimilarity score. Dissimilarity measures the evenness of dispersal between two demographic groups. The index can be interpreted as the percentage of people from one demographic group who would have to move to a different census tract in order for the census tract distribution between the two groups to match the city-level distribution. Both treated and untreated cities have high dissimilarity scores showing that sample cities are highly segregated and further motivating a design that leverages place-specific effects.

The second panel summarizes the zip code characteristics by treatment status. There are no statistically significant differences between treated and untreated zip codes beyond what has been discussed previously. In general, economic activity is greater in the untreated zip codes on average, but the estimates cover a range of outcomes which reflects treatment variability for both groups. Establishments are higher in untreated zips, but are not changing differentially in the pre-period. This likely reflects differences in land use rules and resident preferences across geographies which reflects the importance of unit fixed effects. The differences in Black and White populations between city and zip codes demonstrates the zip codes that are mostly contained in the city contain slightly more White residents, but this is of limited concern as the pattern is not correlated by treatment status.

The final panel contains non-exhaustive information on breakthrough mayors. Breakthrough mayors assume

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<sup>15</sup>“ZIP” refers to a lapsed USPS servicemark meaning “Zone Improvement Plan.”

the mayoralty with more than 8 years of legislative experience. This reflects elected positions in municipal, county, and state government.<sup>16</sup> This is similar to the candidates they face. Service on city council may best prepare mayors for duty as interacting and bargaining with city council members constitutes a significant portion of the job. In addition, previous service on the council leads to a better understanding of idiosyncratic procedural rules that may allow winners to be effective more quickly than novice candidates. City council experience is not different between breakthrough candidates and the candidates they defeat with each averaging slightly more than a traditional term, but the runner-up White candidates were more likely to serve on the city council beforehand.<sup>17</sup>

In the appendix, I present summary characteristics of my sample versus all 1990 cities with similar populations. The sample mirrors the larger universe well; however, drawing broader conclusions from this sample to the population may be dubious. Most excluded cities are those with minimal Black populations so the range for racially-directed policy may be limited and early-adopting cities who may not be able to pursue similar policies because they have likely already used similar levers at earlier periods.

	Treated	Untreated	Difference
<hr/> 1990 City Characteristics			
N	18	29	
Population	239939 (125733)	277188 167202	-37,249 (-0.87)
Property Taxes	444.46 (504.60)	488.42 (378.33)	-43.96 (-0.32)
Sales Taxes	117.81 (142.10)	79.80 (130.33)	38.01 (0.92)
Total Revenue	2225.63 (1217.66)	2797.85 (1617.50)	-572.22 (-1.38)
Debt Outstanding	2766.69 (1518.96)	4011.81 (5157.88)	-1244.82 (1.22)
Police Spending	223.43 (69.91)	240.78 (105.80)	-17.35 (-0.68)
Black Pop. Share	31.32 (11.93)	24.23 (8.16)	7.09 (2.22)**
White Pop. Share	61.55 (10.41)	69.01 (8.21)	-7.46 (-2.58)***
Median House Value	82800 (45570.97)	87579.31 (61833.28)	-4779.31 (-0.30)
Median Rent	355.28 (106.99)	362.48 (89.56)	-7.20 (-0.24)
Mayor-Council	55.55 (51.13)	72.41 (45.49)	-16.86 (-1.15)
Dissimilarity	58.42 (11.87)	60.15 (11.56)	-1.73 (-0.49)
<hr/> 1966-1999 Zip Code Characteristics			
N	256	618	
Population	21986 (12406)	20793 (13311)	1193 (1.27)
Annual Payroll	25295.72 (65864.49)	36071.74 (148381.92)	-10776.02 (-1.49)
Wage	26.37 (8.94)	27.19 (9.83)	-0.82 (-1.98)**
Employment	778.42 (1588.70)	954.70 (2509.10)	-176.28 (-1.25)
Establishments	35.86 (59.35)	44.47 (86.05)	-8.61 (-1.70)*
Black Share	28.95 (26.44)	19.56 (22.29)	9.39 (4.99)***
White Share	65.70 (25.94)	73.74 (23.22)	-8.04 (-4.50)***
<hr/> Breakthrough Candidate Characteristics			
Age	50.13		
Legislative Experience	8.47		
Council Experience	4		
Ever on Council	0.625		

Table 2.1: Balance of Characteristics between Treatment & Untreated

<sup>16</sup>I exclude school board positions because I could not reliably observe whether these positions are elected or appointed, and in many cases this position is less reliably reported relative to other local positions such as city council or county commissioners.

<sup>17</sup>There may be unobservable (to the econometrician) characteristics that make Black candidates more competent beyond their legislative experience. For instance, one mayor was previously a governor, others were a part of classes that integrated universities, or were previously elected to positions where they were the first Black person to hold that position.

## 2.4 Empirical Strategy

To examine the relationship between the election of a breakthrough Black candidate and local economic activity, I use a difference-in-differences (DD) design where I allow for heterogeneous treatment effects according to zip code racial composition. I compare economic activity before and after the election of a Black candidate, relative to cities that observed an interracial election or a Black candidate was elected at a later date, and for majority-Black communities relative to non-majority-Black ones. A common refrain is that “demographics are destiny” meaning that the likelihood of electing a minority candidate — in this case a Black candidate — is dependent on the minority population. However important to this study is that city demographics are not differentially changing and do not predict when that destiny arrives. In the appendix, I show that some 1990 city characteristics predict the likelihood of electing a Black candidate but no characteristic predicts the timing of electing a Black candidate which motivates a strategy where I incorporate cities that could have elected Black candidates and those that do over the sample period.

Similar to Cengiz et al. (2019) and Deshpande and Li (2019), I construct an event-specific dataset from the 18 breakthrough elections. Each event consists of one breakthrough election. The event captures the 6-year pre-event window and the 8 years following where no other unit in the dataset is treated. For example, the Columbus, Ohio breakthrough election (2000) contains all control cities that observe an interracial election and the cities that elect a Black mayor after 2008. Thus, each dataset is a single-treated panel and all other units are “clean controls” that do not experience a change in treatment status over the event window. Then, I stack the 18 breakthrough elections aligning them in event time. Stacked estimation has two critical advantages over traditional two-way fixed effects estimation that previous literature (Goodman-Bacon 2021; Callaway and Sant’Anna 2020; and deChaisemartin and D’Haultfœuille 2020) has shown to be important in this setting. Because each panel is a single treatment, the estimation procedure does not suffer from attenuation bias due to heterogeneous treatment effects arising from multiple treated units. Because events happen contemporaneously (relatively speaking) by design, I remove the concern of potential negative weighting of breakthrough elections caused by conventional two-way fixed effects estimation and weights are not differentially influenced by their treatment length or the size of their timing group.<sup>18 19</sup>

My interest is in whether Black candidates who are the first to become mayor of a city affect economic development conditions given a community’s racial composition. I focus on the first mayor because the treatment effects of successive mayors are confounded by the initial treatment if trends in economic activity are changed. Because elections are non-random events and Black mayors are not assigned at random, a threat to this design is that economic activity in Black communities where a Black candidate is elected are changing differently than Black communities where they are not violating the parallel trends assumption. This could be because the eventual breakthrough mayor currently sits on the council or council composition is changing every election cycle in a manner that benefits Black communities. Or from the demand side, voter preferences could be changing in a way beneficial to race-specific policies. A common method of assessing the plausibility of the parallel trends assumption is through an event study where the pre-treatment leads indicate if the dependent variable systematically differs from zero. I formally estimate,

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<sup>18</sup>This is an unattractive feature to causally interpreting the estimate. In the context of my sample this is doubly problematic as the “2006” timing group sits at the middle of the panel and has the most treated units. Because of this, the “2006” timing group receives more than 30 percent of the weight in the estimation of the treatment effect. Estimates of these weights for each timing group can be found in Appendix Figure 2.20. I use the R function `bacon` from the `bacondecomp` package to estimate these weights.

<sup>19</sup>In the case where treatment effects vary over time, the two-by-two estimates may have negative weights because the change in already-treated units’ treatment effects are being subtracted when they are acting as control units. In the parlance of Goodman-Bacon (an intuitive example can be found in de Chaisemartin and D’Haultfœuille as well), the probability limit of the two-way fixed effects estimator can be expressed as  $plim_{N \rightarrow \infty} \hat{\beta}^{DD} = VWATT + VWCT - \Delta ATT$  where  $VWATT$  is the variance-weighted average treatment effect on the treated,  $VWCT$  is the variance-weighted common trend, and  $\Delta ATT$  is the change in the average treatment effect on the treated. In the case where the  $ATT$  is positive, increasing and larger than the change in the  $ATT$ , we have  $VWATT > 0$  and  $\Delta ATT > 0$  implying that  $\hat{\beta}^{DD}$  is attenuated though in other cases the estimated treatment effect may even be negative. In the appendix, Table 2.10 displays why this is a concern in this setting where policy is not likely to resolve over one period. Relative estimates for all outcomes using conventional TWFE are one-third to one-fifth the effect size of the stacked estimates, and the absolute Black effects are negative in the cases of employment and establishments.

$$y_{zct} = \alpha_{ez} + \delta_{et}^r + \sum_{k=-6, k \neq -1}^{k=8} \lambda_k \mathbf{1}[t - B_{ec} = k] + \sum_{k=-6, k \neq -1}^{k=8} \tau_k BZ_{ezc} \mathbf{1}[t - B_{ec} = k] + \varepsilon_{zct} \quad (2.1)$$

where  $y_{zct}$  are economic outcomes for focal election (event)  $e$  in zip code  $z$  in city  $c$  in years  $t = 1994, \dots, 2018$  such as the natural log of establishments, employment, and annual payroll per capita.  $\alpha_{ez}$  are a set of zip code–event fixed effects that capture observable and unobservable time-invariant differences between zip codes.  $\delta_{et}^r$  are a set of year–event by race  $\in \{\text{Black, non-Black}\}$  fixed effects that capture time-varying changes in the national economic climate that are invariant by zip code depending on racial majority. The relative-year indicators  $\mathbf{1}[t - B_{ec} = k]$  are equal to 1 if the year of observation  $t$  minus the year a breakthrough candidate first takes office  $B_{ec}$  is equal to  $k$ . Event year  $k = -1$ , the year before the candidate assumes office, is omitted. The event year dummies are unrestricted with a max  $k = 8$  and a minimum  $k = -6$  by construction.  $BZ_{ezc}$  is an indicator for zip codes that were majority Black in 1990. I set this at 1990 to avoid endogenous population changes potentially caused by the election of a Black candidate. The correlation to Black zip codes in 2000 is .85 and .72 for Black zip codes in 2010. Because the dichotomous indicator will not capture relative effects for three cities whose Black populations are relatively evenly distributed, I also report results where the explanatory variable is the 1990 Black population share interacted with treatment and results by quartile in the appendix. I conduct my analysis across 18 cities that directly elect their first Black candidate from 2000 to 2010 and 28 to 31 cities that do not elect a Black candidate before 2010 or observe one interracial election after 1994 where the Black candidate finishes as the runner up. I cluster my standard errors at the event (breakthrough election) level because control units will appear in each event and in one case one unit serves as both treatment and control.

Estimation of equation (2.1) provides a description of the effect the election of a breakthrough Black candidate has on non-Black communities, captured by  $\lambda_k$ , and the effect it has on the racial gap between communities in the same city, captured by  $\tau_k$ . The absolute effect on Black communities is found by the linear combination of  $\lambda_k$  and  $\tau_k$ . The leads on these coefficients ( $\lambda_k, \tau_k$  for  $k < 0$ ) provide a test of the parallel trends assumption. If the leads systematically differ from zero this would be evidence that the parallel trends assumption is violated.

I plot the response function over the entire sample window to capture the immediate effects candidates have upon taking office and medium- to longer-term effects which may reflect the endurance of any policy. Of course, not all cities operate under 4 year terms but this is the modal length, and once in office candidates have an incumbency advantage which is why the average stay in office is 8 years. Thus, this specification closely captures the political lifespan of a mayor, and their resultant effect on the local economy.

I then summarize these results by examining the effect from the election of a breakthrough candidate by collapsing the post effect into one coefficient and capturing the slope of the pre-election effects to improve precision. I estimate the following equation

$$y_{zct} = \alpha_{ez} + \delta_{et}^r + \tau_1 BM_{ect} + \tau_2 BM_{ect} \times BZ_{ezc} + \gamma_1 (t - B_{ec}) \times \mathbf{1}\{t < B_{ec}\} + \gamma_2 (t - B_{ec}) \times \mathbf{1}\{t < B_{ec}\} \times BZ_{ezc} + \varepsilon_{zct}, \quad (2.2)$$

where  $\mathbf{1}[BM_{ect}]$  is an indicator for having elected a breakthrough Black candidate in year  $t$  or before.  $(t - B_{ec}) \times \mathbf{1}\{t < B_{ec}\}$  captures the pre-election trend where the indicator  $\mathbf{1}\{t < B_{ec}\}$  is equal to one when the calendar year occurs before the breakthrough election year which I additionally interact with the Black zip code indicator.  $\gamma_1$  shows the pre-breakthrough trend in non-Black communities, and  $\gamma_2$  shows the Black–non-Black difference in pre-breakthrough trend and provides a test for differential linear pre-trends. The interpretations of the parameters of interest are slightly amended. Because the assumption is that any pre-breakthrough trend would have continued

into the treatment period,  $\tau_1$  describes the total effect the election of a breakthrough candidate has on non-Black communities as a deviation from any pre-breakthrough trend, and  $\tau_2$  describes the relative effect a breakthrough collection has on majority-Black communities as a deviation from any pre-breakthrough trend. Together,  $\tau_1 + \tau_2$  describe total effect the election of a breakthrough candidate has on majority-Black communities as a deviation from any linear pre-breakthrough trend. I present both the simple non-trend DD and my preferred specification initially to show that the specification is a good fit to the data and the interpretations are unchanged if using the other model.

#### 2.4.1 Why Not Regression Discontinuity or Lagged Independent Variable?

An intuitive and oft-used design choice for studies involving elections is regression discontinuity where the cutoff rule is applied around the margin of victory. In theory, we could then observe the change in economic activity within a small window of the margin of victory to identify the causal effect of electing a Black candidate on Black community outcomes; however, the validity of the RD design is dubious for two reasons.

First, a sufficient condition for the validity of RD is that treatment is “as good as random.” McCrary (2008) proposes a test that examines the density of the assignment variable. Vogl (2014) first describes the problem of endogenous campaign intensity in the context of racially polarizing mayoral elections, and I find a similar statistically significant result where more close elections are won than lost by Black candidates using cities with 1990 populations over 100,000. When restricting it to the set of cities to not have elected a Black candidate before 2000, elections around the threshold appear continuous; however, another problem arises.

Second, concerns over statistical power arise because the sample, with the inclusion of the additional sample restrictions, includes 45 cities encompassing 99 elections between a Black and a White candidate over the period. Power is a function of the treated proportion, sample size, and the variance of outcomes. Sample size is somewhat augmented from the investigation occurring at the zip-code level giving me 1224 zip-code by election observations. In the RD context, one would evaluate the change in the economic activity variables of interest all of which display high variability which increases the minimum detectable effect size.

Another design choice used in the political economy context is to lag the explanatory variable in order to obviate concerns of unobserved heterogeneity or reverse causality. Bellemare, Masaki, and Pepinsky (2017) show that this design often leads to incorrect inference. In the case of reverse causality the authors highlight two cases where a lagged explanatory variable is appropriate: (1) reverse causality is contemporaneous only, and the causal effect operates with a one-period lag only and (2) the causal effect is contemporaneous, and there are no dynamics in the dependent variable, but there are dynamics in the explanatory variable. Neither of these scenarios seems likely to hold in this context. In the first case, the political process is both contemporaneous and cumulative, therefore it is unlikely the effect of a Black candidate resolves itself the year after an election. In fact, there may be no visible difference at this point as policy itself may operate with some lag, and this is if policy priorities are efficiently implemented. In the second case, no dynamics in economic activity is a dubious assumption which can be tested by including a lag of the dependent variable in the regression.

I formalize all of these considerations in the appendix. First, I show bunching above the margin of victory threshold. Then using the restricted sample, I provide explicit power calculations that show that RD estimates are underpowered to detect most effects that I find in the DD context. Finally, I show estimates using both conventional two-way fixed effects estimation and a lagged independent variable leads to incorrect inference. Estimates using lagged variables are similar when using stacked estimation, and the precision is greatly improved when accounting for dynamics in the dependent variable rejecting case (2) above.

## 2.5 Breakthrough Candidate Effect Of Majority-Black Communities

Figure 2.2 presents event study estimates of equation 2.1 weighted by 1990 zip code population. The top panel plots  $\lambda_k$  and  $\tau_k$ , and the bottom panel plots the sum of the coefficients for each year, the absolute effect in Black communities.<sup>20</sup> The maroon lines depict the absolute effect which can be interpreted as the effect in non-Black communities whereas the blue lines depict the relative effect in majority-Black communities. The dashed lines are 90 percent confidence intervals clustered at the breakthrough election level. Employment in non-Black communities exhibits no pre-trends and the point estimates are nearly zero. In the post-period, there is a small decline reaching approximately 2 percent after four years. Black communities gain relatively to non-Black communities. In relative terms, employment increases nearly 8 percent relative to non-Black ones after four years, and given the small decline in non-Black communities, this translates to an approximately 6 percent absolute gain at the end of a presumed four-year term. In terms of workers, this implies more than 1,000 additional jobs in majority-Black communities at the end of the first term; however, the absolute gains diminish after the first term, but the absolute effect remains positive. The relative gains increase over the sample window which is partially explained by the continued decline in non-Black communities.

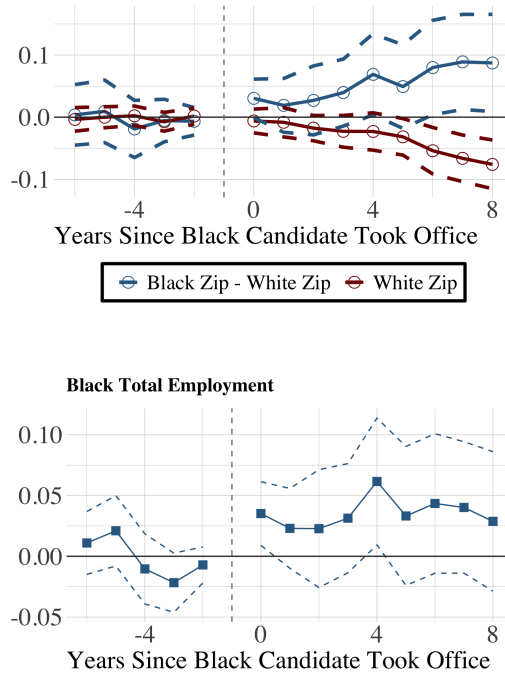


Figure 2.2: Stacked Regression Effect Of Black Mayors On Local Employment

The dependent variable is the natural log of employment per zip code resident. Coefficients are stacked weighted least-squares estimates of equation 2.1 weighted by 1990 zip code population. The top panel displays estimates of  $\lambda$ , the total effect in non-Black zip codes in maroon and  $\tau$ , the relative effect between Black and non-Black zip codes in blue. The bottom panel is the linear combination of  $\lambda$  and  $\tau$  which shows the total effect in majority-Black zip codes. Dashed lines are 90-percent confidence intervals clustered at the breakthrough level. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election.

<sup>20</sup>Alternatively, one can estimate equation 2.1 with only the Black zip code interaction and find equivalent results.

Turning to Figure 2.3 for establishments, non-Black communities exhibit very similar trends to the employment figures. Non-Black communities exhibit no differential trends prior to election and then experience a decline in total establishments. Establishments decrease slightly more than 2 percent relative to the reference period after four years. In contrast, majority-Black communities observe large and significant gains in establishments. Unlike in employment which exhibits an immediate increase, establishments exhibit no growth in the first period and then sharply jumps in subsequent years. This is consistent with businesses requiring time to acquire proper permitting or approval for operations which would operate on a lag from the initial election. At the end of the first term, establishments in Black communities increase nearly 4 percent. Taken together, the evidence coincides with a loss of approximately 20 business in non-Black communities and a gain of roughly 35 businesses in Black communities at the end of the first term. Interestingly, the dynamics for the absolute effect in Black communities indicate that the effects diminish over the eight year window and return to near pre-election levels. A natural question is, then, whether the observed pattern is from competition in Black communities driving the establishment number back to near baseline or businesses in Black communities disproportionately failing.

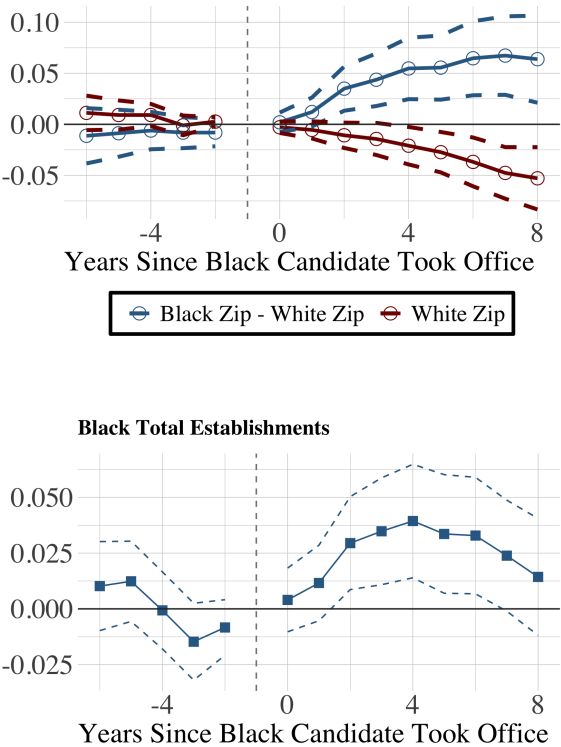


Figure 2.3: Stacked Regression Effect Of Black Mayors On Local Establishments

The dependent variable is the natural log of establishments per zip code resident. Coefficients are stacked weighted least-squares estimates of equation 2.1 weighted by 1990 zip code population. The top panel displays estimates of  $\lambda$ , the total effect in White zip codes in maroon and  $\tau$ , the relative effect between Black and White zip codes, in blue. The bottom panel is the linear combination of  $\lambda$  and  $\tau$  which shows the total effect in majority-Black zip codes. Dashed lines are 90 percent confidence intervals clustered at the breakthrough election level. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election.

A suggestive way to check this is to review how payroll evolves in conjunction with the establishment figures. If we observe a similar rising and falling pattern in the payroll figures, we might conclude that operating businesses in Black communities leads to disproportionately high failure; however, if payroll remains high on the other hand, we might conclude that competitive forces are driving business failure while a few businesses remain. Figure 2.4 plots the event study results for payroll. Similar to the previous results, payroll per resident experiences small declines in non-Black communities over the first term which continues into the subsequent four years. In Black communities gains grow over six years and reach more than 10 percent relative gains at the end of the window. When we observe the absolute gains, payroll in Black communities grow nearly 10 percent over the first term and those gains remain over the subsequent four years suggesting that competition is the more likely candidate for the establishment results than disproportionate business failure.

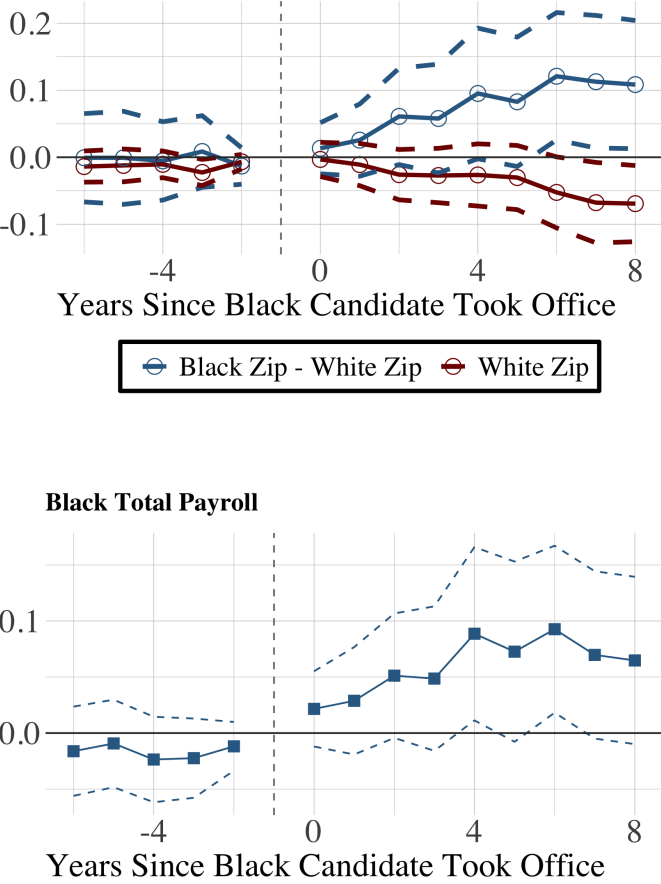


Figure 2.4: Stacked Regression Effect Of Black Mayors On Annual Payroll

The dependent variable is the natural log of annual payroll per zip code resident. Coefficients are stacked weighted least-squares estimates of equation 2.1 weighted by 1990 zip code population. The top panel displays estimates of  $\lambda$ , the total effect in White zip codes, in gray, and  $\tau$ , the relative effect between Black and White zip codes, in black. The bottom panel is the linear combination of  $\lambda$  and  $\tau$  which shows the total effect in majority-Black zip codes. Dashed lines are 90 percent confidence intervals clustered at the breakthrough election level. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election.



Table 2.2 estimates equation 2.2 summarizing the findings from the event studies into one post coefficient in columns (1)-(9). Columns (1), (4), and (7) show the citywide effect for the natural log of employment, establishments, and payroll per zip code resident. Cities experience a 2.2 percent reduction in employment per resident which evaluates to a loss of 4200 workers when evaluated at the 1990 city population and using the zip code employment-to-population average. Establishments experience a similar 2 percent decline after a breakthrough election which translates to a loss of 175 businesses. Payroll declines by less than 1 percent but confidence intervals imply a range of positive and negative outcomes. The losses in both cases are borne by non-majority Black communities which is evinced in columns (2), (3), (5), (6), (8), and (9) which demonstrates why it is important to examine place-specific, intra-city impacts as well as the aggregate results. The second column for each outcome estimates the standard DD without the inclusion of the pre-breakthrough trend, and the third column for each outcome estimates equation 2.2 which includes the pre-breakthrough trend interacted with the Black zip code indicator. Employment in non-Black communities decreases approximately 3.3 percent in the wake of a breakthrough election, and majority Black communities gain 5.8 percent relative to this though I cannot rule out a zero effect. I find the effects are similar after the inclusion of pre-election trends and the trends are near zero lending credence to the design. The point estimates for the relative and absolute effect in Black communities is slightly larger with the inclusion of the pre-election effect where I estimate a statistically significant 6.3 percent relative and 2.9 percent absolute gain though the absolute gain this is not statistically significant. This is consistent with the findings from the lower plot in Figure 2.2 where all the point estimates are positive but not different from zero. I cannot rule out the declines in non-Black communities being driven by reallocation between jurisdictions within the same economic region or an absolute reduction in economic activity driven by intra-city reallocation.<sup>21</sup> This would be the case if a segment of White stakeholders, in expectation of a redistribution of political resources, react to a Black election by relocating operations into adjacent suburbs.

I repeat the analysis for establishments, and find strong growth in establishments in Black communities, consistent with a narrative where Black mayors implement policies supporting disadvantaged business ownership and the mayor and department heads exercise discretion over the disbursement of municipal purchases. Beginning in column (5), non-Black communities observe a 2.9 percent decline, and majority-Black communities observe a 5.1 percent statistically significant relative increase in establishments relative to non-Black communities. Taken together, the total effect in Black communities is a non-statistically significant 2.2 percent increase in establishments. Adding the pre-breakthrough trend in column (6), the trends are not significant and near zero. The effect in non-Black communities decreases in magnitude some, but the effect is a statistically significant decline of 2.1 percent implying a reduction of 17 businesses, and the relative effect in Black communities is 4.5 percent implying a gain of 36 businesses relative to non-Black communities. Over the pre-treatment period Black communities have on average 100 fewer businesses than non-Black communities implying that the gap in businesses between Black and non-Black communities is reduced 36 percent. The absolute effect on Black communities is a statistically significant 2.6 percent. This implies an increase of about 35 businesses in Black communities over the 8-year period after a breakthrough candidate.

Place-specific estimates for payroll begin in columns (8) and (9). Across both columns, payroll decreases in non-Black communities. The 3.1 percent reduction in column (9) implies a loss of total payroll of approximately \$24 million; this loss is partially offset by the gains experienced in majority-Black communities which gain 7.9 percent relative non-Black communities and experience an absolute gain of 4.8 percent, but the absolute gain is not statistically distinguishable from zero. The 4.8 percent gain translates to a payroll increase of approximately \$37 million dollars in 2010 dollars.

We can also take the difference across corresponding columns to understand firms' employment behaviors; that is  $\log(emp\ per\ capita) - \log(est\ per\ capita)$  gives an estimate for firm hiring behavior within communities. If employment gains exceed establishment gains, this is suggestive of firms' positive future outlook for future conditions, while the reverse is true if employment losses exceed business closures. This gives another metric to gauge economic

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<sup>21</sup>Work ongoing.

improvement within a city across communities. Using columns (3) and (6), non-Black communities experience non-statistically-significant reduction in employees per establishment with disemployment estimates 1.3 percent greater than the loss in establishments. Turning to Black communities and remaining in columns (3) and (6), the absolute gains in employment exceed the gains from establishments by .4 percent though these within firm gains are indistinguishable from zero. Another transformation we can make is to take the difference between columns (9) and (3) which describes the change in wages between communities. In non-Black communities, there is no change in the average wage, while in majority-Black communities we observe a non-statistically significant increase of 1.9 percent. This could suggest an increase of demand for labor in Black communities or a decrease in the supply of labor. Given that each community is not an independent labor market, and the city-level results suggest wages go up ((7)-(9)) while employment decreases, the evidence taken together points to a decrease in the supply of labor.

### 2.5.1 Robustness

Tables 2.3 shows effects using alternative specifications and partitioning the data by institutional and electoral arrangement. Column (1) reprints the preferred specification for each outcome of Table 2.2. Column (2) partitions the data excluding all events where the breakthrough mayor is not the head of the executive branch, *i.e.* I exclude all council–manager and commissioner forms of government and retain all mayor–council governments. Within event I do the same, so the comparison is between only mayor–council governments. If there is direct transmission from executive policy to outcomes, one would expect it to be strongest in mayor–council governments, because the mayor serves as chief executive and has more policy tools at their discretion than a mayor whose powers are no different than a council person in a council–manager government. Column (2) provides evidence for this intuition and shows that relative and absolute point estimates in Black communities are larger for establishments and employment though not statistically different from the base results and similar in the payroll figures. The point estimates for non-Black communities across all outcomes are similar to the base specification. We might expect the strongest effects from the mayor serving as chief executive to be in the establishments number as the municipal government has direct say on operations within jurisdiction, and this is conveyed in the results especially in the absolute effect in Black communities that experience a statistically precise 3 percent gain on average after a breakthrough election.

Column (3) partitions the data retaining only non-partisan elections, *i.e.* removing those elections definitively between candidates of differing parties. While non-partisan elections does not guarantee the opposing candidates are from different parties, it is useful in understanding if the effect is possibly one of partisan differences or two candidates who differ by racial identity with otherwise similar partisan identities. The intraparty effects are similar to my base results. For establishments, the relative point estimates are unchanged, and the non-Black community estimates is one percent higher. Across employment and payroll, the relative results are not statistically significant; however, the magnitudes are larger implying large but imprecise effects. The non-Black community results are statistically significant and larger in magnitude suggesting larger losses in these communities when Black candidates first come to power in these cities. Non-partisan elections are often rules handed down by the state, and these electoral rules are more common in younger cities and Midwestern and Western states. The absolute Black community effects are not statistically significant, but remain positive and near the base results.

Column (4) retains the 12 events where the mayor was re-elected. Logan (2020) has demonstrated that after the election of Black policymakers, many of their policies were overturned, and the impact from their terms was reversed after reconstruction. Of course, this occurred more than a century ago, and the conditions for Black political efficacy in the 19th century do not reflect those in the 21st; however, it requires little strain to observe that economic gaps do persist and perhaps equal strain to expect effects should endure in areas where the Black candidate was in office over the entire window. The results from column (4) are consistent with this intuition. Notably, the effects in non-Black communities are not statistically different from zero across all outcomes, and the absolute effect in Black communities are larger in all three categories and statistically significant for establishments and payroll suggesting policymakers are able to mitigate the negative impact in other communities if given sufficient time.

Column (5) addresses the Vogl (2014) finding that cities in the South that elect a Black candidate are different than those in other regions. Vogl posits a group-specific mobilization technology whereby Black candidates in the South are able to galvanize large blocs of historically disaffected Black voters at the time of an interracial election. This may imply that trends between Black communities in the South versus those in the non-South are different, and Black communities in the non-South serve as poor controls. I specifically address this by including a South $\times$ year fixed effect that allows for differential effects by census region; specifically cities in the South are allowed to have a different effect from other regions. Thus, the comparison is now between cities in the South (non-South) that elect a Black candidate in a given year versus the set of control cities in the South (non-South) that do not elect a Black candidate in the same year. Column (5) shows that the inclusion of these does not materially change the conclusions.

Column (6) presents the strictest test restricting the comparison group to cities within the same state with the inclusion of state-by-year fixed effects. Here, identification comes from cities in the same state, one which elects a Black mayor over the event window and the others that do not. This is a restrictive model as this excludes breakthrough cities where there is no control city within the state; however, this estimation is useful as state-level policies are the same, business cycle effects are similar, and municipal organization is likely to be similar within the same state. The relative effects are similar to my base specification across all outcomes, and the magnitudes in non-Black communities are larger across categories. The implication from these results is that Black communities are not harmed after a breakthrough election; however, it is nonetheless encouraging that the pattern remains even in this specification which often means a one-to-one comparison of cities within the same state.

Finally in column (7), I replace the discrete indicator for a majority-Black zip code in 1990 for the Black population share in 1990. I include linear city time trends to show that my results and inference are unchanged by this addition. Both the absolute and relative effects are unchanged by this addition. Altogether, the results across all panels provides strong evidence that Black mayors provide economic benefits to Black communities.

	Log Emp Per Capita			Log Est Per Capita			Log Payroll Per Capita		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black Mayor	-0.022* (0.012)	-0.033* (0.018)	-0.034** (0.016)	-0.020* (0.010)	-0.029** (0.013)	-0.021* (0.011)	-0.009 (0.020)	-0.024 (0.026)	-0.031 (0.026)
Black Mayor $\times$ BZ		0.058 (0.049)	0.063* (0.036)		0.051** (0.023)	0.046*** (0.015)		0.077 (0.065)	0.079 (0.049)
Pre-election Trend			0.0004 (0.002)			-0.002 (0.002)			0.002 (0.003)
Pre-election Trend $\times$ BZ			-0.002 (0.006)			0.002 (0.003)			-0.001 (0.009)
$\tau_1 + \tau_2$		0.025 0.036	0.029 0.024		0.022 0.015	0.025** 0.009		0.053 0.049	0.048 0.032
Observations	111,300	111,300	111,300	111,300	111,300	111,300	111,300	111,300	111,300

*Notes:* The dependent variable is the natural log of establishments per zip code resident in columns 1-3, the natural log of establishments per zip code resident in columns 4-6, and the natural log of annual payroll per zip code resident in column 7-9. Coefficients are stacked weighted least-squares estimates of 2.2 weighted by 1990 zip code population. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of control cities whose Black populations are above 10 percent that either experience an interracial election over the sample period or elect a Black candidate more than 8 years afterward. Each breakthrough city forms a panel with the set of control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election.  $\tau_1 + \tau_2$  represents the total effect of Black mayors on majority-Black communities. Standard errors for those estimates are printed below. Standard errors are clustered at the breakthrough election level.

Table 2.2: Stacked Event Regression Of Breakthrough Mayor's Effect On Economic Activity

	Base (1)	Mayor–Council (2)	Intraparty (3)	Re-elected (4)	South × Year FEs (5)	State × Year FEs (6)	1990 Black Zip Share (7)
<i>Panel A: Establishments</i>							
BM × BZ	0.046*** (0.015)	0.055** (0.020)	0.047* (0.021)	0.045* (0.021)	0.038** (0.016)	0.036* (0.018)	0.059 (0.037)
BM	−0.021* (0.011)	−0.024 (0.013)	−0.032** (0.011)	−0.013 (0.014)	−0.025** (0.009)	−0.038*** (0.012)	−0.029* (0.016)
<i>Panel B: Employment</i>							
BM × BZ	0.063* (0.036)	0.067 (0.052)	0.078 (0.055)	0.067 (0.048)	0.069* (0.036)	0.063 (0.039)	0.073 (0.075)
BM	−0.034** (0.016)	−0.035 (0.024)	−0.049** (0.019)	−0.028 (0.022)	−0.044** (0.016)	−0.054** (0.019)	−0.043 (0.026)
<i>Panel C: Payroll</i>							
BM × BZ	0.079 (0.049)	0.072 (0.074)	0.098 (0.068)	0.086 (0.063)	0.093* (0.048)	0.077 (0.051)	0.126 (0.111)
BM	−0.031 (0.026)	−0.025 (0.041)	−0.054 (0.033)	−0.015 (0.035)	−0.047* (0.024)	−0.062** (0.023)	−0.051 (0.039)
$\tau_1 + \tau_2^{Est}$	0.025** (0.009)	0.03*** (0.011)	0.015 (0.012)	0.032*** (0.012)	0.013 (0.017)	−0.002 (0.021)	0.031 (0.024)
$\tau_1 + \tau_2^{Emp}$	0.029 (0.024)	0.033 (0.032)	0.029 (0.037)	0.039 (0.03)	0.025 (0.025)	0.009 (0.039)	0.031 (0.052)
$\tau_1 + \tau_2^{Pay}$	0.048 (0.032)	0.047 (0.042)	0.045 (0.04)	0.072* (0.037)	0.046 (0.049)	0.015 (0.049)	0.075 (0.078)
Observations	111,300	40,851	56,940	73,830	111,300	111,300	111,300

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

*Notes:* Models are weighted least-squares estimates of equation 2.2 where the dependent variables are the natural log of per zip code resident employment in panel A, the natural log of establishments per zip code resident in column B, and the natural log of annual payroll per zip code resident in panel C. Column (1) replicates my base specification from table 2.2. Column (2) retains all events for breakthrough cities with mayor–council forms of government. Column (3) retains all non-partisan elections. Column (4) retains all events where the breakthrough candidate was re-elected. Column (5) includes South-by-year fixed effects, column (6) includes State-by-year fixed effects, and column (7) includes a linear city time trend. Sample includes 18 cities (9 for column (2), 10 in column (3), and 12 in column (4)) that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of control cities whose Black populations are above 10 percent in 1990 that either experienced an interracial election over the sample period or elected a Black candidate more than 8 years later. Each breakthrough city forms a panel with the set of control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election.  $\tau_1 + \tau_2$  represents the total effect of Black mayors on majority-Black communities. Standard errors for those estimates are printed below. Standard errors are clustered at the city-by-event level.

Table 2.3: Robustness Of Establishment Growth To Alternative Specifications

## 2.5.2 Do Interracial Elections Independently Improve Black Community Outcomes?

An unaddressed concern thus far is that independent of electing a Black candidate, election winners must account for the political preferences of Black residents as a significant voting bloc. Therefore, the initial results may capture some combination of the Black mayor effect and the effect stemming from increasing Black political power. To rule this out I construct a dataset using the interracial elections from 16 of the 25 control cities where the Black candidate was not elected and 22 cities with 1990 populations greater than 100,000, a Black population share of at least 10 percent,

and did not observe an interracial election over the sample period. I set a false treatment in the year a Black candidate would have taken office. For sample size considerations, I require that I observe at least four pre-period years and six post period years around taking office. In cities where multiple interracial elections are observed, I make two considerations. First, I opt for the election that satisfies the four pre-period years and six post-period years. Second, if both elections satisfy the first criteria, I choose the election where the vote share for the Black candidate is the highest.<sup>22</sup> I estimate equation 2.2 and report the results in Table 2.4.

The first column reports the employment results. The numbers are imprecise and indicate that if anything Black communities in interracial election cities perform worse compared to control cities though part of the effect is being captured by the imprecise pre-election effect. The second column shows that relative establishment growth per zip-code resident is no different between treated and control, and the pre-election point estimates are small. The third columns displays results for payroll growth per zip-code resident and the results are imprecise suggesting no significant differences between communities that observed an interracial election and those that did not. Of the most interest for this study are the relative effects in Black zip codes. In each column, we observe the estimates are negative or small and always imprecise. This contrasts the results from Table 2.2 which show consistently large, positive, and generally significant results for each outcome. This evidence provides additional support for the hypothesis that Black mayors have a causal impact in Black communities.<sup>23</sup>

	Log Emp Per Capita	Log Est Per Capita	Log Payroll Per Capita
Black Mayor	0.017 (0.016)	0.010 (0.015)	0.008 (0.018)
Black Mayor x BZ	-0.040 (0.029)	0.006 (0.013)	0.022 (0.030)
<i>Pre-Election Effect</i>			
Pre-Election Trend	-0.004 (0.005)	0.001 (0.003)	-0.001 (0.005)
Pre-Election Trend x BZ	0.011 (0.009)	-0.000 (0.008)	-0.012 (0.010)
Total Effect	-0.023 ( 0.022 )	0.016 ( 0.014 )	0.03 ( 0.022 )
N	49293	49293	49293
*** p < 0.01; ** p < 0.05; * p < 0.1.			

Model is weighted least squares estimates of equation 2 where the dependent variables are listed above each column. The sample is composed of cities that experienced an interracial election between 2e+03 and 2.01e+03 and 22 cities whose 1.99e+03 populations were greater than 100,0 and had a Black population share of at least 10 percent, but experienced no interracial election. A false treatment is set in the year a Black candidate would have take office, and each false breakthrough is its own panel appended with the 22 control cities. Standard errors are clusterd at the breakthrough election level.

Table 2.4: Do Black Communities Improve From Interracial Elections Alone: Falsification Exercise

<sup>22</sup>This constitutes a couple of cases, and my results are not sensitive to choosing the other dates. Further, my conclusions remain unchanged if I estimate an unbalanced panel using all cities.

<sup>23</sup>To conserve space, I report event study figures in the appendix.

## 2.6 Channels

### 2.6.1 By Industry

Minority businesses encounter numerous impediments to forming and growing businesses such as lack of financial, social, and human capital in addition to lack of access to markets outside of their immediate geographic location. Minority firms also face barriers in participating in the government contracting process due to capital limitations and limited information on contract competitions to offset fewer growth opportunities presented in private markets (Enchautegui et al. 1997). Mayors wield considerable influence over the distribution of city contracts, so I turn to examine changes in local industry composition at the zip-code level after the election of a breakthrough candidate.

Black mayors have the ability to encourage Black business formation in at least two ways. First, Black mayors can reallocate the distribution of city funds through the use of disadvantaged business (DB) programs. In earlier periods, these programs would have been minority set-aside programs, but these were ruled unconstitutional under the equal protection clause in *Croson v. Richmond* (1989). DB programs most often impact construction firms. To confirm this I collect available disadvantaged business approved vendor list, and find that construction firms, administrative services firms, and professional services firms most frequently appear on vendor lists. This finding coincides with previous work on set-asides by Chatterji, Chay, and Fairlie (2014) who include these industries in their definition of “affected” firms.<sup>24</sup> <sup>25</sup> In addition to DB programs, mayors and department heads exercise significant discretion in routine purchases which are defined as purchases not exceeding a city-specific threshold and are not subject to council approval or additional oversight in most cases.

A perhaps complementary narrative is that mayors have influence over the location of potential firms and court potential businesses to locate in areas of their voters. If this is true we may expect gains to come industries with larger average sizes and that represent “good” jobs such as manufacturing. If these firms are larger I may be less likely to detect absolute effects because it would constitute a shift in business location from a non-Black community in the counterfactual to a Black community. I estimate equation 2.2 first by grouping industries by how they would likely be affected by city contracting similar to Chatterji, Chay, and Fairlie (2014).

I group the industries into four categories and also report results for construction firms separately. I include two “affected” categories. The first is “affected - broad” which mimics the industries listed as “affected” in (Chatterji, Chay, and Fairlie 2014); however, the set of firms affected in the mid- to late-eighties may not be the same set of firms potentially affected presently. I narrow the set of potential firms affected only including those firms most frequently listed on government vendor lists (construction, professional services, and administrative services), and I label these as “affected - narrow.” “Unaffected” firms are those not listed in the broadly affected category and include manufacturing, wholesale trade, accommodation and food services and firms in industries not frequently located in cities (*e.g.* agricultural services and mining).<sup>26</sup> This set of firms may be unaffected by the city-contracting process, but could be affected in other ways such as political influence or spillovers from increased location-specific wages or employment in the case of the entry decision for firms in accommodation and food services. I then construct a category of industries that may reflect changes in local conditions and label those as “foot-traffic” industries.<sup>27</sup> Businesses in this category may enter or endure longer than they otherwise would have if the location-specific return on investment improves due to possibly unobserved localized policies implemented after breakthrough elections. I

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<sup>24</sup>Chatterji, Chay, and Fairlie (2014) list these “affected” firms as construction, professional services, transportation-communications-utilities, business services, repair services, and retail trade as the most affected industries.

<sup>25</sup>A critique of DB and set-aside programs has been that they are ineffective because municipal contracts are granted to firms located outside of the city. The critique appears to be true as most approved vendors are located outside of the city; however, the static list does not tell me if the mix of approved vendors changes or the amount granted to each vendor which I do not observe.

<sup>26</sup>Manufacturing, wholesale trade, accommodation & food services, agriculture, mining, information, finance and insurance, real estate and rental leasing, management of companies, educational services, health care and social assistance, arts; entertainment; and recreation

<sup>27</sup>Retail trade, accommodation & food services, other services, arts; entertainment; and recreation

report point estimates in Figure 2.5 with 90- and 95-percent confidence intervals clustered at the breakthrough election level. The blue bars show the total effect in Black communities, the red bars show the total effect for non-Black communities, and the gray bars show the relative effect for Black communities.

Figure 2.5 provides some evidence for the city contracting narrative. First looking at the set of bars for construction firms, we observe imprecise absolute and total gains in Black communities and an imprecise 4 percent loss in non-Black communities; This reallocation between non-Black to Black communities does constitute a statistically imprecise relative gain of 8.5 percent. We observe similar patterns between non-Black and Black communities when examining other industries affected by city contracting. Using the broadest definition of affected industries, the results indicate positive gains in Black communities and perhaps small losses in non-Black communities. The relative gain in Black communities is nearly 5 percent, and while not statistically different from zero, I can rule out all but the smallest negative effects. The same is true of the absolute Black estimates. When using the narrow definition of industries most affected by contracting, we observe statistically significant relative gains of approximately 6 percent. Non-Black communities experience slightly larger losses than in the broad context but I cannot rule out small-sized gains.

Unaffected industries exhibit both absolute and relative gains in Black communities. The relative gain is similar in size to the effect for the broadly affected industries. Interestingly, the average loss in non-Black communities is of nearly equivalent magnitude to the absolute gain in Black communities. Looking at the foot-traffic industries which removes those industries that are infrequently found in cities and adds back in retail trade firms, the same pattern in the previous results emerges. There are positive relative and absolute effects in Black communities and small losses in non-Black communities. The point estimates, however, exhibit considerable overlap both with zero and the Black–non-Black absolute intervals which may reflect that the number of retail firms is not changing much between communities.

My results cannot confirm nor deny that existing firms are improving by hiring more employees or through increased survival because I cannot identify individual firms or that the potential set of firms that are new or attracted from other locations do not impose a negative environmental externality on the community. The industry-specific patterns reflect my base findings where non-Black communities exhibit no gains to small losses across all categories. The evidence taken together implies that Black communities improve relative to non-Black ones after the election of a Black candidate.



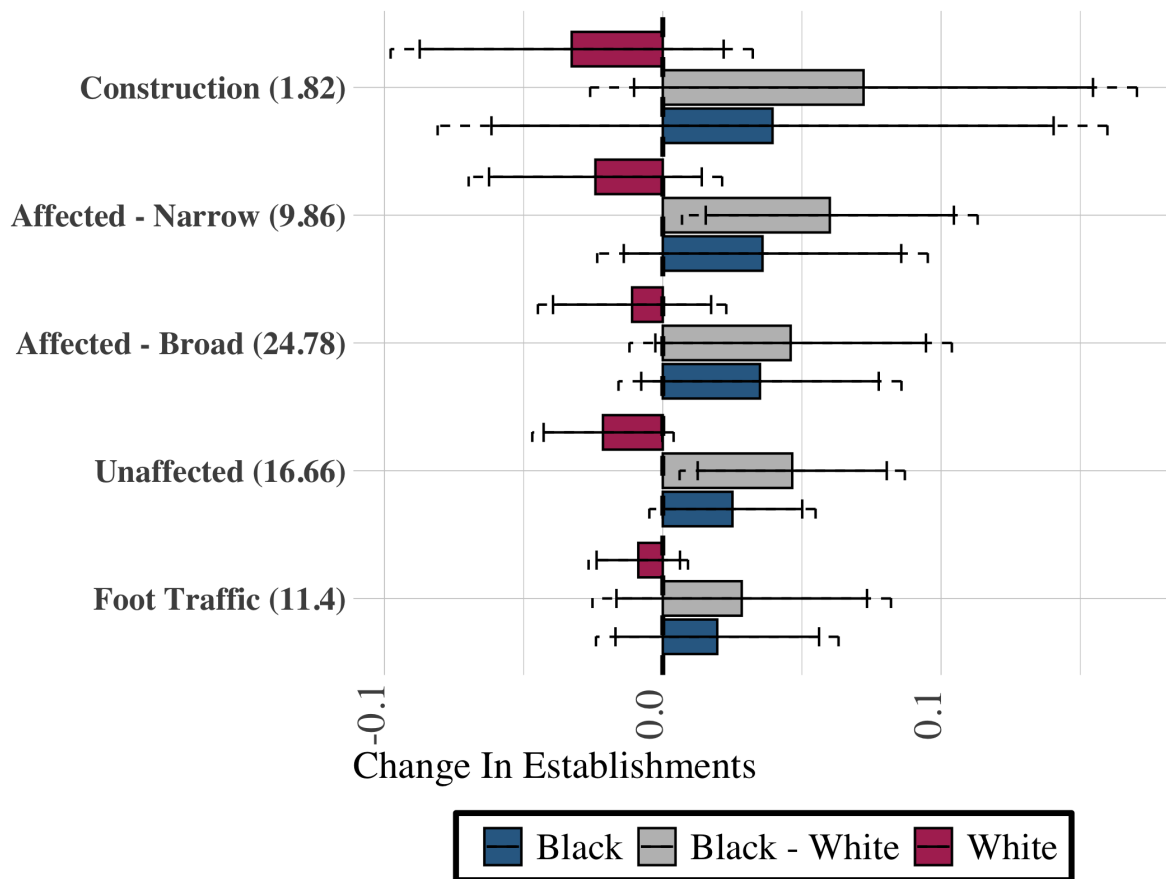


Figure 2.5: Industry-Specific Estimates of Breakthrough Mayor Effect

The dependent variable is the log count of firms per zip code resident by each industry according to broad NAICS categories. The industries are listed along the y-axis with the number of businesses per 10,000 residents printed in parentheses. The coefficients are stacked weighted least-squares estimates of equation 2.2 of  $\tau_1$  and  $\tau_2$  weighted by 1990 zip code population. The red bars represent  $\tau_1$ , the total effect in non-Black zip codes, the gray bars represent  $\tau_2$ , the relative effect in Black zip codes, and the blue bars represent  $\tau_1 + \tau_2$ , the total effect in Black zip codes. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. Standard errors are clustered at the breakthrough election level.

### 2.6.2 Do Small Businesses In Black Communities Benefit?

Because we observe an increase in establishments, I now analyze where these gains are distributed by establishment size as measured by the firm's number of employees and if there are place-specific gains. My previous results, namely Figure 2.3, cannot strongly speak to this dimension because smaller businesses constitute 68 percent of businesses within a zip code in the sample, so the previous results tend to reflect gains in the number of small firms. A particular plank of mayors' economic development policies is attracting large firms. Because attracting large firms provides larger employment gains, one might expect gains in Black communities to be pronounced with large employers. An alternative explanation extends the city contracting narrative whereby mayors boost small businesses operated by

Black, minority, and disadvantaged entrepreneurs who tend to work with smaller levels of capital and fewer employees. In some cases, municipal governments have changed their contracting policies to allow for many small firms to submit joint contracts to fulfill city services in order to increase municipal resources allocated to minority firms. Of course, these policies can be pursued and implemented in concert so gains for both are possible. I split the data into 3 different bins —small, medium, and large establishments. Small establishments are defined as establishments with fewer than 10 employees. Medium-sized establishments are establishments with greater than 9 employees and fewer than 50 employees. Large establishments are those with at least 50 employees. I estimate equation 2.2 which gives the effect of breakthrough mayors on establishment gains by the employment size within each bin.

Table 2.5 reports the results broken out by number of employees within an establishment. For small firms, there is a small, statistically non-significant negative effect in non-Black communities reflective of the earlier findings in Table 2.2. Black communities experience similar relative and absolute gains in small businesses. Small businesses in Black communities grow 3.9 percent relatively and 2.3 percent absolutely after the election of a breakthrough candidate. Evaluated at the average number of small businesses, this translates to 19 more businesses relative to non-Black communities and 11 more absolutely over the eight year window. Both of these estimates are significant at the 5-percent level both reflective of earlier estimates and supporting the role of the mayor encouraging small-business growth among minorities. When turning to other establishment-size classes, we observe noisier but similar relative gains in Black communities for medium-sized firms. The equivalent is true in non-Black communities as well. After a breakthrough election, there are an additional 5 large firms (9.7 percent gain) in Black communities relative to non-Black ones, and 4 businesses (7.9 percent gain) in absolute terms which is a greater absolute gain than medium-sized firms. Non-Black communities experience an imprecise 1.9 percent decline similar to the results across the other size classes. I cannot say with certainty whether the gains in large firms constitute the emergence of new firms or the transitioning of medium-sized businesses to large-sized businesses. The magnitude of the point estimates paired with mayors’ stated policy goals at the least indicates that some of these firms are likely entrants.

	< 10 Employees (1)	10 ≤ Employees < 50 (2)	≥ 50 Employees (3)
BM	-0.016 (0.011)	-0.013 (0.011)	-0.019 (0.017)
BM × BZ	0.039** (0.016)	0.032 (0.021)	0.097*** (0.029)
$\tau_1 + \tau_2$	0.023 ** 0.011	0.019 0.02	0.079 *** 0.025
Observations	109,665	109,599	108,821

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

*Notes:* Results represent the estimation on equation 2. The dependent variable is the natural log of establishments per zip code resident separated into three groups by the number of employees at each establishment: fewer than 10 employees, between 10 and 49 employees, and greater than 49 employees. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. The event window is six years before the election of a breakthrough candidate and the eight years following the election.  $\tau_1 + \tau_2$  represents the total effect Black mayors have on majority Black communities. Standard errors for those estimates are printed below. Standard errors are clustered at the breakthrough election level.

Table 2.5: The Relationship Between Breakthrough Black Mayors And Establishment Gains By Employment Size

### 2.6.3 Do Black Mayors Boost Entrepreneurship Among Black Americans? Evidence From The ACS

#### 2.6.3.1 Case Study: Black-Owned Businesses in Shreveport vs. Little Rock

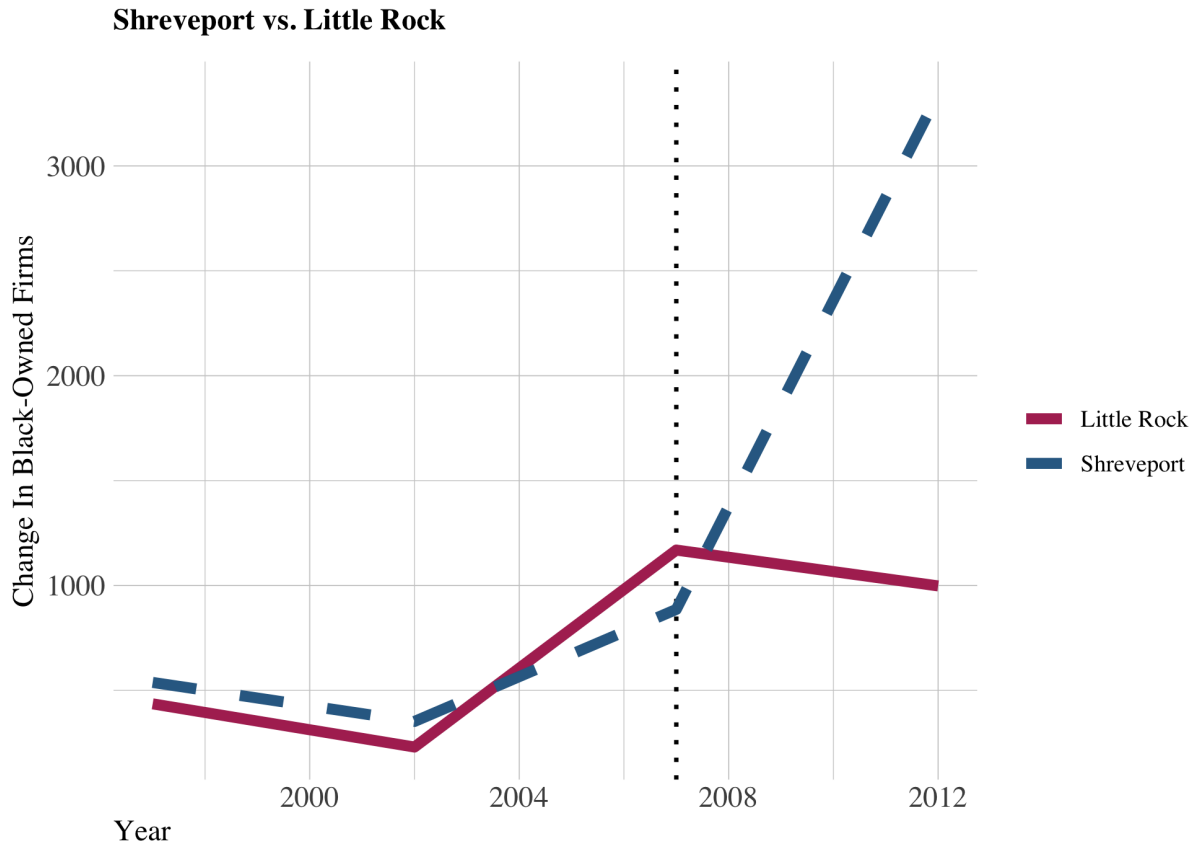


Figure 2.6: Changes In Black-Owned Firms - Shreveport vs. Little Rock

Sources: Survey of Business Owners 1997, 2002, 2007, and 2012

Because we observe establishment gains in Black communities and Black business owners are disproportionately more likely to serve their neighborhood (A. M. Perry and Romer 2020), we might expect gains in Black-owned businesses. The evidence presented thus far can neither confirm nor deny this channel. A motivating example for Black business gains is Shreveport, Louisiana which elected Cedric Glover as the city’s first Black mayor. Figure 2.6, using data from the Census’s Survey of Business Owners, provides a case study suggesting the election of a Black mayor increases Black business formation. The dashed line depicts the change in Black-owned firms in Shreveport, the solid line depicts Little Rock, Arkansas, and the vertical, dotted line shows the timing of the Glover election. Roughly 200 miles separate Shreveport and Little Rock, and 12 years separate the election of Black candidates.<sup>28</sup> Between 1997 and 2007, both cities had similar growth in Black-owned firms, but after the Glover election, Shreveport gained over 3000 more Black-owned firms by 2012. Growth in Black-owned firms slightly decreased over the same

<sup>28</sup>Frank Scott became the first directly elected mayor of Little Rock in 2019.

5-year period in Little Rock.<sup>29</sup>

Glover was the youngest person elected to the Shreveport city council and spent 10 years in the Louisiana House of Representatives before the mayoralty. His mayoral competitor, Jerry Jones,<sup>30</sup> spent eight years as a city attorney but had never been elected to any office. Jones outpointed Glover in the primary where turnout in predominantly Black precincts trailed predominantly White precincts by 10 to 15 percent.<sup>31</sup> Turnout for the runoff was 10 points higher than expectation attributable to a 15 percent surge by Black voters, reflecting Vogl’s findings.<sup>32</sup> Glover has been credited with building “the most diverse leadership team in the city’s history,” where his administration pursued policies of business growth and retention, support for minority businesses, property standards, and crime reduction.<sup>33</sup> Support for minority and disadvantaged businesses was an explicit departure from previous administrations which consistently failed to meet their 25 percent goal of city contracts to disadvantaged businesses.<sup>34</sup> The city of Shreveport also partnered with the Strategic Action Council of Northwest Louisiana to supply funding for the Minority Suppliers Institute for three years to provide assistance to minority-owned businesses.

In an ideal setting, I would observe Black-owned businesses every year and estimate the change in Black-owned businesses before and after a breakthrough election relative to non-breakthrough cities. However, this data do not exist. Even the Survey of Business owners only reports this every 5 years, is subject to non-reporting and censoring, and was discontinued in 2012. In consideration of this, I turn to the decennial censuses and the American Community Surveys (ACS) to estimate changes in Black self-employment.

### 2.6.3.2 Turning To The Census & ACS

To determine if gains accrue to Black residents, I use the 1990 and 2000 Censuses, and ACS from 2005 to 2019,<sup>35</sup> which report outcomes at the public-use microdata area (PUMA), to examine race-specific changes in the likelihood of entrepreneurship proxied by self employment. I restrict my analysis to individuals that report as being White or Black and are between the ages of 25 and 54 inclusive.<sup>36</sup> I match PUMAs<sup>37</sup> to cities and estimate

$$y_{iect} = \alpha_{ec} + \delta_{et}^r + \tau_{e1} \mathbf{1}[BM_{ect}] + \tau_{e2} \mathbf{1}[BM_{ect}] \times Black_{iect} + \Gamma \mathbf{X}_{iect} + \varepsilon_{iect}, \quad (2.3)$$

where  $y_{iect}$  is a measure of self employment for person  $i$  for event  $e$  in city  $c$  in year  $t$ . Estimation of 2.3 represents two departures from earlier estimation. First, my pre-treatment years and post-treatment years are limited because the ACS did not begin reporting location-specific outcomes until 2005, so I extend the pre-treatment window to 15 years before a breakthrough election to make sure each observation has at least two pre-treatment observations. I extend the post-treatment window by two years so each breakthrough election has as many as 10 post-treatment observations. This adjustment adds observations to the earlier treated units, creates an unbalanced panel, and introduces weighting issues caused by differences in each breakthrough’s treatment proportion. However, in lieu of stacking the events together, I estimate separate treatments for each event and then aggregate those treatment effects to avoid this

<sup>29</sup>Black employer firms observe no growth the 15 years before the election of Glover. In fact, Shreveport lost 3 Black employer firms from 2002 to 2007 reducing its total to 183 firms. In the 5 years after Glover’s election, Black employer firms increased more than 20 percent. Receipts to Black firms grew nearly 40 percent, faster than business growth over the same period.

<sup>30</sup>not that one

<sup>31</sup>“Republican, Democrat head to runoff in Shreveport mayoral race.” Associated Press State & Local Wire, October 1, 2006 Sunday.

<sup>32</sup>Shreveport Times, Michelle Mahfoufi, November 8th, 2006

<sup>33</sup><https://www.shreveportla.gov/DocumentCenter/View/2542/Mayor-Cedric-B-Glover---BIO?bidId=>

<sup>34</sup>Set asides and quotas to minorities were ruled illegal by the Supreme Court, so the Fair Share program was a race-neutral, gender-blind program in name though the program disproportionately aided them.

<sup>35</sup>The ACS did not report location-specific attributes before 2005, the first year of its full implementation.

<sup>36</sup>My results are not sensitive to this restriction. Self employment sharply jumps at 65 from individuals working “bridge jobs” before ultimately retiring. While of scholarly interest, this particular dimension of self employment is beyond the scope of this work.

<sup>37</sup>1990 PUMAs follow boundaries of counties and census-designated places and do not cross state-lines. If these areas exceed 200,000 residents, they are split into PUMAs containing 100,000 residents. 2000 and 2010 PUMAs do not cross state lines and are also groupings of 100,000 residents.

unattractive feature. In equation 2.3, I denote the event-specific treatment effects on each coefficient. e.g.  $\tau_{e1}$  measures the baseline difference in self-employment rate for each event  $e$ .  $\mathbf{X}'_{iect}$  is a vector of individual-level controls including a quadratic in age, gender, income, education, and an interaction between age and education. The two parameters of interest are  $\tau_{e1}$  which represents the baseline difference in self-employment between treatment and control which I interpret as self-employment gains for White residents and  $\tau_{e2}$  which represents the event-specific relative gain in self employment for Black residents relative to White residents. Knowing the existing gap in self-employment is paramount to understanding the magnitude and economic significance of the relative effect, so I estimate the gap in two stages. First, I regress the self-employment indicator on a vector of controls  $\mathbf{X}_{iect}$  and a city indicator. I obtain the residuals from the first regression, and then regress the residuals on an indicator for being black interacted with a treatment indicator. I obtain the coefficients for the Black indicator to use later to gauge the city-specific changes in the B-W self-employment gap.

I report the results in Figure 2.7. <sup>38</sup> In the top-left corner of Figure 2.7 are the estimates for the average self-employment gap in the treated group and the aggregate results from estimation of equation 2.3 on an indicator for self employment. The Black– gap in self-employment in 2000 is 3.4 percentage points which is not much higher than the 3.3 percentage point gap in 2015 (Hipple and Hammond 2016). This persistence, however, is unsurprising as Fairlie and Meyer (2000) have found the ratio to be nearly constant for a century. When looking at the aggregate self-employment results, the pattern is similar to the previous results. Black residents experience a relative gain of nearly one percentage point and White resident reductions in self-employment are of similar size. The pattern suggests crowding out whereby newly, self-employed Black residents replace the production of exiting self-employed White residents. However, the results in Figure 2.7 point to heterogeneity in the effect of Black candidates. Also of note is the direction of the results. There are greater gains in self-employment in areas where the self-employment gap is smaller, and many of the areas with greater gaps and negative relative effects are located in the Deep South suggesting the mayor may be of limited assistance in places with longer histories and established patterns of discriminatory behavior.

The lower panel of Figure 2.7 takes the relative effect and divides it by the 2000 B-W self employment gap and plots the resulting number to describe the city-specific reductions in the self-employment gap. Two-thirds of treatment cities experience reductions in the gap, and we can clearly see the geographic pattern in the cities where they do not. The average gap reduction can be found by dividing the relative point estimate by the gap and doing this yields an average reduction of 27 percent in the decade after a breakthrough election. This estimate suggests that, on average, the mayor can play a significant role in reducing racial inequality in the areas they have authority.

### 2.6.3.3 Employment & Income Effects

Table 2.6 shows employment and income effects after a breakthrough election. The results indicate that mayors may be limited in their ability to improve employment outcomes for Black residents both in the private and public sector. The first column shows changes to the employment rate; that is, the outcome is an indicator equal to one if the resident indicated they were employed at the time of the survey. There’s an imprecise decline of 1.1 percentage points for White residents and no relative difference for Black Americans which is consistent with a citywide decline in employment described earlier. While the point estimate is not statistically significant the implied change in employment ranges from a 3.6 percent reduction to a 1 percent increase, so I can rule out all but the small positive changes in the labor force.<sup>39</sup> The second column reports results for the labor force participation rate which is an indicator equal to one if the resident responded as being in the labor force. The results in column 2 are similar to the first column where

<sup>38</sup>I report the results for those who identify as self-employed with an incorporated business in Appendix Figure 2.15. This is a useful check to the initial results as policies to address gaps in entrepreneurship would affect business owners more likely to incorporate or have incorporated businesses. In addition, this removes the concern that breakthrough elections are causing disemployment at lower income levels which triggers a transition from employed wage worker to self employed.

<sup>39</sup>I do this by taking the 95 percent confidence intervals and dividing it by the sample average of those employed (85.07 percent) among the prime age labor force.

there is a 1.2 percentage point decline in labor force participation and no relative differences between races. Taking the absolute estimates ( $\bar{\tau}_1$ ) in columns 1 and 2 and assuming that none of those who are unemployed also leave the labor force, the implied change in employment is 2.7 percent which is well within the confidence intervals from the employment effect in Table 2.2. While the assumption of no labor market to out-of-labor-market transitions is strong, if I were to make weaker assumptions about those transitions this would move the average closer to the 2.2 percent reduction found in my location-specific analysis.

Columns 3 and 4 show results for government employment which I have not been able to discuss with my data hitherto. Works in earlier periods have found employment in municipal government to be one of the main pathways Black residents are rewarded whether this be through changes in descriptive representation or electoral representation caused by civil rights legislation. Column 3 shows the changes for employment in local government and find no changes after breakthrough elections in this period. Earlier work has found implied effects as large as a 55 percent change in Black municipal employment (Nye, Rainer, and Stratmann 2014). The average implied effect in this context is zero, and even the relative effect is 3.6 percent suggesting limited ability to improve Black outcomes through municipal employment.<sup>40</sup> Column 4 shows changes to employment in state and federal government. The results suggest, if anything, White residents are more likely to seek employment in the state and federal government, and Black residents employment in other non-local governments does not change consistent with the fact that mayors cannot effect employment decisions at other levels of government.

Column 5 shows a noisy 2.3 percent change in wages for White residents and no relative difference for Black residents. The findings are consistent with the results in Table 2.2 which suggested a positive, but noisy, change in wages; because zip codes are a part of the larger city labor market, we should not expect differences in wages by race. Because B-W wage gaps exist in private markets (Wilson and Rodgers III 2016), we might expect that the B-W wage premia would be reduced if Black employees transitioned from private to public employment, but Black residents experience no changes in public employment which reinforces the null effect.

## 2.7 Conclusion

Thirty-five years after the VRA and end of the Great Migration, Black Americans in many large cities with substantial Black populations had never been represented by someone that resembled them at the executive level. I show cities that elect a Black candidate for mayor for the first time experience large and positive changes in economic development in majority-Black communities. Businesses in majority-Black communities grow in the 8 years following the election of a breakthrough candidate both absolute and relative to non-Black communities. Similar relative gains are observed in areas where the mayor exerts less direct influence such as employment and payroll. The effects are stronger in cities where the mayor has more power either by institutional arrangement or having more time to effect policy by being re-elected, and cannot be explained just by the presence of an interracial election.

I examine industry-specific outcomes to investigate the presence of a direct channel of municipal policy whereby Black mayors are channeling resources to Black communities and business owners through city contracting. When examining changes in the number of firms grouped by their potential affectedness to government contracting, I observe gains in firms most frequently affected by government participation. Interestingly, gains are similarly large in firms that are not affected by participating in the government contracting process suggesting a second channel whereby mayoral policy is improving the local business environment in Black communities. This notion is confirmed when observing that the strongest business gains are in the number of businesses with more than 50 employees. Because my initial exploration is place-specific, I turn to individual data from the ACS and decennial census to examine if Black residents become self employed in the wake of a breakthrough election. I find closure in the B-W self-employment gap is smaller in areas where the Black-White gap in self employment is already high suggesting mayors in these areas

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<sup>40</sup>The sample average of Black residents employed by the local government is 8.3 percent, whereas the sample average of Black residents employed by the local government was 5.5 percent in Nye, Rainer, and Stratmann (2014).

may be constrained by local conditions. Compared to previous work which observed gains in municipal employment as a central driver to improved Black labor condition, I find zero gains in municipal employment consistent with the observation that Black residents across all cities are already overrepresented in public jobs.

There are two implications from these findings. The first and direct implication is that representation matters consistent with findings from previous work on Black and minority candidates. Given the renewed focus on issues of representation and identity especially as it pertains to persistent economic and social disparities, further research into longer-term effects to determine if gains are reversed similar to previous work will be fruitful. Research on other minority groups or examining different areas of government with different administrative authority will help determine the areas where representational parity will help affect economic and social parity. While increased representation is an important topic, the second implication is that persistent economic differences between Black and White Americans are not a fact of life. This work shows that candidates credibly committed to policies reducing disparities can serve a role whereby the governments they helm eliminate structural barriers to participation and improve access to markets for minority business owners.

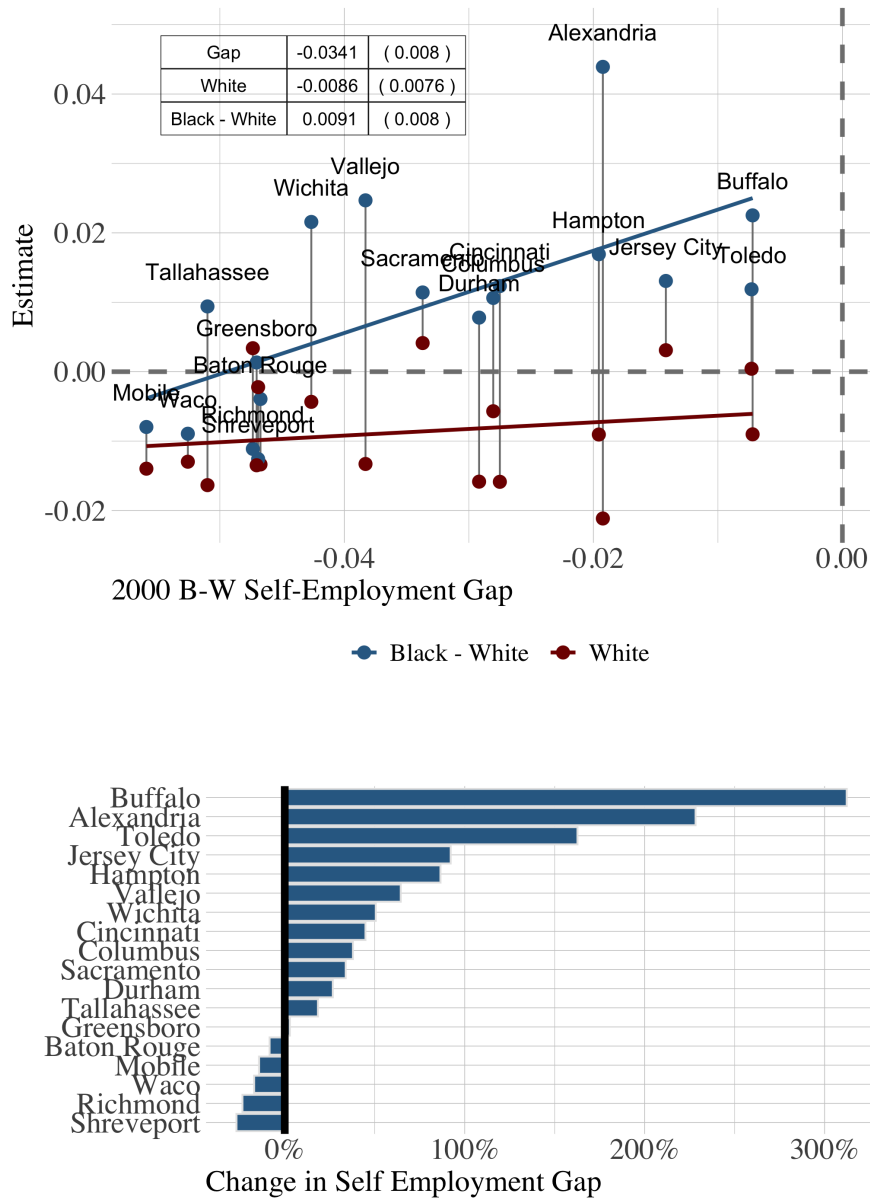


Figure 2.7: Breakthrough Mayors, Self-Employment Outcomes, & City-Specific Changes In The B-W Gap

The top panel displays results from the estimation of Equation 2.3 using Census and ACS person weights where the outcome is an indicator for being self-employed. The results from that regression are then plotted against the estimated city-specific Black-White gap in self employment in 2000. The red points show the breakthrough-specific point estimates of  $\tau_{e1}$ , the breakthrough effect on White self employment. Blue points show relative self-employment gains for Black residents relative to White estimates. The table at the top of the panel shows the average effect across events. *e.g.*  $Gap = \overline{gap} = \frac{1}{18} \sum_{e=1}^{18} gap_e$ . Other estimates are defined similarly. The bottom panel takes the blue points and divides it by the city-specific self-employment gap, then plots the reduction in the B-W self employment gap ordering them from largest to smallest.



	Employment Rate	Labor Force Participation	Employed - Local Government	Employed - State Government	Log Wage
BM × Black	0.002 (0.013)	0.004 (0.010)	0.003 (0.009)	-0.012 (0.011)	-0.002 (0.036)
BM	-0.011 (0.010)	-0.012 (0.008)	-0.003 (0.007)	0.009 (0.009)	0.023 (0.030)

*Notes:* Table contains manually averaged estimates of equation 2.3 on employment outcomes and wages. Data are from the 1990 and 2000 censuses and the ACS for the years after 2004. Events are constructed by taking each breakthrough election and retaining all cities that observed an interracial election or elected a Black candidate more than a decade later. Sample includes all residents in each city matched to PUMA that identify as only Black or White alone and are between the ages of 25 and 54 inclusive that report age, education, income, and gender. Column 1 is an indicator equal to one if resident was employed in the year of the survey. Column 2 is an indicator equal to one if resident was in the labor force in the year of the survey. Column 3 is equal to one if resident was employed by the local government in the year of the survey. Column 4 is an indicator equal to one if resident was employed in the public sector but not by the local government. Column 5 is the natural log of wage. Residents who reported a zero wage are excluded. Manually averaged, heteroskedastic-robust standard errors are reported below each estimate.

Table 2.6: Breakthrough Mayor Effect On Employment Outcomes And Wages - Manual Averaging

## 2.8 Appendix

### 2.8.1 Additional Tables & Figures

	All	Sample
<b>1990 City Characteristics</b>		
Population	337793 (646548)	251557 (146573)
Property Taxes (429.80)	424.79 (409.07)	457.76
Sales Taxes	133.79 (161.70)	93.21 (131.10)
Total Revenue	2317.90 (1507.57)	2446.61 (1456.02)
Debt Outstanding	2828.29 (2689.43)	3254.66 (3961.40)
Police Spending	219.54 (86.52)	226.01 (90.91)
Black Pop. Share	23.46 (17.99)	23.87 (11.89)
White Pop. Share	66.97 (16.69)	68.70 (11.20)
Median Housing Value	93574.44 (64511.65)	85821.74 (56020.93)
Median Rent	376.92 (114.51)	359.89 (94.69)
Mayor–Council	66.67 (47.32)	71.11 (45.84)
<b>Zip Code Characteristics</b>		
Population	21209 (13144)	21145 (13060)
Annual Payroll	29222.08 (107142.19)	32915.38 (129850.41)
Wage	26.08 (7.89)	26.95 (9.59)
Employment	876.11 (2229.84)	903.07 (2279.61)
Establishments	41.91 (80.22)	41.95 (79.26)
HPI	84.84 (9.84)	84.84 (9.84)
Black Share	22.51 (24.04)	22.33 (23.97)
White Share	71.16 (24.33)	71.36 (24.33)

Table 2.7: Summary Stats of Sample Cities Versus All Cities

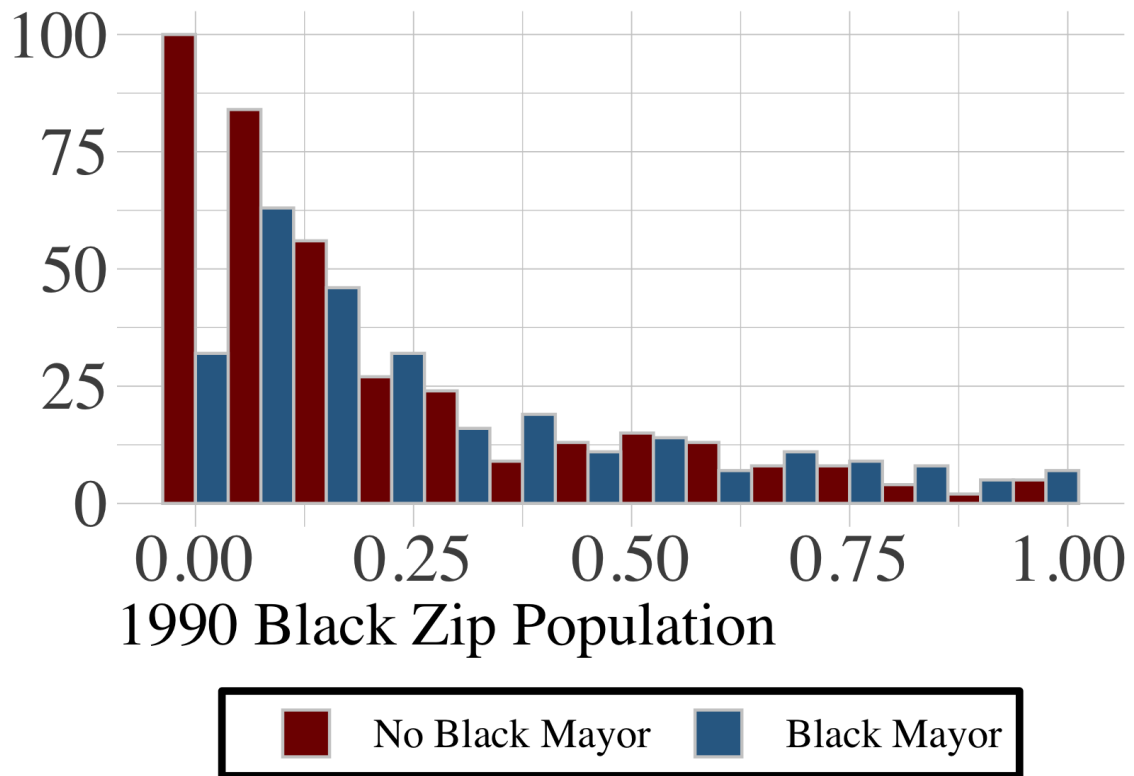


Figure 2.8: Distribution of 1990 Black Zip Code Population By Treatment Status

Figure displays the 1990 distribution of zip codes according to their Black population share. Blue bars show the distribution of treated cities which elected a black candidate from 2000 to 2010. Red bars show the distribution of control cities which observed an interracial election over the sample period or elected a Black candidate after 2010.

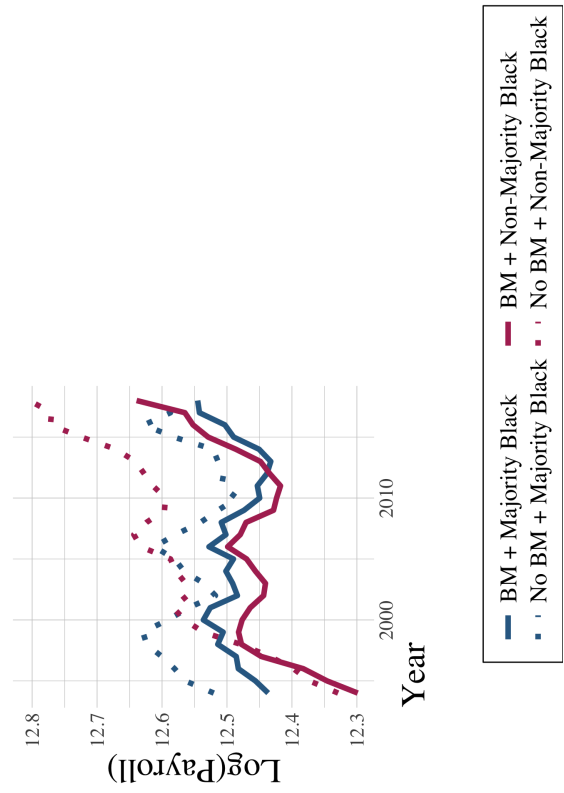
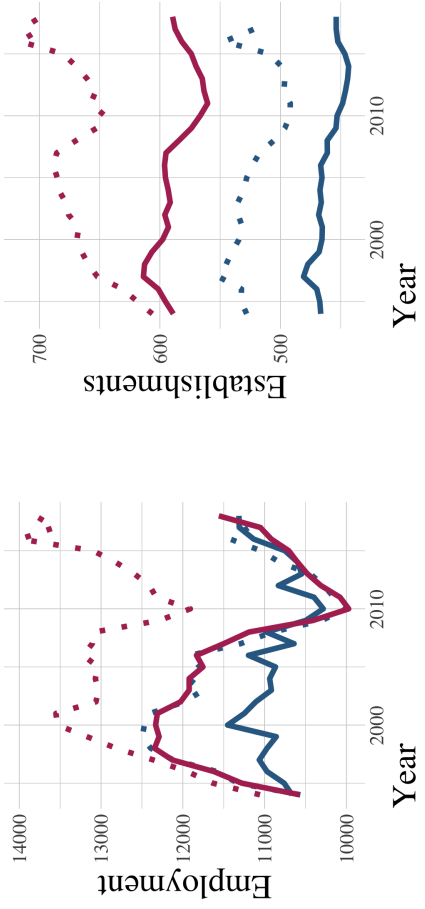
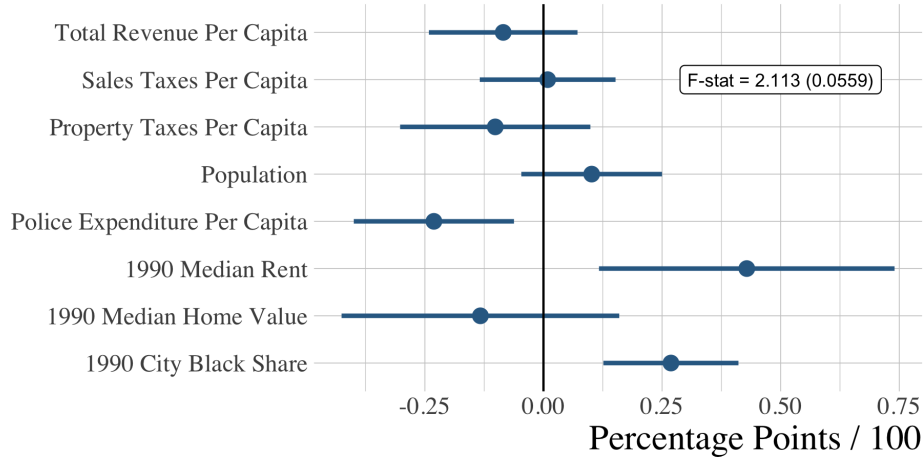


Figure 2.9: Average Levels Of Economic Outcomes By Treatment Status And Racial Composition

Figure displays the yearly averages of each economic activity measure at the zip code level separated by treatment status and by zip code racial composition from 1994-2018. Solid lines indicate the city elected a Black candidate for the first time from 2000 to 2010, and dotted lines are averages for control units. Blue lines show averages for majority-Black zip codes, and red lines are for non-majority-Black zip codes.

A

**Higher Black Populations, Higher Rents,  
And Lower Per Capita Police Expenditures  
Increase The Likelihood Of A Black Election**



B

**1990 City Characteristics Do Not Predict  
When Black Candidates Are Elected**



Figure 2.10: 1990 Characteristics Have Some Predictive Power On If But Not When A Black Candidate Is Elected

Panel A displays results from the regression of  $Black Mayor_c = \Gamma X_c + \varepsilon$  where  $X_c$  is a vector of city-level characteristics and a constant. The vector of characteristics are standardized, therefore the interpretation for each characteristic is a one standard deviation increases the likelihood of a Black candidate ever being elected by the number of percentage points shown in the figure. Panel B displays results from the regression  $Breakthrough Year_c = \Lambda X_c + v$  where  $X_c$  is a vector of city-level characteristics and a constant. The vector of characteristics are standardized, therefore the interpretation for these estimates is that a one standard deviation change in the characteristic predicts the year in which a Black candidate will become mayor. Intercepts not shown. Joint-F statistics printed in the top right of each panel.

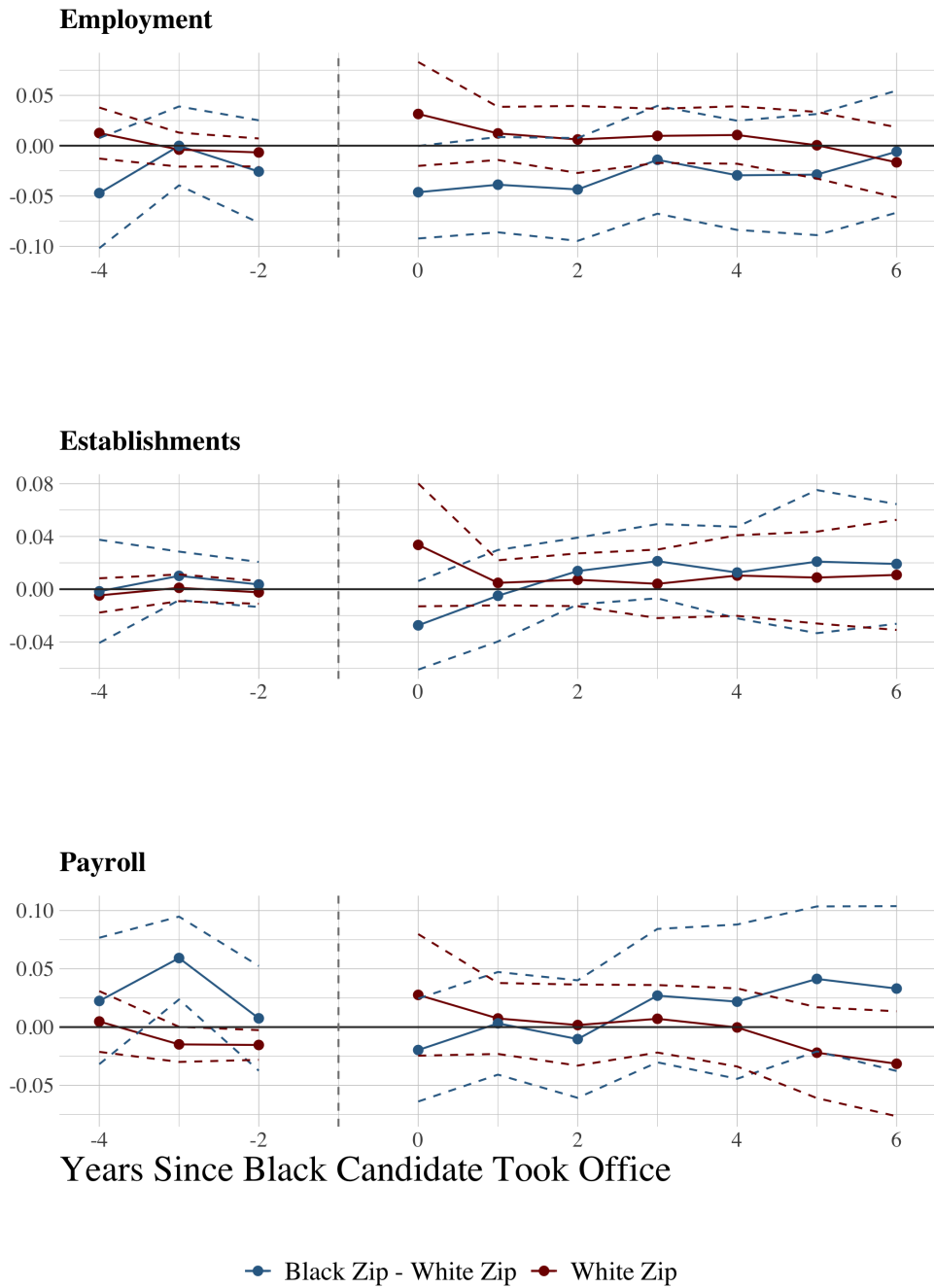


Figure 2.11: Event-Study Estimates Of Falsification Exercise

Results display the event study analog of Table 2.4. The sample is the set of cities in 1990 who did not elect a Black candidate over the period but had a Black population share of at least 10 percent. Cities that observed an interracial election from 1998 to 2012 (14 cities) are given a placebo treatment in the year the Black candidate would've taken office. Stacks are constructed by appending the placebo treatment city to the set of sample cities that never experienced an interracial election where the event window is four years before the placebo breakthrough and six years afterward. The dependent variables are the natural log of employment (top panel), establishments (middle panel), and payroll (lower panel) per zip code resident.

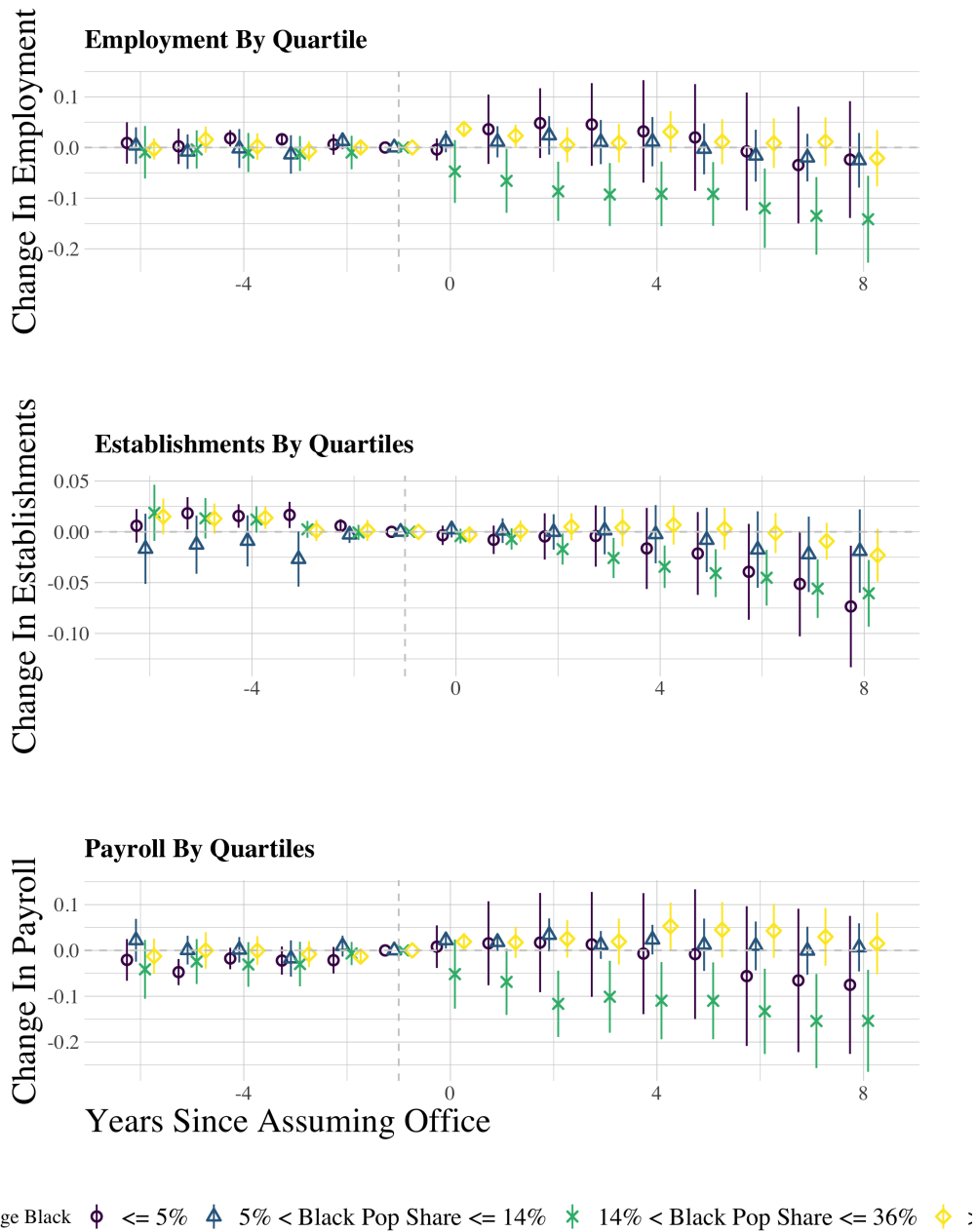


Figure 2.12: Event-Study Estimates Of Changes In Economic Activity By Quartile

Results display event-study estimates for the total effect in each quartile by the 1990 zip code Black population. The dependent variables are the natural log of employment, establishments, and payroll per zip code resident. The first quartile are those whose 1990 Black populations are less than or equal to 5 percent; the second quartile are zip codes greater than 5 percent and less than or equal to 14 percent; third quartile are zip codes greater than 14 percent and less than or equal to 36 percent; the fourth quartile are the zip codes greater than 36 percent. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. 90 percent confidence intervals clustered at the breakthrough election level are shown.

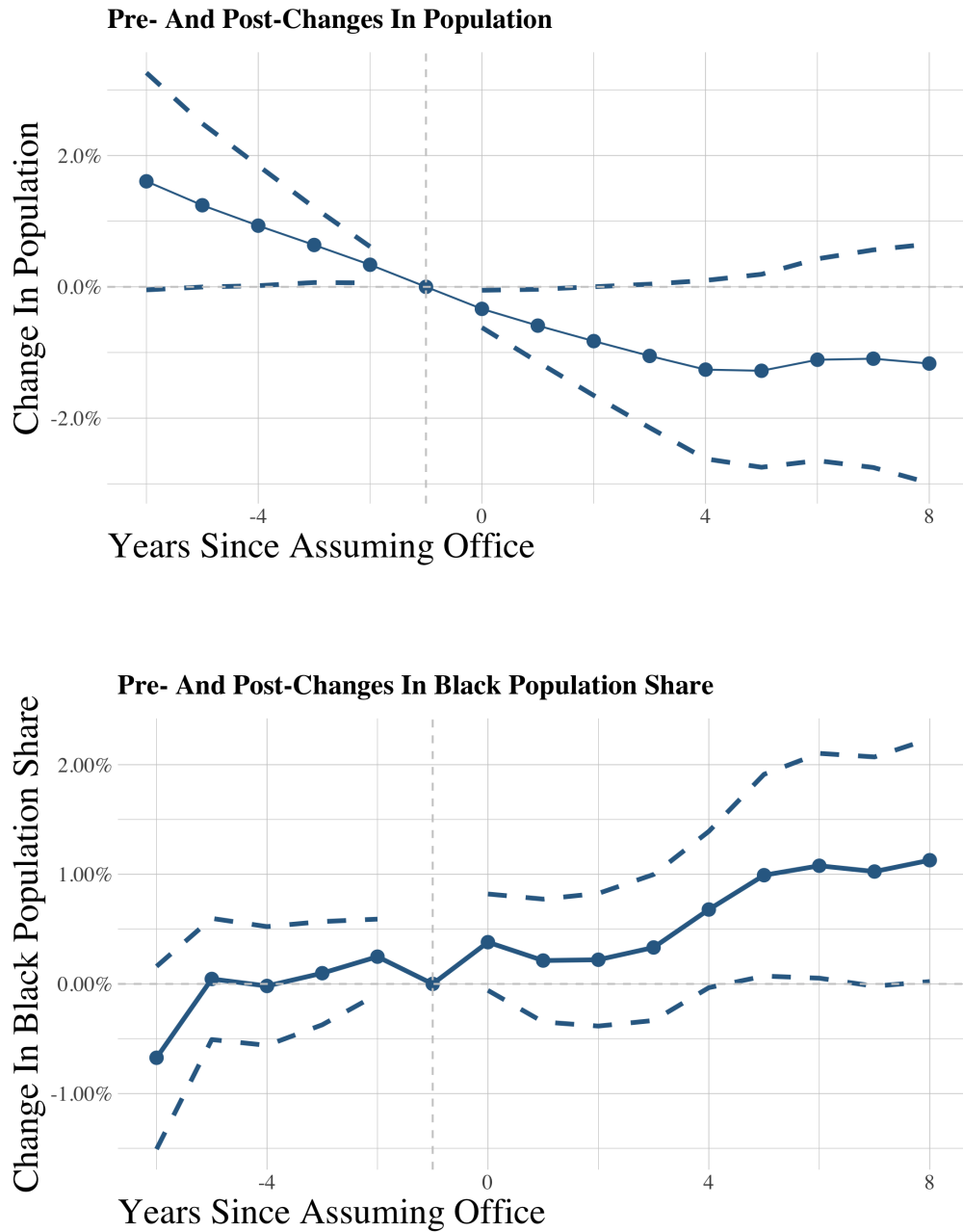
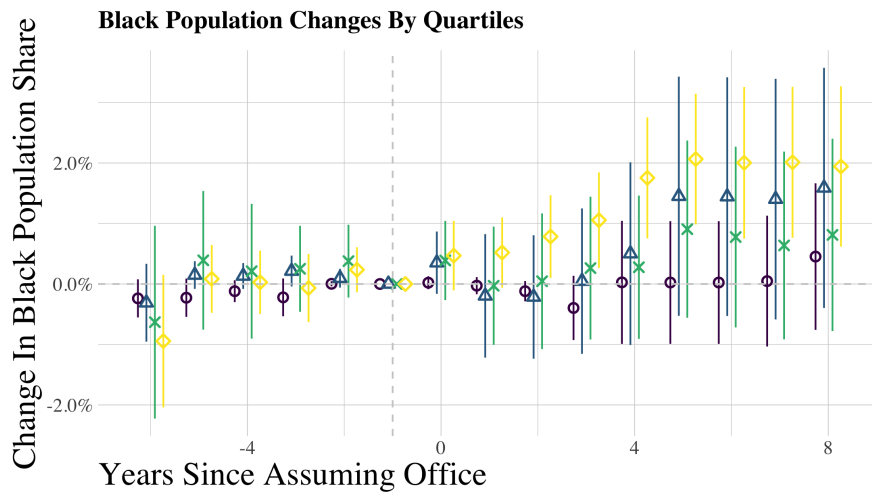
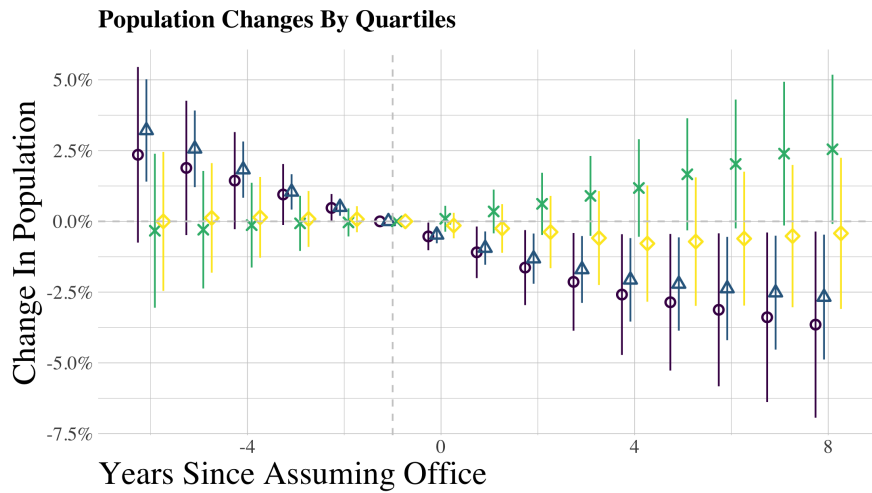


Figure 2.13: Population Is Declining In The Pre-Period & Continues For Four Years But The Black Population Share Is Not Increasing Until After The Breakthrough Election

Results display event-study estimates for the natural log of population in the top panel and the share of the Black population in the bottom panel at the zip code level. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. 90 percent confidence intervals clustered at the breakthrough election level are shown.





: % -age Black    $\phi$   $\leq 5\%$     $\blacktriangle$   $5\% < \text{Black Pop Share} \leq 14\%$     $\ast$   $14\% < \text{Black Pop Share} \leq 36\%$     $\blacklozenge$  :

Figure 2.14: The First & Second Quartiles Are Depopulating But It Is Not Reflected By Changes In The Black Population Share

Results display event-study estimates by quartile for the natural log of population in the top panel and the share of the Black population in the bottom panel at the zip code level. The first quartile are those whose 1990 Black populations are less than or equal to 5 percent; the second quartile are zip codes greater than 5 percent and less than or equal to 14 percent; third quartile are zip codes greater than 14 percent and less than or equal to 36 percent; the fourth quartile are the zip codes greater than 36 percent. Standard errors are clustered at the breakthrough election level, and 90 percent confidence intervals are shown in each panel. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of cities whose Black populations are above 10 percent and either experienced an interracial election or elected a Black candidate more than 8 years after the focal breakthrough election. Each breakthrough city forms a panel with the permissible control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. 90 percent confidence intervals clustered at the breakthrough election level are shown.

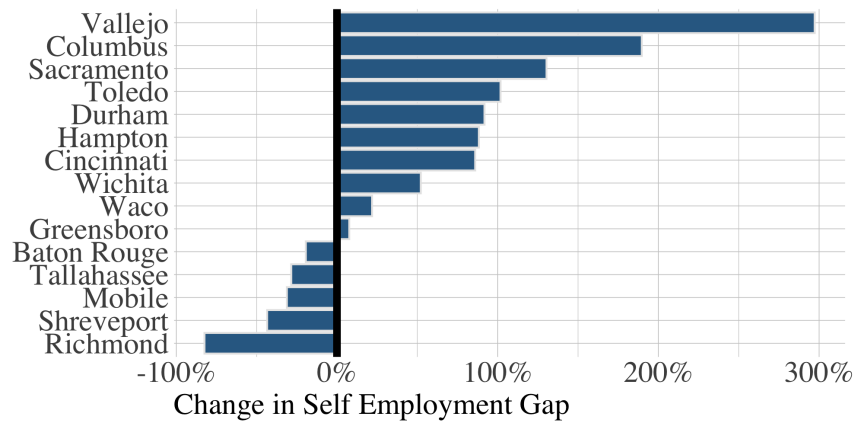
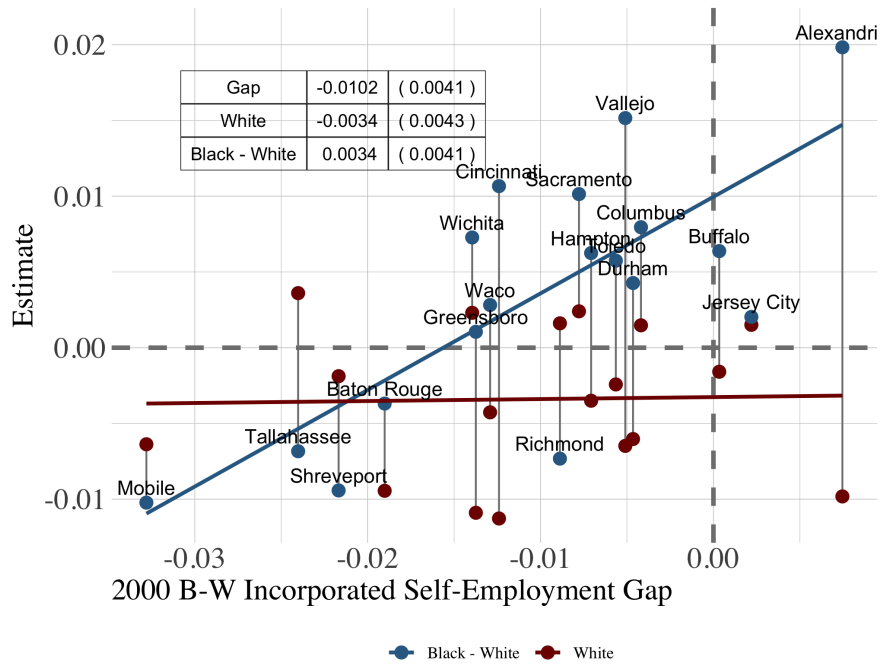


Figure 2.15: Breakthrough Mayors, Incorporated-Self-Employment Outcomes, & City-Specific Changes In The B-W Gap

The top panel displays results from the estimation of Equation 2.3 using Census and ACS person weights where the outcome is an indicator for being self employed with incorporated status. The results from that regression are then plotted against the estimated city-specific Black-White gap in self employment in 2000. The red points show the breakthrough-specific point estimates of  $\tau_{e1}$ , the breakthrough effect on White self employment. Blue points show relative self-employment gains for Black residents relative to White estimates. The table at the top of the panel shows the average effect across events. *e.g.*  $Gap = \bar{gap} = \frac{1}{18} \sum_{e=1}^{18} gap_e$ . Other estimates are defined similarly. The bottom panel takes the blue points and divides it by the city-specific self-employment gap, then plots the reduction in the B-W self employment gap ordering them from largest to smallest.

	Share Filing (1)	Log(AGI) (2)	Log(Salary) (3)	Log(Business Income) (4)	Log(AGI Per Filer) (5)	Log(Salary Per Filer) (6)	Log(Business Income Per Filer) (7)
Black Mayor	-0.009** (0.003)	-0.020 (0.041)	0.0001 (0.047)	-0.108 (0.066)	-0.029 (0.030)	-0.013 (0.012)	-0.095 (0.070)
$BM \times BZ$	0.017* (0.009)	0.181 (0.133)	0.158 (0.128)	0.176** (0.076)	0.048 (0.042)	0.031 (0.028)	0.180** (0.085)
$\tau_1 + \tau_2$	0.008 (0.011)	0.161 (0.166)	0.158 (0.159)	0.068 (0.064)	0.019 (0.033)	0.018 (0.029)	0.086 (0.062)
Observations	86,598	86,598	86,510	50,374	86,598	86,510	50,374

*Notes:* Table 2.8 displays estimation of stacked weighted least squares of equation  $\text{efeq}:\text{stackDD}$  weighted by 1990 zip code population. Column 1 shows the effect on the zip code share of filers which is the total number of filers in a year divided by zip code population. Column 2 displays the natural log total adjusted gross income at the zip code level. Column 3 displays the natural log of total salaries and wages earned at the zip code level. Column 4 displays the natural log of total income earned from sole proprietorships at the zip code level. Columns 5-7 repeat the analysis from columns 2-4 but is the natural log of each outcome divided by the number filers in each zip code. Data come from the Internal Revenue Service's Statistics on Income for the years 1998, 2001, 2002, 2004-2018. Observations are missing for some zip-code-years to avoid disclosure of information about individual taxpayers.

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

Table 2.8: The Share of Filers In Black Zip Codes Increases Relative To Non-Black Zip Codes

### 2.8.1.1 Why Not Regression Discontinuity

Vogl (2014) finds that Black candidates win more competitive interracial elections than would be expected if they were random. Figure 2.16 plots a histogram of all interracial elections involving a Black candidate from 1994 to 2018. I can confirm Vogl’s finding in this sample period. Black candidates win substantially more close elections than non-Black ones. In practice, non-Black candidates are generally White. I plot the histogram using `rdplotdensity` based on the local polynomial density estimator in Cattaneo, Jansson, and Ma (2020). The null hypothesis of a randomly elected candidate can be rejected for margins up to 22% which contains even landslide victories.

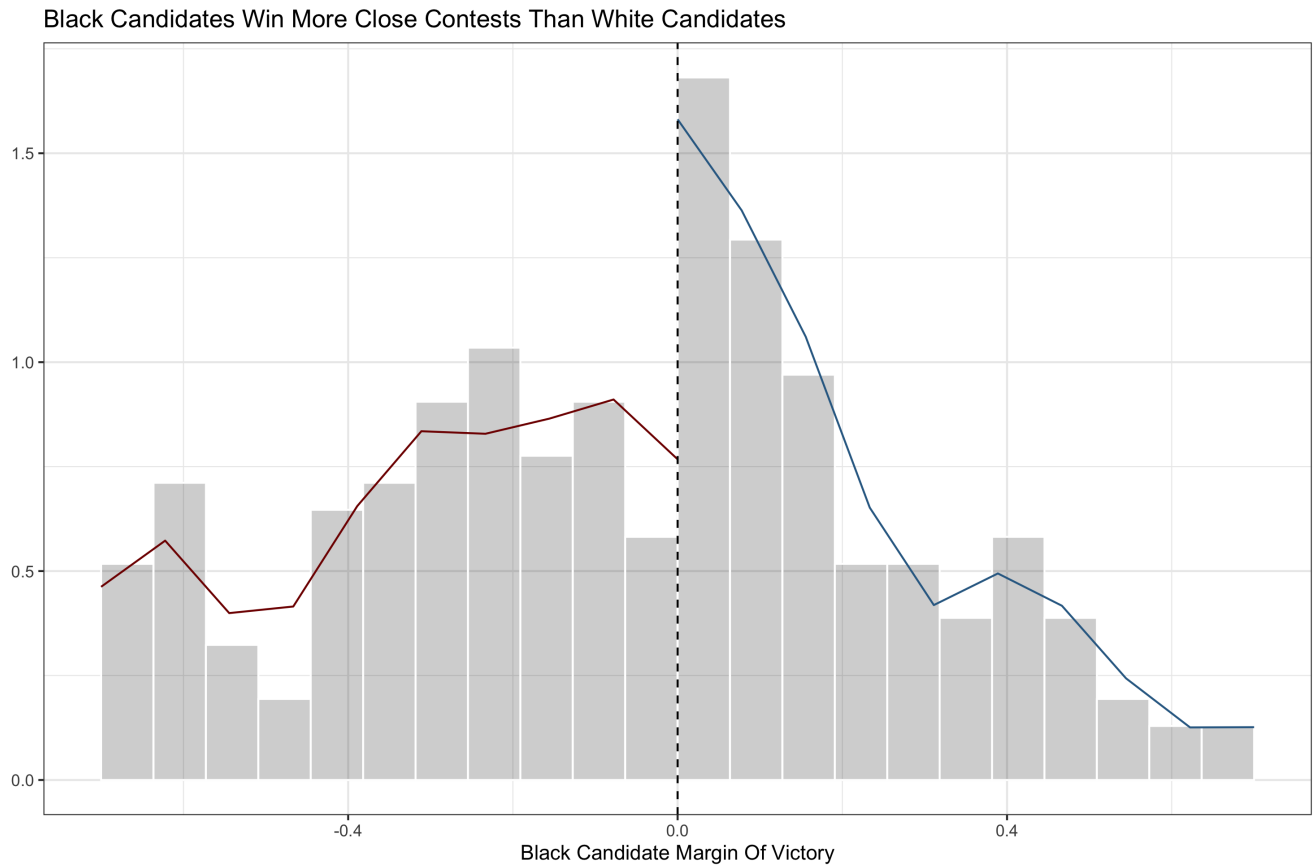


Figure 2.16: Histogram Of Margin Of Victory Of Black Candidates

Histogram produced using `rdplotdensity`. Sample is all interracial elections from cities with 1990 populations greater than 100,000 from 1994 to 2018.

**2.8.1.1.1 Power Calculations** Following Mello (2019) and Schochet (2009), the minimum detectable effect (MDE) for significance level  $\alpha$  and power  $\kappa$  under the assumption that the change in the outcome of interest  $\Delta(y)$  is a linear function absent the discontinuity is

$$MDE = (t_{\alpha/2} + t_{1-\kappa}) \times \sqrt{\frac{1}{D(1-D)} \frac{\sigma_{\Delta y}^2}{N} \frac{1}{(1-\rho^2)}} \quad (2.4)$$

where  $D$  is the fraction of cities assigned to treatment and  $\rho$  is the correlation between the score and treatment status. Outcome variability is  $\sigma_{\Delta(y)}^2$ , and  $N$  is the number of observations. Following convention, I set  $\alpha = 0.5$  and

$\kappa = 0.8$  so  $(t_{\alpha/2} + t_{1-\kappa}) = 2.8$ . In the traditional RD setting, only a subset of observations within the score threshold are used for estimation, *i.e.* the bandwidth. For a range of bandwidths (reported as standard deviations of the score threshold) I report the minimum detectable effect in Table 2.9 for the aggregate effect of each outcome and compare those results to the difference-in-differences estimates from Table 2.2 used as an example. Across all outcomes, RD is not sufficiently powered to detect an effect for any outcome. Even at 2 standard deviations which covers all but the most one-sided elections RD is underpowered to find any effect.

Outcome	DD estimate (Table 2.2, columns 1, 4, 7)	0.5	0.75	1	1.5	2
Employment	2.2%	6.3%	6.3%	5.5%	5.1%	4.9%
Establishments	2.0%	4.1%	4.0%	3.5%	3.3%	3.1%
Payroll	-0.009%	7.4%	7.3%	6.3%	6.0%	5.7%

Table 2.9: Regression Discontinuity Power Calculations

See Appendix 2.8.1.1.1 for details. Table shows the minimum detectable effect for a regression discontinuity design under a linearity assumption where the outcome is change in economic activity listed in the first column and the bandwidths used to construct the sample are listed in columns 3-7. Column 2 shows the difference-in-differences citywide effect from Table 2.2.

### 2.8.1.2 Lagged Identification Design & Conventional TWFE Estimation

In this section, I present results for conventional two-way fixed effects estimation and results where the independent variable is lagged one-period and a non-absorbing state. In minor contrast to Equation 2.2, I estimate

$$y_{zct} = \alpha_z + \delta_t^r + \tau_1 BM_{ct} + \tau_2 BM_{ct} \times BZ_{zc} + \epsilon_{zct} \quad (2.5)$$

where the event notation  $e$  is dropped; otherwise estimation is similar to the stacked procedure.

Then, I estimate the breakthrough election effect where the Black mayor indicator is lagged by one year, and it is no longer an absorbing state. In other words, the lagged variable captures if there was a Black mayor in the previous period. I estimate

$$y_{zct} = \alpha_z + \delta_t^r + \tau_1 BM_{ct-1} + \tau_2 BM_{ct-1} \times BZ_{zc} + y_{zct-1} + \epsilon_{zct} \quad (2.6)$$

where all variables are defined similarly. The independent variable is often used to overcome concerns of reverse causality; however, the validity of the model requires that there are no dynamics in the dependent variable (see Bellemare, Masaki, and Pepinsky (2017)). A simple way to test this is to include a lag of the dependent variable on the right hand side. If the lag is significant, then the assumption is likely violated. I present results of these estimations in Table 2.10.

	Stacked (1)	Conventional (2)	Lagged (3)	Lagged Dynamic (4)
<i>Panel A: Employment</i>				
BM	-0.033* (0.018)	-0.026 (0.024)		
BM × BZ	0.058 (0.049)	0.011 (0.055)		
BM <sub>-1</sub>			-0.022 (0.019)	-0.005 (0.005)
Log Emp Per Capita <sub>-1</sub>				0.853*** (0.025)
BM <sub>-1</sub> × BZ			0.025 (0.045)	-0.0001 (0.007)
<i>Panel B: Establishments</i>				
BM	-0.029** (0.013)	-0.029 (0.022)		
BM × BZ	0.051** (0.023)	0.018 (0.034)		
BM <sub>-1</sub>			-0.017 (0.014)	-0.004 (0.003)
Log Est Per Capita <sub>-1</sub>				0.876*** (0.042)
BM <sub>-1</sub> × BZ			0.034 (0.024)	0.010*** (0.004)
<i>Panel C: Payroll</i>				
BM	-0.024 (0.026)	-0.014 (0.033)		
BM × BZ	0.077 (0.065)	0.024 (0.075)		
BM <sub>-1</sub>			-0.004 (0.024)	-0.005 (0.006)
Log AP Per Capita <sub>-1</sub>				0.852*** (0.019)
BM <sub>-1</sub> × BZ			0.026 (0.064)	0.008 (0.010)
$\tau_1 + \tau_2^{Emp}$	0.025 (0.036)	-0.015 (0.041)	0.003 (0.035)	-0.006 (0.025)
$\tau_1 + \tau_2^{Est}$	0.022 (0.015)	-0.01 (0.023)	0.017 (0.016)	0.006 (0.041)
$\tau_1 + \tau_2^{Pay}$	0.053 (0.049)	0.01 (0.061)	0.022 (0.053)	0.003 (0.018)
Observations	111,300	16,200	15,521	14,895

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Notes: Results show estimates from 4 separate estimations of my base outcomes. Column 1 contains estimates of my main specification of Equation 2.2 without the inclusion of pre-election trends. These are the same as the estimates in columns 2, 5, and 8 of Table 2.2. Column 2 displays results from conventional two-way fixed effects estimation of Equation 2.5. Columns 3 and 4 are conventional two-way fixed effects estimation where the independent variable is lagged one year of Equation 2.6. Column 4 includes a lag of the dependent variable to test for dynamics in the dependent variable. Each estimation is weighted by 1990 zip code population and standard errors are clustered at the breakthrough election level in column 1 and the city level in all other columns.

Table 2.10: Breakthrough Election Effect By Zip Code Racial Composition Using Conventional TWFE And Lagged Independent Variable

### 2.8.1.3 Do Trends Change Post-Election? Trend-Break Specification

If trends in cities that elect a Black candidate for a first time change relative to control cities, then we are unable to identify the effect of subsequent Black winners using a similar strategy. My main event study results suggest that breakthrough elections constitute a “trend break,” that is, a change in the growth rates of the economic measures I observe. To formalize this notion, I estimate a “trend-break” specification similar to Goodman-Bacon and Schmidt (2020) which interacts a time trend centered on each breakthrough election with a Black mayor trend break. I estimate

$$y_{ezct} = \alpha_{ez} + \delta_{et}^r + T_1 BM_{ect} \times (t - B_{ec}) + T_2 BM_{ect} \times (t - B_{ec}) \times BZ_{ezc} \\ + \Gamma_1 (t - B_{ec}) + \Gamma_2 (t - B_{ec}) \times BZ_{ezc} + \epsilon_{ezct}, \quad (2.7)$$

where  $\Gamma_1$  tests for differential linear pre-trends in each breakthrough city and  $\Gamma_2$  tests for differential linear pre-trends in Black relative to non-Black zip codes.  $T_1$  measures the yearly differences in economic activity in non-Black communities, and  $T_2$  measures relative yearly differences in Black communities. I present the results in Table 2.11 where the outcomes are from my main specification and then I present trend-break results from the zip code totals of each outcome as well as a property value index measure from the Federal Housing Financing Agency (FHFA) in Table 2.12.

	Log(Employment Per Capita)	Log(Establishments Per Capita)	Log(Payroll Per Capita)
	(1)	(2)	(3)
Pre-Breakthrough Trend	0.001 (0.002)	-0.002 (0.002)	0.002 (0.003)
Pre-Breakthrough Trend $\times$ BZ	0.003 (0.008)	0.004 (0.004)	0.004 (0.011)
Post-Breakthrough Trend $\times$ BZ	0.008 (0.007)	0.005 (0.003)	0.011 (0.008)
Post-Breakthrough Trend	-0.009*** (0.003)	-0.005** (0.002)	-0.010** (0.004)
$\tau_1 + \tau_2$	-0.001 0.006	0 0.003	0 0.007
Observations	111,300	111,300	111,300

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

*Notes:* The dependent variables are listed above each column. The first three columns estimate my main outcomes where the log of the zip code total is used instead of adjusting for the zip code population. The final column estimates the change in the Federal Housing Finance Agency Housing Price Index at the zip code level using 2000 as the base year. Observations are lower in the final column because there was not a corresponding HPI value for all zip codes, so they are excluded from the housing index analysis. Model is weighted least squares estimates of equation 2.7 weighted by 1990 zip code population.

Table 2.11: Breakthrough Election Trend Breaks In Economic Activity Per Zip Code Resident



	Log(Employment)	Log(Establishments)	Log(Payroll)	HPI w/ 2000 Base
	(1)	(2)	(3)	(4)
Pre-Breakthrough Trend	-0.002 (0.003)	-0.004 (0.003)	-0.0001 (0.003)	-0.032** (0.013)
Pre-Breakthrough Trend $\times$ BZ	-0.003 (0.008)	-0.002 (0.004)	-0.002 (0.011)	0.002 (0.014)
Post-Breakthrough Trend $\times$ BZ	0.010 (0.006)	0.007*** (0.002)	0.013 (0.008)	0.007 (0.018)
Post-Breakthrough Trend	-0.007** (0.003)	-0.003 (0.002)	-0.008* (0.004)	0.038 (0.023)
$\tau_1 + \tau_2$	0.003 0.005	0.005 0.003	0.005 0.007	0.045 * 0.025
Observations	111,300	111,300	111,300	89,653

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Notes: The dependent variables are listed above each column. The first three columns estimate my main outcomes where the log of the zip code total is used instead of adjusting for the zip code population. The final column estimates the change in the Federal Housing Finance Agency Housing Price Index at the zip code level using 2000 as the base year. Observations are lower in the final column because there was not a corresponding HPI value for all zip codes, so they are excluded from the housing index analysis. Model is weighted least squares estimates of equation 2.7 weighted by 1990 zip code population.

Table 2.12: Breakthrough Election Trend Breaks In Zip Code Economic Activity

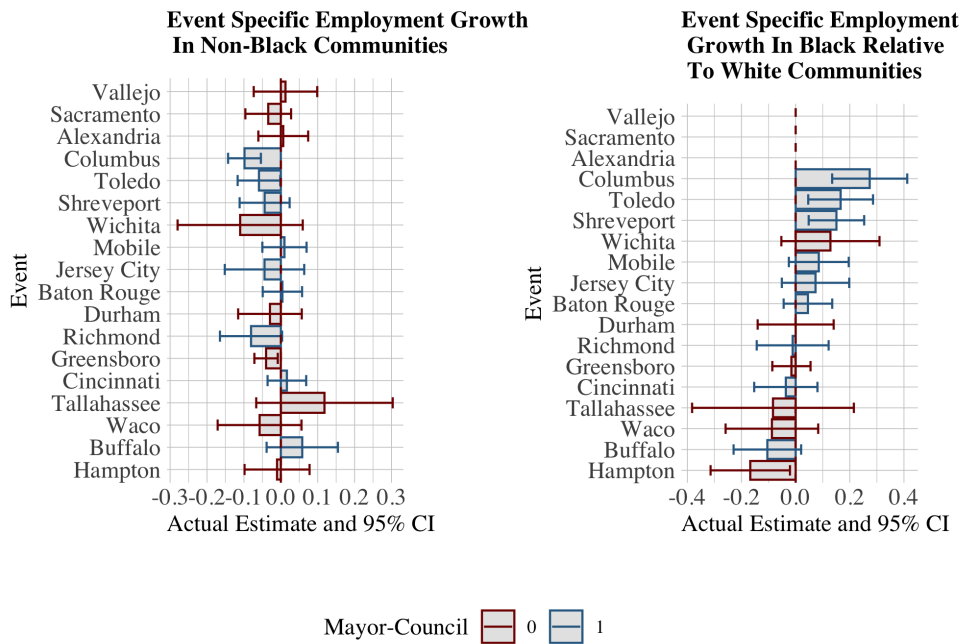
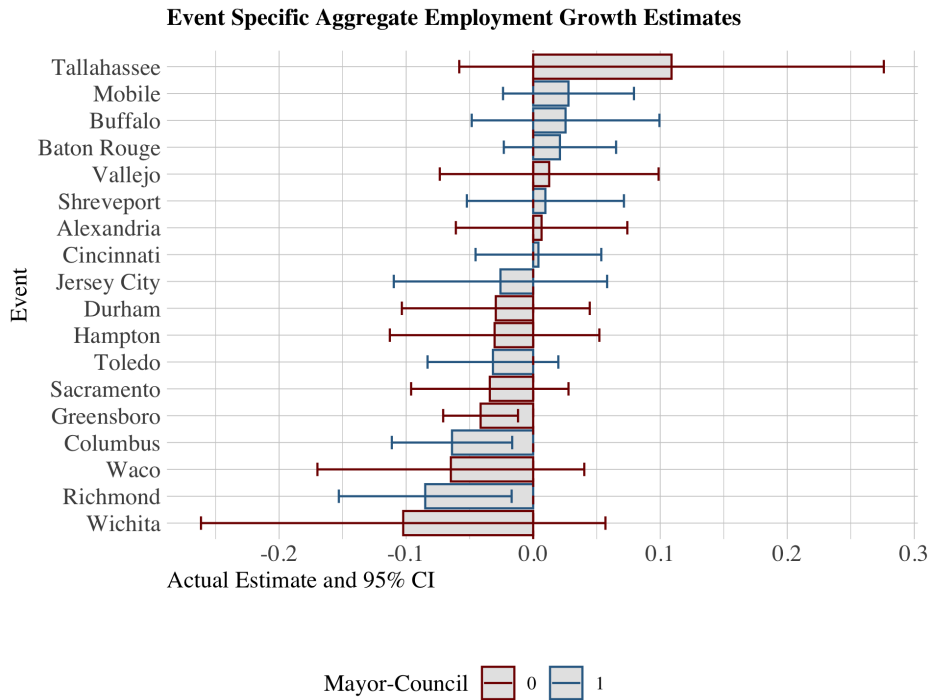


Figure 2.17: Event-Specific Estimates Of Employment

Results display event-specific estimates for the natural log of employment per zip code resident. For each event, sample is constructed similarly to my main specification. The top panel captures the aggregate effect for each treatment city. The bottom panel estimates an event-specific version of equation 2.2 for each event. The bottom left figure captures  $\tau_1$ , the total effect in non-Black zip codes, and the bottom right panel captures  $\tau_2$  the relative effect between Black and non-Black zip codes. I report 95 percent heteroskedastic robust confidence intervals.

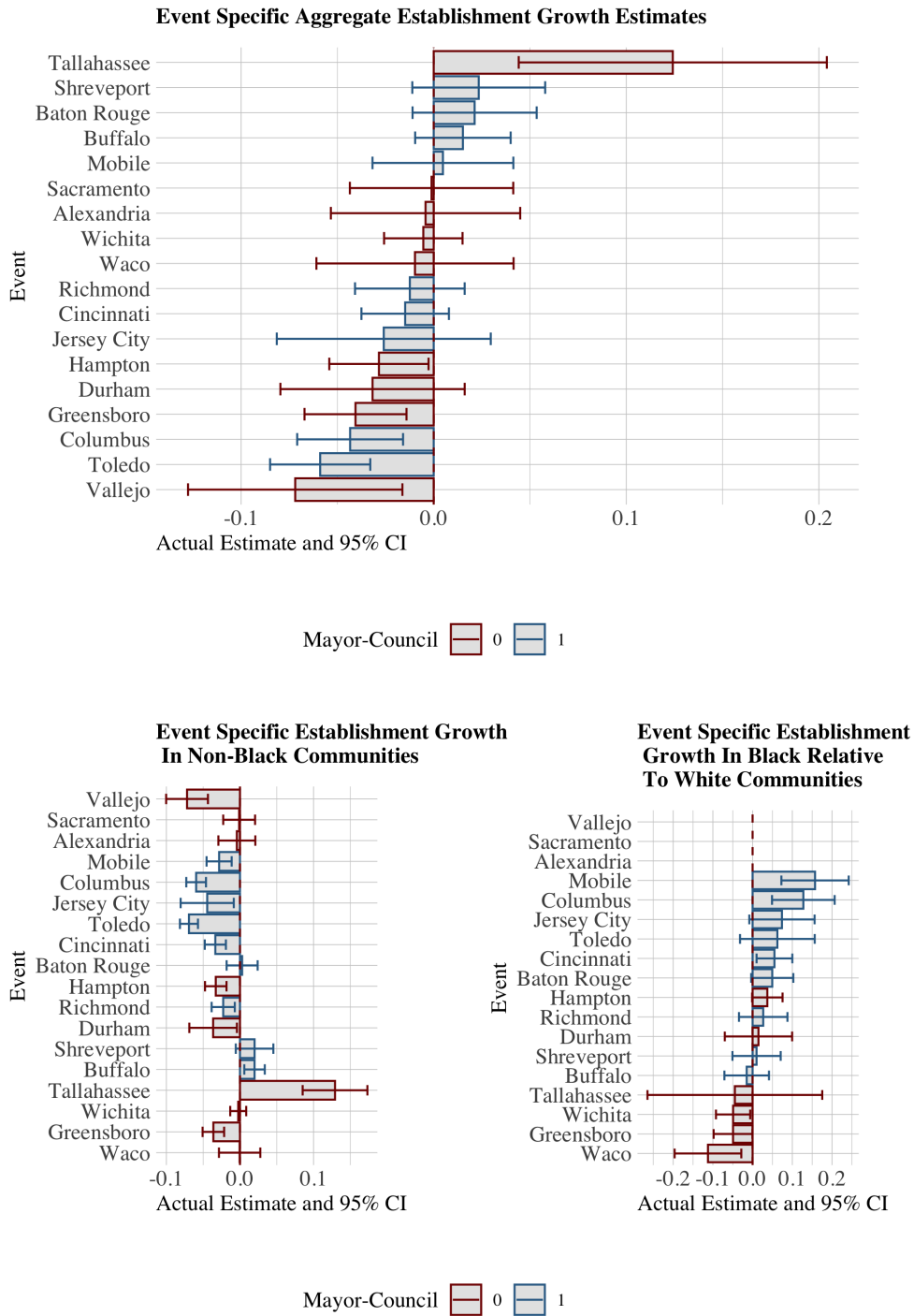


Figure 2.18: Event-Specific Estimates Of Establishments

Results display event-specific estimates for the natural log of establishments per zip code resident. For each event, sample is constructed similarly to my main specification. The top panel captures the aggregate effect for each treatment city. The bottom panel estimates an event-specific version 2.2 for each event. The bottom left figure captures  $\tau_1$ , the total effect in non-Black zip codes, and the bottom right panel captures  $\tau_2$  the relative effect between Black and non-Black zip codes. I report 95 percent heteroskedastic robust confidence intervals.

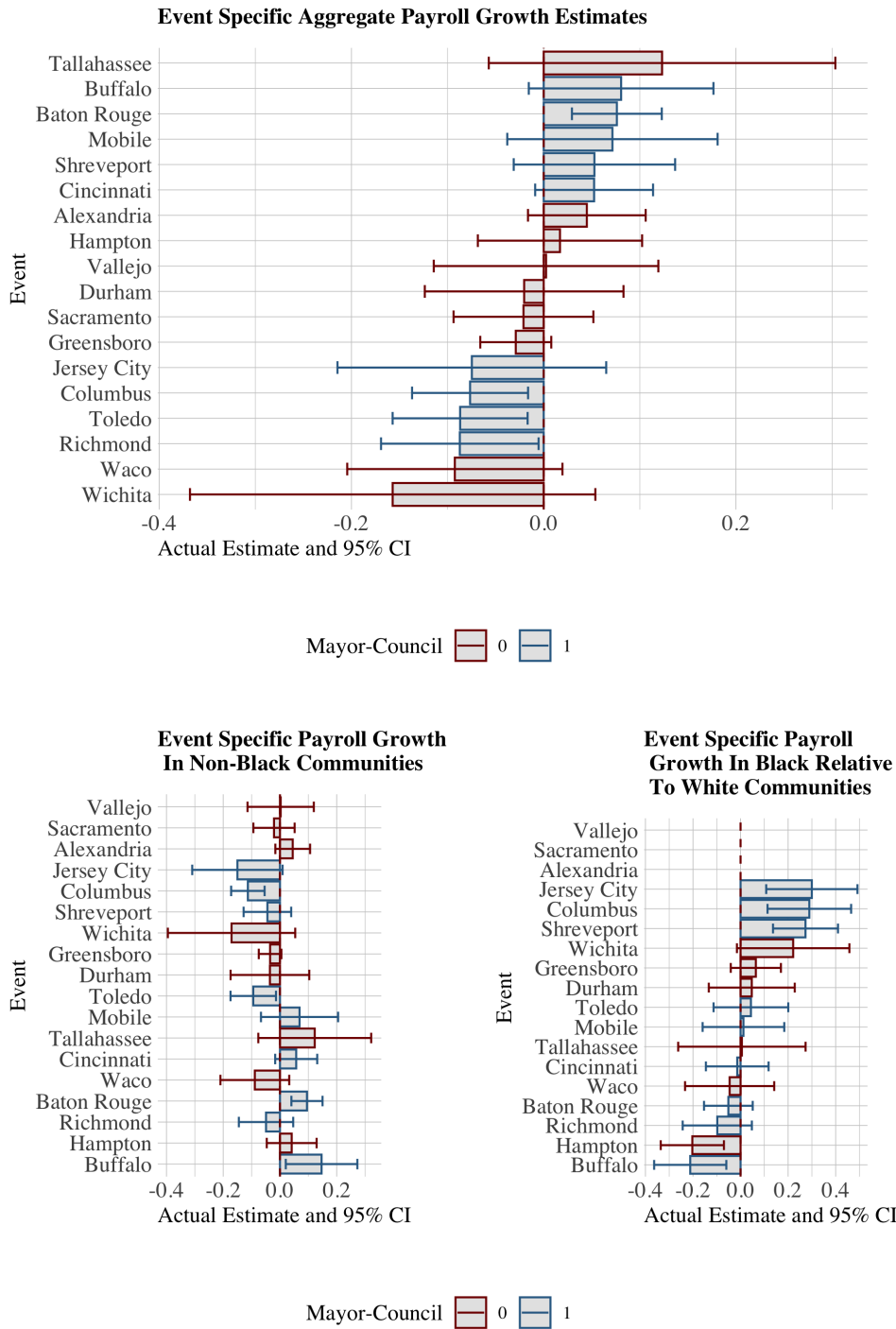


Figure 2.19: Event-Specific Estimates Of Payroll

Results display event-specific estimates for the natural log of payroll per zip code resident. For each event, sample is constructed similarly to my main specification. The top panel captures the aggregate effect for each treatment city. The bottom panel estimates an event-specific version of 2.2 for each event. The bottom left figure captures  $\tau_1$ , the total effect in non-Black zip codes, and the bottom right panel captures  $\tau_2$  the relative effect between Black and non-Black zip codes. I report 95 percent heteroskedastic robust confidence intervals.

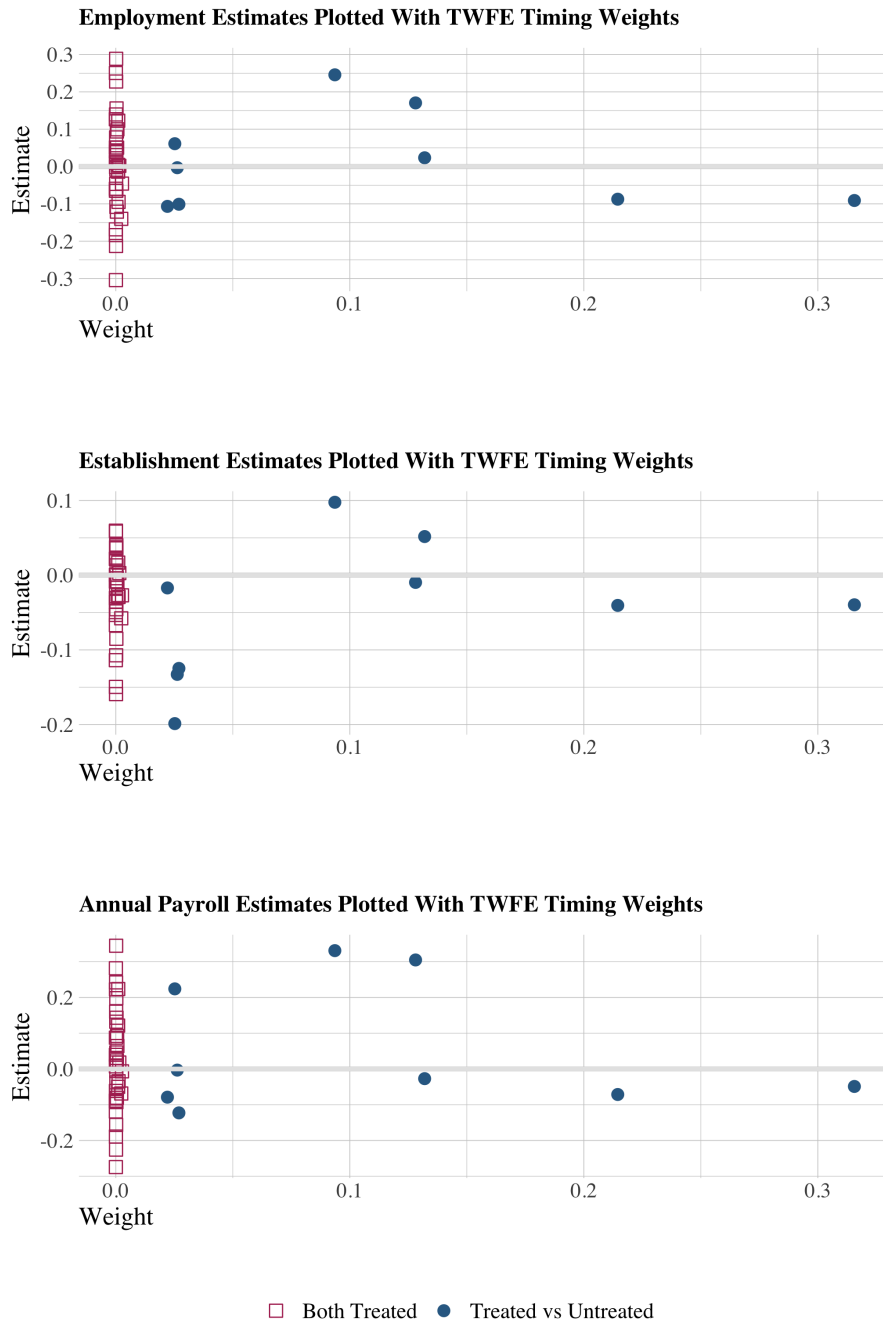


Figure 2.20: Difference-In-Differences Decomposition Of Estimates And Timing Weights

Figure plots out differences-in-differences decomposition of two-way fixed effects estimation as suggested by Goodman-Bacon (2018). Coefficient estimates for each timing group are on the y-axis and the weights attributable to each timing group are listed on the x-axis. 2x2 treated vs. untreated estimates are plotted using solid circles, and 2x2 estimates between timing groups are plotted using hollow red squares. To maintain balance, cities that elect a breakthrough candidate after 2010 are excluded.

## 2.8.2 Discussion Of Citywide Mechanisms: How Do Mayors Affect Local Outcomes?

An active literature in both economics and political science debates the policy impact mayors have or whether they have any at all.<sup>41</sup> Despite this debate mayors possess a number of both direct and indirect levers to influence economic development. I focus on three levers —crime reduction, housing, and taxes —that influence economic development to understand the drivers in the community-specific changes to economic activity that potentially have differential impacts by race. Because breakthrough mayors have varying authority to affect policy even though they might campaign on improving a range of outcomes, I also estimate each set of regressions including only those events where the mayor has explicit influence over the outcome because they serve as the chief executive.

### 2.8.2.1 Crime Reduction

Policies on crime and policing impact the economic trajectory of a community. If reductions in crime improve neighborhoods through increases in housing value and beliefs about the return on investment for potential businesses, then both the types of policies pursued and the police chief charged with leading the department are important decisions. Additionally, mayors may direct changes to policing patterns that have often resulted in discriminatory behavior toward Black residents. Reduced interactions with law enforcement and incarcerations may then mechanically lead to increased economic activity in Black communities. I look for evidence of this in Table 2.13 which reports results for various offense categories. Regressions are stacked city-level estimates of offenses at the yearly level. I report standard errors clustered at the breakthrough election level below each estimate. The first panel shows results for the entire sample. Across a range of outcomes, there appears to be no change in offenses. The first column shows that after a breakthrough election the total number of crimes declines 3.4 percent, but the estimate is imprecise as I cannot rule out small-to-medium positive or negative effects. Because “all crimes” likely captures changes to crimes committed most frequently like theft, I narrow the definition in columns 2-5. Column 2 shows the effect on index crimes, a set of serious crimes that “occur with regularity in all areas of the country, and are likely to be reported to police,”<sup>42</sup> and columns 3 and 4 separate index crimes into violent and property crimes categories.<sup>43</sup> The point estimates are of both signs, and the confidence intervals do not allow me to rule out effects of modest size in either direction. Murder is the crime which receives the greatest focus both in campaigns and cultural perception. Column 5 shows results for changes to the murder rate, and the effect corresponds to an small increase in murder but this is imprecisely measured. Last, I look at the murder clearance rate, the total numbers of murders solved divided by the total number of murders which may describe changes in investigative capacity, and I do not find any changes to agencies where a Black candidate is elected.

The imprecise results may be because in half of the breakthroughs considered, the mayor has no official channel to change policing behavior or staffing. In panel B, I retain only mayor–council governments and report the results for those events. For these governments, the mayor can appoint the police chief and influence staffing. The police department reports to the mayor in this situation, so we should expect an effect, if there is any, in these municipalities. The results in panel B across all outcomes are all of the same sign and of similar magnitude to those of panel A suggesting that the mayor has little effect on crime.

<sup>41</sup>For more on the debate, see Gerber and Hopkins (2011), Ferreira and Gyourko (2009), Ferreira and Gyourko (2014), Benedictis-Kessner and Warshaw (2016).

<sup>42</sup><https://ucr.fbi.gov/crime-in-the-u.s/2011/crime-in-the-u.s.-2011/offense-definitions>

<sup>43</sup>Violent index crimes are criminal homicide, forcible rape, robbery, and aggravated assault. Property index crimes are burglary, larceny, motor vehicle theft, and arson. These crimes compose total index crimes.

	Log All Crimes P.C.	Log Total Index P.C.	Log Property Index P.C.	Log Violent Index P.C.	Log Murder P.C.	Murder Clearance Rate
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Entire Sample</i>						
Black Mayor	-0.034 (0.051)	-0.034 (0.056)	-0.041 (0.056)	0.028 (0.061)	0.053 (0.078)	0.029 (0.049)
<i>Panel B: Mayor-Council Events</i>						
Black Mayor	-0.029 (0.042)	-0.010 (0.052)	-0.018 (0.051)	0.047 (0.084)	0.089 (0.098)	0.018 (0.068)
Observations	6,899	6,899	6,899	6,899	6,898	6,898

*Notes:* Coefficients are city-level, stacked least squares estimates. The dependent variables are listed at the top of each column. The second panel retains stacks where the mayor serves as chief executive. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of control cities with 1990 populations greater than 100,000 whose Black populations are above 10 percent that either observed an interracial election or elected a Black candidate more than eight years afterward. Each breakthrough city forms a panel with the control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. Standard errors, printed below point estimates and are clustered at the breakthrough election level. Data come from Jacob Kaplan's concatenated files of the Uniform Crime Reporting Program's Offenses Known and Clearances By Arrest at the place level from 1994 to 2018.

Table 2.13: Relationship Between Black Mayors And Crime

### 2.8.2.2 Residential Development

As the head of the executive branch, the mayor is charged with administrating and supervising all other departments. The mayor has the authority to appoint directors of departments and board members such as planning and zoning commissions, housing authorities, redevelopment commissions, *etc.*. One channel to increased development, then, is more permissive planning and/or zoning officials. Planning and zoning boards have outsized influence in the number of buildings, both residential and commercial, that get approved for construction and the size these buildings are constructed at after series of abutters hearings and impact studies. The mayor, then, influences the built environment of the municipality through these appointments.<sup>44</sup> I proxy for lenient development policy using the Census Bureau's Building Permits Survey which provides city-level data on the number of buildings and units authorized each year. The data are also broken down by the size of the structure by units &mdash; single-family home, duplex, triplex, . . . — and the value of each structure-by-size class.

Panel A shows the results for the entire sample and panel B shows the results for mayor–council events that would have the authority described in the previous paragraph. Column 1 of Table 2.14 shows that the number of new buildings permitted for construction per capita declines by 16.5 log points in the entire sample but the effect is reversed entirely in panel B. In both instances the confidence intervals overlap with zero, so I cannot rule out a null effect; however, I cannot rule out substantial gains in the number of buildings approved in panel B. For context, the average number of buildings approved are 21.5 per ten thousand residents in a year. At the low end of the interval would be 18.6 buildings approved per ten thousand and 35.48 per ten thousand at the high end. While the potential reduction in panel A is concerning, the effect may be driven by the *types* of building being permitted. Change in the number of buildings approved is overwhelmingly influenced by the number of single-family homes, so the number of units could potentially be unchanged if there is a shift from single-family homes to larger developments. I test for these kinds of changes in columns 2 and 3. Column 2 shows the total number of units permitted experiences the same patterns as column 1 in both panels. Column 3 shows changes to the total number of units for approved buildings slated to be constructed with at least three units. After a breakthrough election, the number of units in large developments declines 33 log points relative to control cities. Panel B shows no change to larger developments after breakthrough elections. The last column shows breakthrough elections lead to a 10 log point increase in the value of the structures receiving permits in the entire sample. Restricting the observations to the mayor–council events, we observe large and statistically significant gains in the total assessed value of permitted developments. Because property taxes are a function of assessment values, a lenient development policy may be a politically viable way to increase or maintain revenues which I explore in the next section.

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<sup>44</sup>A larger discussion of the planning and construction process can be found in Einstein, Glick, and Palmer (2020).



	Log Buildings Per Capita (1)	Log Units Per Capita (2)	Log Large Development Units Per Capita (3)	Log Value Per Capita (4)
<i>Panel A: Entire Sample</i>				
Black Mayor	-0.165 (0.173)	-0.123 (0.167)	-0.335 (0.296)	0.101 (0.184)
<i>Panel B: Mayor-Council Events</i>				
Black Mayor	0.186 (0.156)	0.229 (0.215)	-0.046 (0.429)	0.533** (0.191)
Observations	5,762	5,762	5,762	5,762

*Notes:* Coefficients are city-level, stacked least squares estimates. The dependent variables are listed at the top of each column. Buildings refer to the total number of building permits issued, and units are the total number of units added across all building sizes. Large developments are buildings approved to be constructed with at least 3 units. The second panel only retains stacks where the mayor serves as chief executive. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of control cities with 1990 populations greater than 100,000 whose Black populations are above 10 percent that either observed an interracial election or elected a Black candidate more than eight years afterward. Each breakthrough city forms a panel with the control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. Standard errors, printed below point estimates and are clustered at the breakthrough election level. Data come from the U.S. Census Bureau's Building Permits Survey at the place level from 1994 to 2018.

Table 2.14: Relationship Between Black Mayors And Residential Development

### 2.8.2.3 Changes To Taxes And Long Term Capital Projects

Discriminatory investment patterns in local public goods have left Black neighborhoods deprived of local public goods relative to White neighborhoods (Trounstine 2018). Segregation is high in sample cities as measured by the dissimilarity index ( $D \approx 60$ ) which measures the percentage of Black Americans that would need to move to another census tract in order for the census tract distribution of Black and White Americans to reflect the city-level distribution of Black and White Americans. Trounstine (2018) (p. 160-165) specifically describes race-specific disparities in sewer maintenance systems. She finds highly segregated cities spend \$200 per capita less on sewer maintenance than more integrated cities. More generally, improvements in local infrastructure potentially increase returns to local economic development through decreases in the costs from unforeseen disruptions or delays. In Table 2.15, I explore the effect of the election of Black candidates on long-term capital expenditure proxied by the amount of debt per resident the municipality holds and the effect on other typical revenue sources municipalities turn to fund governments.

	Log Debt Per Capita	Log Property Taxes Per Capita	Log Sales Taxes Per Capita	Log Total Taxes Per Capita	Log Revenue Per Capita
Black Mayor	0.096 (0.191)	-0.079 ** (0.036)	0.256 (0.381)	-0.046 (0.029)	0.059 (0.066)
N	5911	5911	3222	5291	5279

\*\*\* p < 0.01; \*\* p < 0.05; \* p < 0.1.

Table 2.15: Relationship Between Black Mayors And Changes To Public Finances

Coefficients are city-level, stacked least squares estimates. The dependent variables are listed at the top of each column. Sample includes 18 cities that elected a Black candidate for the first time between 2000 and 2010 inclusive and the set of control cities with 1990 populations greater than 100,00 whose Black populations are above 10 percent that either observed an interracial election or elected a Black candidate more than eight years afterward. Each breakthrough city forms a panel with the control cities, and the panels are stacked together in event time. The event window is six years before the election of a breakthrough candidate and the eight years following the election. Standard errors, printed below point estimates, are clustered at the breakthrough election level. Data come from the Annual Survey of State And Local Governments from 1994 to 2018.

### 2.8.2.4 Other Potential Explanations: Preferential Tax Treatment

The results thus far support the design choice of searching for effects at the sub-municipal level. However, some policies may be localized but reported at the city level restricting the researcher’s ability to detect an effect. Because there may be persistent differences in employment and business levels across communities within the same city, one localized policy, in particular, to increase economic development is to offer tax inducements to businesses for relocation. This could also be cash grants or customized job training for firms (Bartik 2020). For example, New Orleans and the Morial administration persuaded SFE Technologies, a computer parts manufacturer, to relocate from California and open a \$9M plant by exempting the manufacturer from a suite of taxes for 5 years. The New Orleans government, in return, required the firm to hire 35 percent of its employees from within the Ninth Ward, a community in New Orleans whose Black population exceeds 90 percent (Piliawsky 1985, 13). Unfortunately, no such centralized information exists to analyze the effect of tax inducements and granted tax relief on local economic development.

## Chapter 3

### Black Mayors & Race-Specific Arrests

Black communities tend to face a dual public safety problem. Black families are hit hard by high levels of perceived offenses committed by Black men destabilizing familial structure,<sup>45</sup> and policies that target high crime areas increase civilian interaction with police forces whose heritage is owed to historical institutions of social control. Because of victimization both by criminals and the police themselves, 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.

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 Hopkins and McCabe (2012), McCrary (2007), and Rae (2003)). 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 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 policing) and appoint police chiefs. This creates a layer of accountability between law enforcement and the voting public which may lead to divergence from the politically desirable outcome.

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.<sup>46</sup> 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 mayors are high-information (in the political economy sense), highly polarizing events. This works to the disadvantage of traditional RD designs which require plausible 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 towards particular offenses or incarceration change.

With these challenges in mind and taking advantage of recent advancements in difference-in-differences (DD) estimation (Callaway and Sant’Anna (2021a)), this paper uses a DD design leveraging the timing of when Black candidates were elected for mayor for the first time to show that Black mayors have an impact on the diversity of the police force and this translates noisily into reductions in discriminatory and violent arrests for Black Americans. I compare the set of cities that elect a Black candidate in the same year against a set of cities that had not elected a Black candidate at the time period in question.<sup>47</sup>

I begin by documenting that after a Black election, the share of expenditure devoted to police protection is reduced by .64 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 additional expenditure being devoted to other sources. I further observe no changes in the number of officers after a breakthrough election but this masks changes

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<sup>45</sup>What the data does (and doesn’t) say about crime in the United States. I say “perceived offenses” because it is true that Black men are arrested at higher rates than other groups, but much crime goes unreported, and higher Black arrests could be an artifact of higher levels of policing relative to other groups.

<sup>46</sup>see <https://www.nytimes.com/1992/09/20/nyregion/dinkins-and-the-police-a-campaign-issue.html?searchResultPosition=25>

<sup>47</sup>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 had not elected a Black candidate by 1991 for the year 1991, and repeat that same procedure for every year in the data.

to the number of Black and female officers once a Black candidate assumes office. Using data that tabulates officer counts by race and by sex (but not their intersection), I show that breakthrough elections increase the number of Black officers by 1.795 per ten thousand residents and the number of female officers by 1.616 per ten thousand residents. Taken at the average population for treatment cities, this implies an increase of 57 Black officers and 54 female 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.

With this in hand, I turn to examine if Black mayoralties translate to differences in arrests 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 both absolutely and relative to White Americans when comparing the same offense, though most estimates are statistically imprecise. This relationship holds when restricting the data to cities that have elected a Black candidate at any point, examining only the cities where the mayor holds executive power, and by comparing cities in the same state where one has elected a Black candidate and the others have not. When estimating changes to the number of offenses known to police a similar pattern arises whereby across most crime categories the number of offenses falls, but estimates are imprecise. Interestingly, the number of officers assaulted declines suggesting changes to department composition lead to fewer violent interactions with law enforcement. While results are heterogeneous according to timing group supporting use of the estimation procedure, overall there are not different effects by time period with a minor exception for violent Black arrests where timing group estimates are uniformly negative. This makes sense given high levels of violent crime at the time the presumptive candidates would assume office.

I conclude by determining the relationship between size of the police force or diversity of the force on petty and violent arrests. Petty arrests, otherwise called discriminatory or quality of life arrests, are a set of offenses where one would expect officers to exercise considerable discretion in the arrest decision. I estimate the mayor-induced changes to the total number of race-specific arrests and the total number of officers, then to Black and female officers for each treatment city in the data. I then fit a line through the results and find that increasing the number of officers increases the number of petty arrests and decreases the number of violent arrests similar to previous work. I find the reversed relationship between increasing Black officers and the number of petty arrests suggesting a dimension of officer discrimination. An increase of one Black officer leads to a reduction of 1.1 violent crimes. When combined with the Black officer increase this means Black officers were responsible for preventing 63 violent crimes on average. An increase in female officers revealed a similar pattern to that of Black officers though estimates were noisier.

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 driven 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 reducing discriminatory arrests then this would be a negative feature. Second, if politicians are able to influence behavior in the sense that White officers also limit discriminatory behavior, this may also improve police–community relations. Kapustin, Neumann, and Ludwig (2022) show that changes to police leadership in Chicago lead to sizeable effects on policing outcomes though in this context given the management change happens elsewhere in the municipality, bureaucracy might limit its efficacy. Other work on management quality, looks at the mayor specifically. Colombo and Tojerow (2020) find improving mayoral accountability decreases crime, whereas Akhtari, Moreira, and Trucco (2022) find that political influences can reduce the quality of public good provision through the link of political and bureaucratic turnover.

A second literature deals directly with police composition and effectiveness. Chalfin et al. (2021) find that increases to police manpower cause a decrease in arrest 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 changes to department diversity in Black communities have the ability return the double dividend consistent with the findings of Ba et al. (2021), Harvey and Mattia (2021), and Miller and Segal (2019). Donohue III 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 Whites 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) finds 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 their work by determining if these changes lead to better outcomes within the population. Other work notes particular cases where Black mayors came to power and often finds similar conclusions, always making note of the heightened criminal justice aspect of Black elections (see Reft (2008), George (2004), and R. P. 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 mayor in the modern period<sup>48</sup>, larger Black populations from the 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 3.2 I provide information about the powers available to mayors to change policing outcomes and why changing departmental representation is a primary focus; in Section 3.3 I describe the data. Section 3.4 highlights my empirical approach. Section 3.5 describes the results, and Section 3.6 I give some concluding remarks.

## 3.2 Background

### 3.2.1 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 find that Black arrests rates fell in counties covered by the Voting Rights Act (VRA). In their model, the VRA shock to voter preferences driven by the Black population size induces the election of chief law enforcement officers (CLEOs) whose policies improve the treatment of Black residents. Importantly, this is driven by elected CLEOs and not appointed ones in their context. While they note that electing policymakers like a mayor is in effect a selection a bundle of goods, we may still expect to see changes in treatment of Black residents if the elected candidate is Black.

In many counties and smaller jurisdictions, the sheriff is the most powerful elected person which is 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."<sup>49</sup> However, all mayors are not created equally. The enterprising reader can sleuth through the Vallejo (CA) municipal 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."<sup>50</sup> Thus some mayors are powerful by rules and others only have the potential to be powerful through influence.<sup>51</sup>

<sup>48</sup>as opposed to Reconstruction, see Logan (2020) for more

<sup>49</sup>[https://codelibrary.amlegal.com/codes/los\\_angeles/latest/laac/0-0-0-571](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.

<sup>50</sup>[https://library.municode.com/ca/vallejo/codes/municipal\\_code?nodeId=CH\\_ARTIIIMACO\\_S318PODUMA](https://library.municode.com/ca/vallejo/codes/municipal_code?nodeId=CH_ARTIIIMACO_S318PODUMA)

<sup>51</sup>In situations of low information, the mayor may be the only recognizable public figure, even with similar powers to other council members, whereby they have the ability to influence downstream elections.

The rest of this section will focus on mayors that 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 the mayor often serve as roadblocks for implementing their most preferred policy. Returning to Los Angeles as an example, supposing the mayor wants to appoint a CLEO who aligns with their policing ideology, the mayor can appoint members to the Board of Police Commissioners, who would then recommend candidates to the mayor for appointment, who then offers a recommendation to the city council. A similar stepwise process is required for the removal of a police chief which can be thwarted by a two-thirds vote by 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.<sup>52</sup>

### 3.2.2 Police Enforcement In Black Communities

Improving diversity of the police force can lead to improvements in community relations (e.g. see Miller and Segal (2019), Harvey and Mattia (2021), and Ba et al. (2021)). Figure 3.21 shows changes to the Black and female officer shares beginning in 1987 using the Law Enforcement Management and Administrative Survey (LEMAS) for cities in 1970 with population at least 75,000 and a 4 percent Black population. 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 which began decades earlier with the explicit goal of increasing diversity to prevent riots and improving police–community relations (Cox, Cunningham, and Ortega 2022). Settlement of affirmative action litigation 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 their police department supervisory representation which was 2.5 percent at the time (Piliawsky 1985).

An important point is that because the mayors themselves are Black, they are or have been subject to police-specific discrimination.<sup>53</sup> This means they have (in general, stated) interest in changing department interest to improve their constituent's satisfaction and by extension decreased or less conflictive officer interactions improve long-term outcomes for residents. Aizer and Jr. (2015) find that incarcerated juveniles are more likely to become recidivist and less likely to complete high school considerably limiting future earnings.

## 3.3 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 variation in election timing and geography. I begin by restricting my inquiry to cities whose populations were greater than 75,000 in 1970 and have a Black population of at least 4 percent in 1970. This leaves me with 66 cities that elected a Black candidate and 92 cities that did not elect a Black candidate but meet the population threshold. Figure 3.22 illustrates the timing of elections of Black candidates grouped into decadal intervals. Both within and across decade we see variation by geography and Black population. For instance in the 1970s, we observe cities with Black populations less than a quarter (e.g. Los Angeles and Raleigh), and these cities cover all four census regions. Bolded boxes indicate being within or after the term of a Black mayor.<sup>54</sup>

I rely on a collection of sources to conduct my analysis. Information on elections mainly comes from Vogl (2014) which is the most comprehensive source of Black mayors of medium- and large-sized 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

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<sup>52</sup>Presumably, the mayor would have been able to install all of the members to the Police Board of Commissioners leaving minimal friction in decision making.

<sup>53</sup>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 began in 1991.

<sup>54</sup>A map of treatment and comparison can be found in the Appendix.

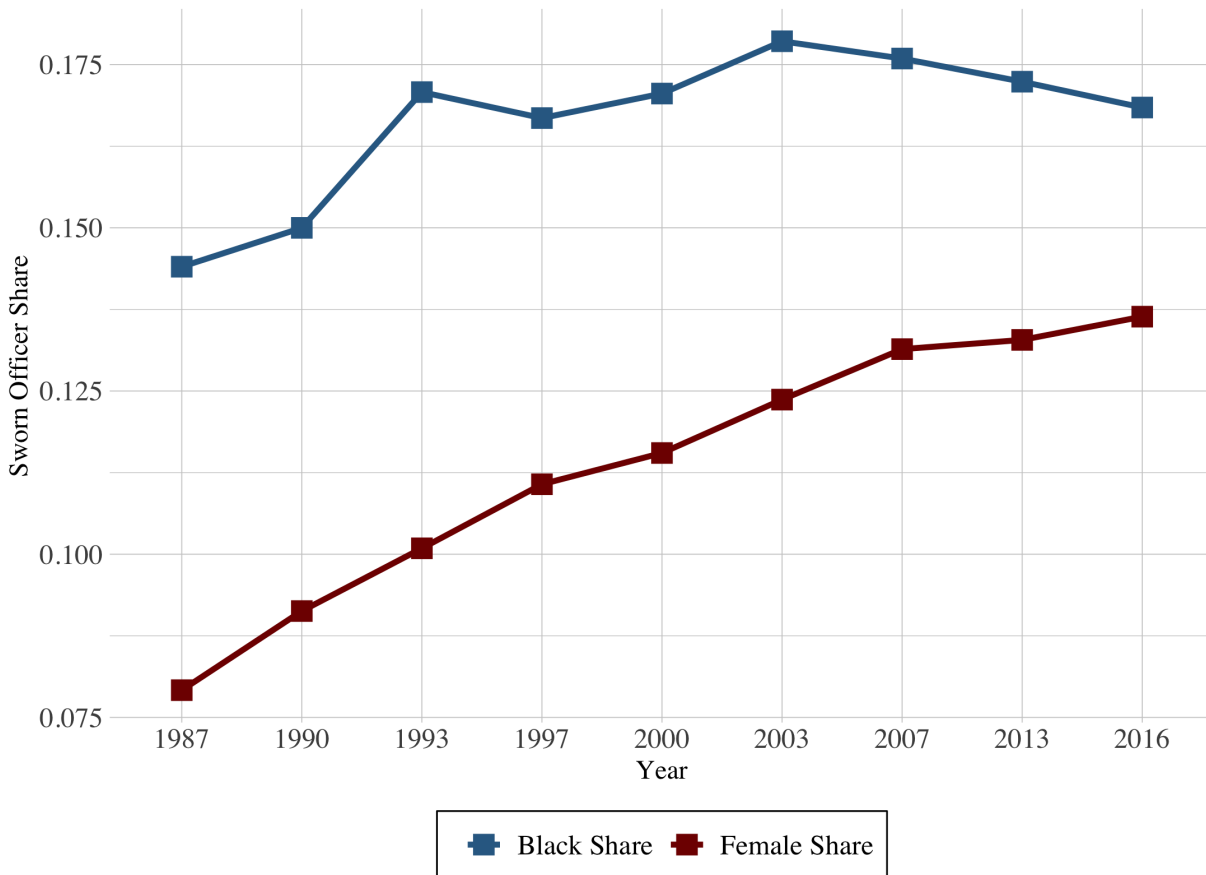


Figure 3.21: Share of Black and Female Officers, 1987-2016

Figure displays average share of Black and female officers from the Law Enforcement Management and Administrative Survey. Each data point corresponds to a survey year.

least 4 percent. In addition, it includes both candidates' race, their vote shares, and party affiliation. Much of that data are derived from Ferreira and Gyourko (2009) who mailed surveys to all cities with a population greater than 25,000 who 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 which I then confirm candidate racial identity through web search or explicit mention. In other words, the photograph assists in determining candidate race but the ultimate factor is if the candidate or external source 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 near one of being surveyed. Reporting on the number of police officers begins in 1977, so data coverage is from 1977 to 2018. ASPEP was not collected in 1996 due to a change in reporting date so data for that year are interpolated from the adjacent years.

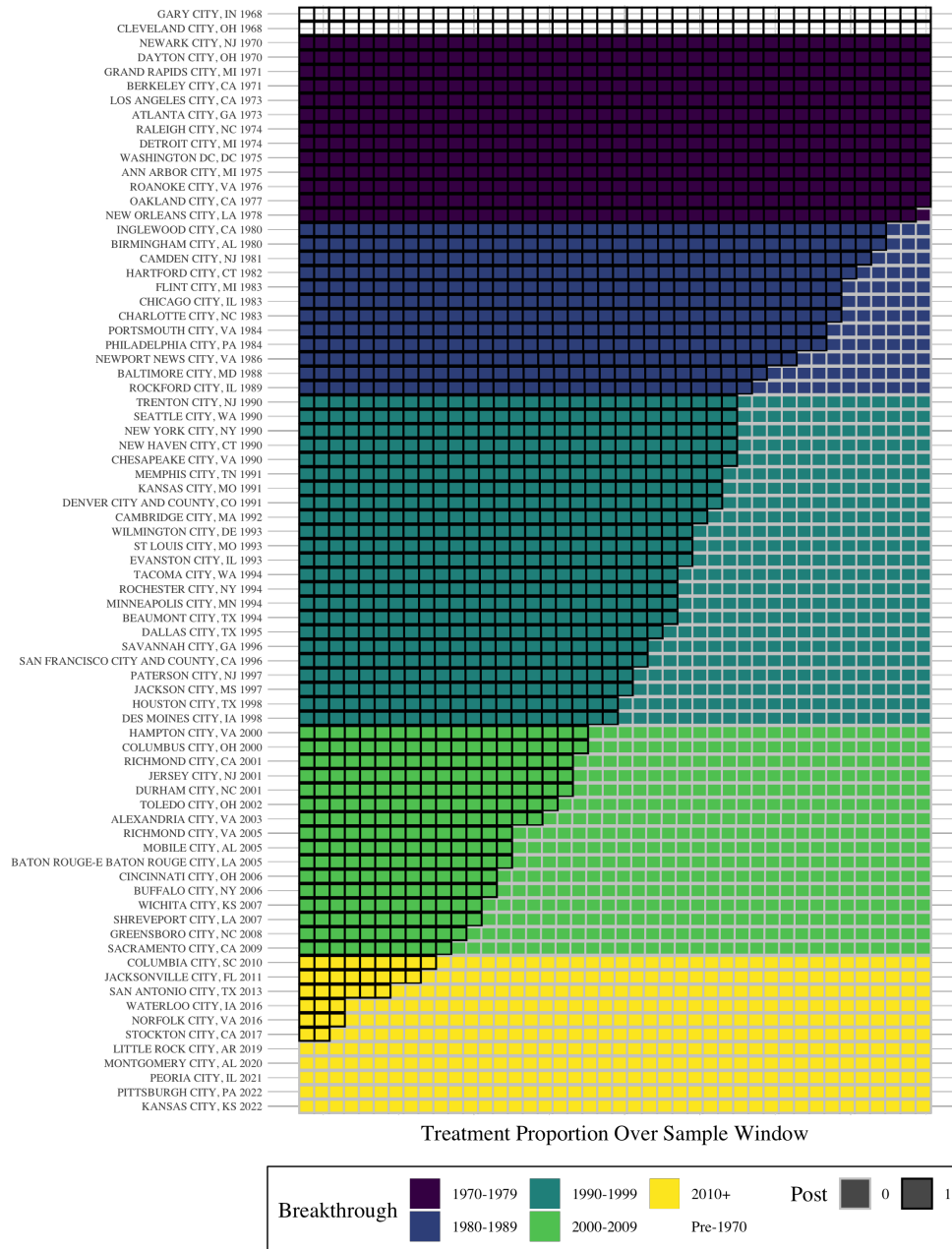


Figure 3.22: Breakthrough Election Timing: 1977-2018

Figure displays cities in the sample that elected a Black candidate. Cities are categorized by the decade they elect their first Black candidate and then arranged by year. Bolded boxes correspond to the length of the post period. Elections from Vogl (2014), Ferreira & Gyourko (2009), and collected sources.



My outcomes are arrests by race and offenses known to law enforcement. I use Jacob Kaplan's (Kaplan 2020) concatenated files for the Uniform Crime Reports (UCR) arrests by age, sex, and race, 1977-2018 and the UCR program data for offenses known and clearances by arrest. The data contain arrests 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 arrests variables which I denote arrests for violent crimes and arrests for petty crimes. Violent arrests are the yearly sum of aggravated assault, murder, rape, and robbery. Petty arrests are the yearly sum of suspicion, vagrancy, vandalism, gambling, prostitution, liquor, curfew, loitering, and drunkenness.<sup>55</sup> I only consider municipal police departments and exclude county, state, and other agencies<sup>56</sup> who 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. 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. Most agencies submit crime reports to a state UCR program.<sup>57</sup> The state UCR program forward 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 agencies. Similar to the arrests data, I restrict my attention to municipal police departments and offenses stats beginning in 1977. The offense data contain reports for "index" crimes which are sum of violent crimes—homicide, forcible rape, robbery, and aggravated assault—or property crimes—arson, burglary, larceny-theft, motor vehicle theft. Because it's the sum of crimes, these indices are heavily influenced by the crimes that are most commonly committed in each reporting category (robbery for violent and larceny for property). Like the arrests data, offense data are reported using the hierarchy rule. The offense 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. e.g. if an assailant murders an individual during a robbery than only the murder is recorded. In 85 percent of cases only one crime is committed. Also, while much effort is taken to confirm the accuracy and consistency of reporting through quality assurance reviews, the data still contain some discrepancies and missing data. First, I look for instances of within municipality "jumps" that are not explained by (reported) 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 which cannot be explained by changes in reporting patterns but appears to be related to the method in reporting "other" arrests.<sup>58</sup> 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 backwards/forwards imputation and interpolation I fill the missing data.

The combined dataset yields 153 cities consisting of 58 cities that elect a Black candidate after 1977 containing data on city arrests, offenses, taxes, revenue, expenditure, and 1970 demographics. Those cities that elect a Black candidate before are excluded.

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<sup>55</sup>Chalfin et al. (2021) construct a similar "quality of life" offenses including many similar crimes. My initial construction notably excludes drug possession. I do this because we may believe a Black election should induce fewer petty arrests, but there may be substitution at the possession-sale threshold whereby we may could conclude that possession arrests for Black residents increases in the wake of a breakthrough election. Future work will test whether its inclusion meaningfully alters the initial results.

<sup>56</sup>e.g. airline and university police

<sup>57</sup>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.

<sup>58</sup>This feature is in general exclusive to cities in Alabama and Florida indicating actions taken by their state reporting agencies.

### 3.4 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 and where a Black candidate assumes office. I aim to compare cities that directly elected a Black candidate for the first time in a specific year relative to cities that did not. Because Black candidates are not randomly assigned to cities, it is important to assess race-specific arrests patterns before a candidate takes office. A primary concern is that patterns for Black or White arrests are changing before the election of a breakthrough Black candidate because the breakthrough candidate may sit on the council that enacts legislation or voter preferences are shifting, and current policymakers are responding to the shift. Secondary and tertiary concerns are 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 by example, one might expect the political opposition and potential effect size to be larger in 1984 Philadelphia (Wilson Goode) than in 2006 Cincinnati (Mark Mallory).<sup>59</sup> However, numerous scholars have identified these issues in difference-in-differences settings and proposed solutions. (Callaway and Sant’Anna 2021a; deChaisemartin and D’Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021; Borusyak and Jaravel 2018)

I follow Callaway and Sant’Anna (2021a) and leverage variation in the timing of breakthrough Black elections to estimate group-time average treatment effects. My data run from 1977 to 2018 (i.e.  $t = 1977, 1978, \dots, 2018$ ). Across the sample period, 58 Black candidates assume office in 32 of these periods beginning in 1978 which I index  $g = 1978, 1980, \dots, 2017$ . I estimate the group-time average treatment effects 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 policing outcomes by aggregating these different treatment effects together.

Under a parallel trends assumption, the group-time average treatment effect for each group  $g$  at time  $t$  with no anticipation is

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

where  $Y_t$  is the average of a policing outcome at time  $t$ ,  $G_g$  is a dummy variable equal to 1 for units in timing group  $g$ , and  $D_t$  is a dummy equal to zero for units not-yet-treated at time  $t$ . I estimate each  $ATT(g, t)$  with its sample analog,  $\widehat{ATT}(g, t)$ , via outcome regression.<sup>60</sup> This process yields many  $\widehat{ATT}(g, t)$  most of which are identified off of relatively few observations, so instead of interpreting each group-time ATT, I aggregate these effects together into simpler and more intuitive effects.

First, I report the overall treatment effect parameter by taking a weighted average of all  $\widehat{ATT}(g, t)$  such that  $g \leq t$  where the weights are based on timing group size defined as

$$\hat{\theta}^O = \frac{1}{\kappa} \sum_{g=1978}^{2017} \sum_{t=1978}^{2018} \mathbf{1}\{g \leq t\} \widehat{ATT}(g, t) P(G = g) \quad (3.8)$$

where  $\kappa = \sum_{g=1978}^{2017} \sum_{t=1978}^{2018} \mathbf{1}\{g \leq t\} P(G = g)$ . Upon inspection the reader may notice this parameter puts more weight on earlier treated units which may or may not be desirable. Readers, instead, may want to know what the effect of being exposed to a black mayor after a certain length of time across units is, so I examine how the effect of Black mayors on policing outcomes evolves over time with a 10-year balanced, event-study specification.<sup>61</sup> For each time

<sup>59</sup>Admittedly, some of us may not.

<sup>60</sup>In effect this means partitioning the data to time periods  $t$  and  $g - 1$  and retaining the focal timing group and the not-yet-treated-units. Then the regression  $Y_{i,t} = \tau_1 TREAT_i + \tau_2 POST_t + \tau_3 TREAT_i \times POST_t + \varepsilon_{it}$  is estimated. Estimation procedures done using `did` from Callaway and Sant’Anna (2021b).

<sup>61</sup>I do this to avoid any compositional changes similar to traditional event studies. Using the entire sample of elections does not materially alter the results.

relative to treatment  $e$ , I estimate the effect of treatment for units that have been treated for  $e$  periods with  $\hat{\theta}_D(e)$ ,

$$\hat{\theta}_{es}(e) = \sum_{g=1978}^{2008} \sum_{t=1977}^{2018} \mathbf{1}\{t-g=e\} \widehat{ATT}(g,t) P(G=g|t-g=e), \quad (3.9)$$

while also reporting the post-exposure average  $\hat{\theta}_{es}$  which provides a summary measure of the effect of Black mayors on particular outcomes in the 10-year period after assuming office. While similar to traditional event studies, the estimands in the post period are “long differences” of  $g+e$  and  $g-1$ , so do not suffer from bias coming from potential treatment effect heterogeneity. The pre-period estimation is also different which is the “short difference” or the year-to-year difference of  $\widehat{ATT}(g,t)$  in the pre-period.

Lastly, it may be of interest to show the group-specific average treatment effects in order to document differences across periods. In particular, we may be interested in observing if treatment effects are larger for civil rights era mayors than those more recently elected, so I aggregate treatment effects according to timing group,

$$\hat{\theta}_{sel}(\tilde{g}) = \frac{1}{2018 - \tilde{g} + 1} \sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t) \quad (3.10)$$

for each  $\tilde{g} = 1978, 1980, \dots, 2017$  which is the average effect for cities electing a Black candidate in  $\tilde{g}$  in the post period. We might expect given trends in crime and period-specific events like the crack epidemic of the 1980s may lead to differences in arrest rates, so I report the group-specific treatment effects for arrests in the main text. Results for other outcomes can be found in the appendix.

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. Standard errors are constructed via multiplier bootstrap proposed by Callaway and Sant’Anna (2021a).

## 3.5 Results

### 3.5.1 Descriptive Stats

Figure 3.23 depicts time trends of the outcomes of interest by race (blue for Black outcomes and red for White outcomes) and treatment status (dashed for electing a Black candidate 1970- and solid for non-electors) on the left and a table of 1970 city characteristics separated by treatment status on the right. 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. Outcomes used in the analysis are defined similarly. There is a clear level difference between Black and White arrest rates in both petty and violent crimes. Petty arrests across race and treatment are declining across the sample period perhaps reflecting rises in average income or employment. Important for the design when comparing Black (White) arrest rates across treatment status, there does not appear to be differences in the trends in arrest rates across the sample period. In fact there are minimal level differences within race suggesting minimal differences in police behavior across cities.

Trends in violent crime reveal a perhaps more interesting pattern. First, Black residents are more than twice as likely to be arrested for violent crimes than White residents. Unlike petty arrest, 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.

The table displays city characteristics in 1970, and we for the most part observe no differences. As should be

expected, the Black share is higher in cities that elect Black candidates, but this doesn't seem to have an effect on race-specific arrest trends as states. A rigorous inspection of this will be done in the next few sections. Average population is higher in Black mayor cities because of the inclusion of the three largest cities in the United States. Finally, we observe that there are not substantial differences in municipal revenues and expenditure, and cities tend to spend the same share of expenditure on policing in 1970.

In the next section, I begin by describing changes to police expenditure or it's priority in the budget before moving towards estimating changes in department racial and demographic composition which may provide belief that one should observe changes to arrests figures. With that in hand, I then move to estimating changes in race-specific arrest patterns and ask if these patterns differ across timing group.

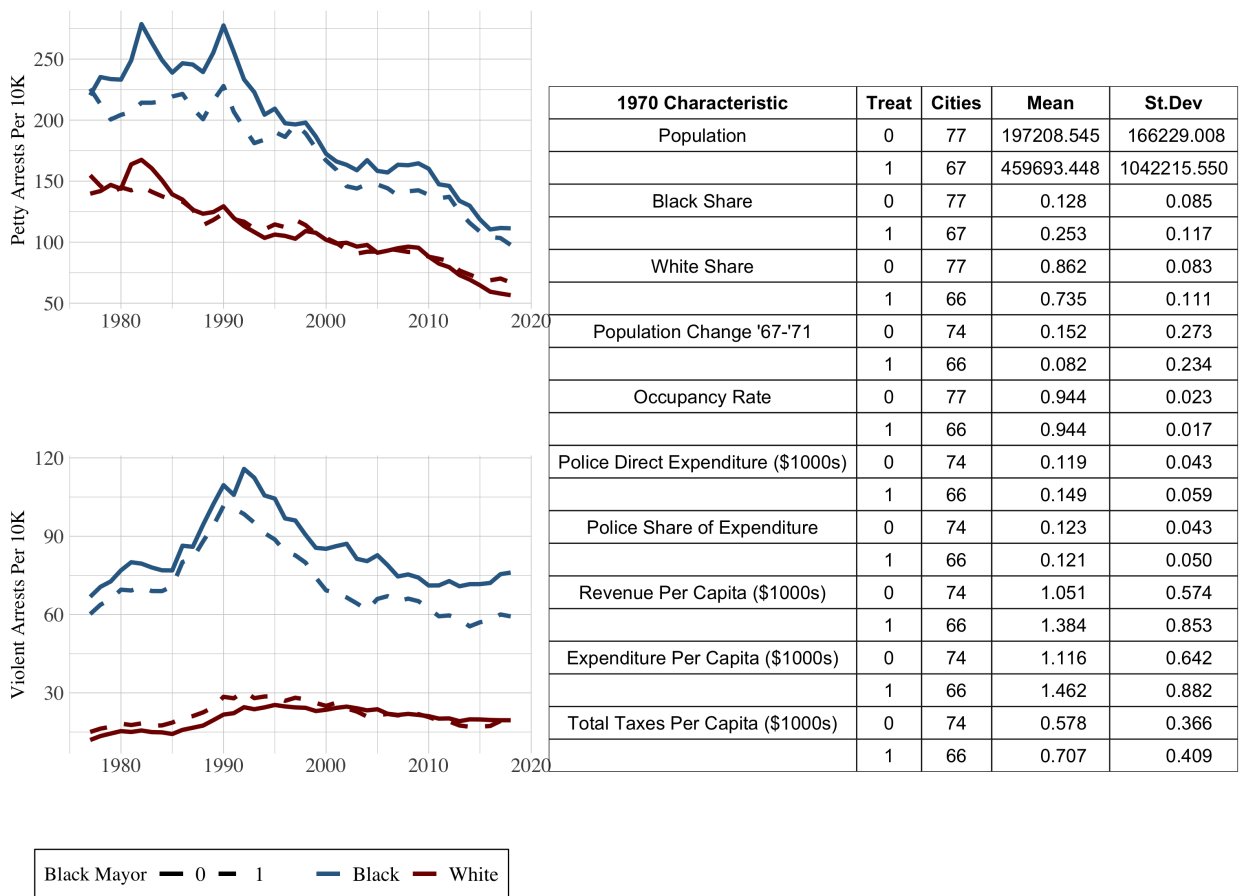


Figure 3.23: Trends in Crime: 1977-2018 & 1970 City-Level Characteristics

Figure displays trends in violent and petty crimes separated by treatment status and race of offender on the left. Violent crimes are the annual sum of murder, rape, robbery, and aggravated assault. Petty crimes are the annual sum of suspicion, vagrancy, vandalism, gambling, prostitution, liquor, curfew, loitering, and drunkenness. Results are per 10K residents of offender's race. The table on the left shows 1970 city-level characteristics separated by treatment status.

### 3.5.2 Changes to Spending & Department Size

Figure 3.24 displays results from the estimation of Equation 3.9 on a quartet of public finance outcomes using 90-percent uniform confidence bands. The upper-left quadrant of each panel contains the average effect of exposure in the 10-year period after election. Panel A shows changes to the total share of government employment in policing. Immediately, there is little to no change, but by the eighth year or the average duration of a breakthrough mayor’s administration police employment falls one percentage point relative to the year before election; however this decline is ultimately reversed, and the change is roughly nil. Panel B describes the change to police expenditure as a share of total expenditure. The share of spending devoted to police protection decreases over the window and by the tenth year, the share is 1 p.p. less than in the year before assumption indicating a shift in priority within the budget. Notably, while the share of expenditure declines, expenditure per capita does not decline which is evident in Panel C. The average change over the period is 0.63 with the confidence bands limiting the change to minimal in either direction suggesting that the additional dollar of government expenditure went elsewhere. Panel D moves from police expenditure to the change in officers per 1000 residents. Once again, the change in the post-period is minimal implying Black candidates do not look to increase the size of the police force on average.

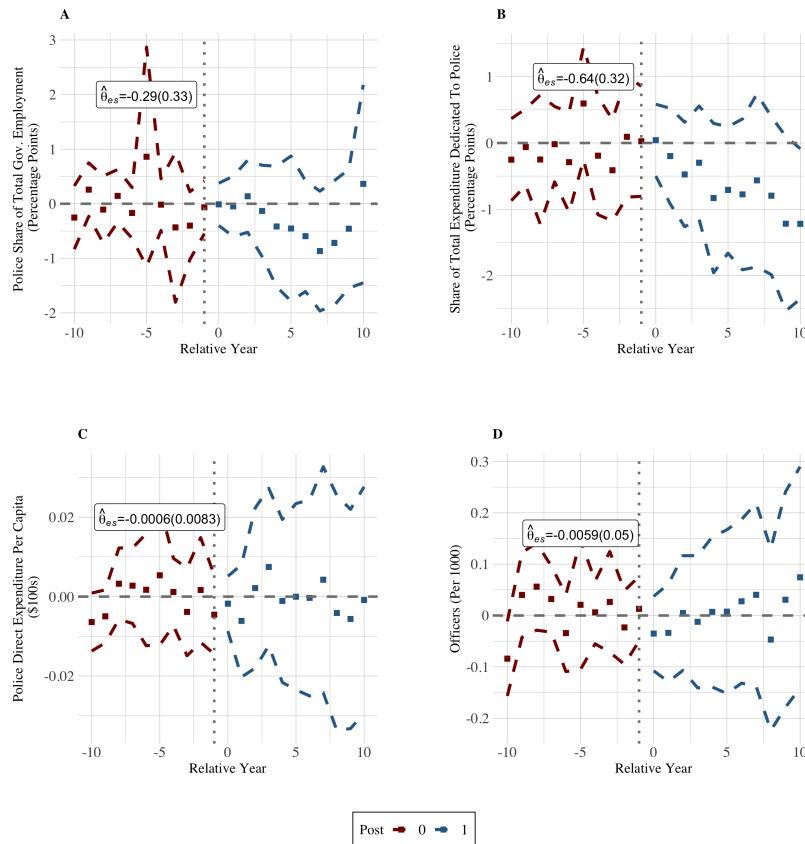


Figure 3.24: Impact of Black Election On Share of Government Devoted To Policing and No. Of Officers

Figure displays results from Equation 3.9 of the relationship between breakthrough mayors and public finance-related outcomes. Column A shows the change in the share of police employment to total government employment. Column B shows the share of direct police expenditure as a share of total current expenditure. Column C shows changes in police direct expenditure per capita, and column D shows the change in sworn officers per 1000 residents. 90-percent uniform confidence bands displayed.

### 3.5.3 Are There More Black Officers? Evidence From The Law Enforcement Management And Administrative Survey (1987-2016)

While the size of the police department does not change, the racial and gender composition of the department might. In the 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 racially discriminant behavior.

Using LEMAS, I report estimates of Equation 3.8 in Tables 3.16 and 3.17 which show changes to the shares of Black or female officers and their per capita change respectively<sup>62</sup> These estimates will capture changes in department that elect a Black candidate after 1987 as that is the first year the survey is taken. We might assume that we are capturing a lower estimate for changes to Black officers (see Fig. 3.23) because much of the change in Black officers seems to come by 1993; however, if this is a policy area common to all Black mayors, we should presume to observe an 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. While we observe an increase in the share of Black officers of 2.2 p.p., this estimate is statistically imprecise. A similar pattern holds in the executive governments with the point estimates 0.5 p.p. which should be expected. The share of White officers declines 3 p.p. with the range implying small gains to large declines in the share of White officers. The share of Hispanic officers increase and the share of male officers declines though neither estimate is significant at traditional levels. Interestingly in both specifications, the share of female officers increases and the increase is large relative to its 1987 average. Using the increase caused by the election of a chief executive mayor, the share of female officers increased by nearly 40 percent of its 1987 average. While not as large as the female effect, the Black officer effect is still large in comparison to its 1987 average (~14%).

The number of Black and female officers increase after a breakthrough election, and the estimates are larger and more precise when restricting estimation to chief executive treatments. Evaluating the chief executive estimates at the population average implies a total increase of 57 Black officers and 54 female officers which one should assume some officers occupy both categories. The average number of both male and White officers goes up in the post period but the confidence intervals do not rule out relatively large declines to the number of male or White officers; even with positive point estimates, the results are consistent with the share of both male and White officers declining in the wake of a breakthrough election. I now turn to analyze whether Black elections lead to changes in arrest patterns and if there might be a pattern between the results in this section and the next.

### 3.5.4 Are Black Americans Arrested Less & For What Kinds Of Offenses

Figure 3.25 displays estimates of Equation 3.8 on race-specific arrest outcomes in the top panel and offenses known to law enforcement in the bottom panel. Two observations are immediate: estimates across all arrests are imprecise and the reduction in Black point estimate is consistently larger than the White point estimate for the same crime. The largest race-specific difference is in the petty crimes category as defined previously where there is no change to White arrests and an approximate 20 arrest reduction for Black arrests. The bounds on the Black estimate are however large though implying a possible range of 20 additional arrests to 60 fewer arrests at 95-percent confidence. Violent arrests decline for both Black and White persons, though in both cases we cannot rule out small increases in the number of arrests.

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 for cannabis, and on the other if there is an element of manipulation at the possession–sale threshold then

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<sup>62</sup>Event study-like estimation is not possible for this sample as LEMAS is not a yearly survey, so we cannot make group-exposure length comparisons for each treatment cohort for each period of exposure.

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 but the estimate suggests no changes to the number of arrests for sale and an imprecise reduction for possession. The last two estimates are for minor theft, the sum of larceny and burglary, and arrests made for dui. Across both categories, the Black estimate is larger but the results are imprecise.

**Panel A: Full Sample**

	Black Share	White Share	Hispanic Share	Male Share	Female Share
BM × Post	0.022 (0.022)	-0.030 (0.024)	0.016 (0.016)	-0.015 (0.009)	0.026*** (0.008)
Cities	153	153	153	153	153

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

**Panel B: Chief Executive Cities**

	Black Share	White Share	Hispanic Share	Male Share	Female Share
BM × Post	0.027 (0.025)	-0.034 (0.028)	0.023 (0.019)	-0.010 (0.011)	0.030*** (0.010)
Cities	119	119	119	119	119

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

Table 3.16: Changes In Department Composition After Black Election

Table shows estimates of Equation 3.8 for the relationship between Black mayors and police department composition by race, ethnicity, and gender. Panel A shows the full sample, all treated cities after 1977 and cities that had a Black population over 4 percent in 1970, and Panel B excludes treatment cities with council–manager governments. Multiplier bootstrap in parentheses.

**Panel A: Full Sample**

	Black Per Capita	White Per Capita	Male Per Capita	Female Per Capita
BM × Post	1.240* (0.722)	0.601 (2.592)	2.010 (3.280)	1.178** (0.522)
Cities	142	142	142	142

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

**Panel B: Chief Executive Cities**

	Black Per Capita	White Per Capita	Male Per Capita	Female Per Capita
BM × Post	1.795** (0.844)	1.132 (3.594)	3.212 (4.327)	1.616*** (0.615)
Cities	108	108	108	108

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

Table 3.17: Changes In Department Size After Black Election

Table shows estimates of Equation 3.8 for the relationship between Black mayors and police department size by race, ethnicity, and gender. Panel A shows the full sample, all treated cities after 1977 and cities that had a Black population over 4 percent in 1970, and Panel B excludes treatment cities with council–manager governments. Multiplier bootstrap in parentheses.

In general the number of arrests decline. This could be the result of crime levels declining via some method of deterrence or it could be caused because Black or female cops make fewer arrests (without commenting upon whether or not this is a net positive). The second panel shows qualitatively similar results to the first panel in that it suggests crimes tend to decline but the pattern is in no way uniform. Interestingly, there is a statistically significant decline in

the number of officers assaulted which may be a result of department composition changes in the post period. This suggests a method to reduce violent interactions with law enforcement by having department composition reflect city composition.

Figure 3.26 restricts attention to petty and violent crimes and examines the change in a 10-year event window. I display the post-event average in the top-left quadrant of each. Violent arrests for both Black and White persons declined in the post-period, but arrest figures display more variability with the Black estimate. The same variability is present in the petty arrest figure. The point estimate for petty Black arrests is 6 times larger but overlaps the petty White arrest interval considerably.

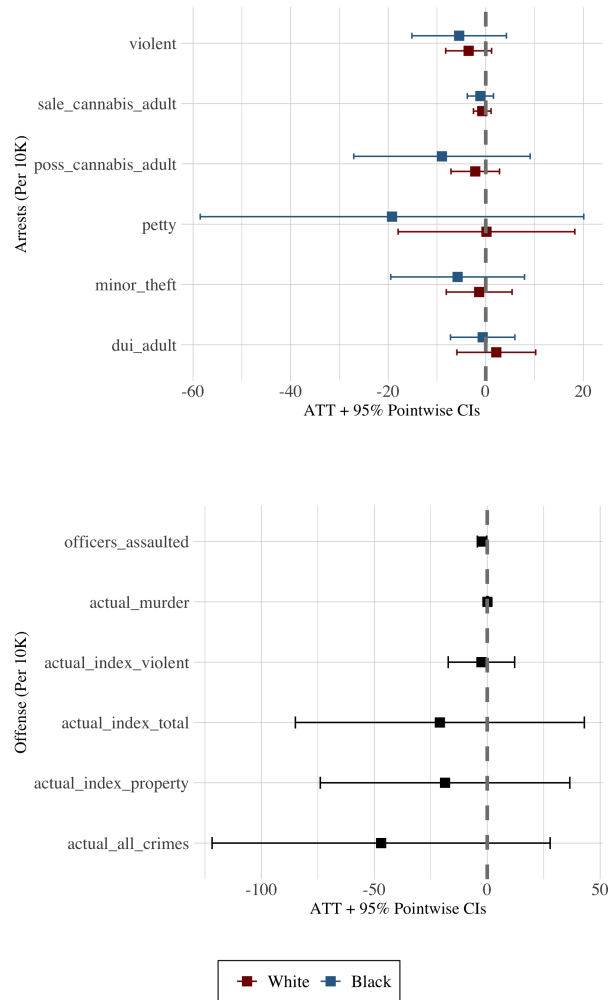


Figure 3.25: Effect of Breakthrough Election on Race-Specific Arrests And Offenses Known To Law Enforcement

Figure displays results from Equation 3.8 of the relationship between breakthrough mayors and race-specific arrests in the top panel and offenses known to law enforcement in the bottom panel. 95-percent confidence intervals constructed from multiplier bootstrap.



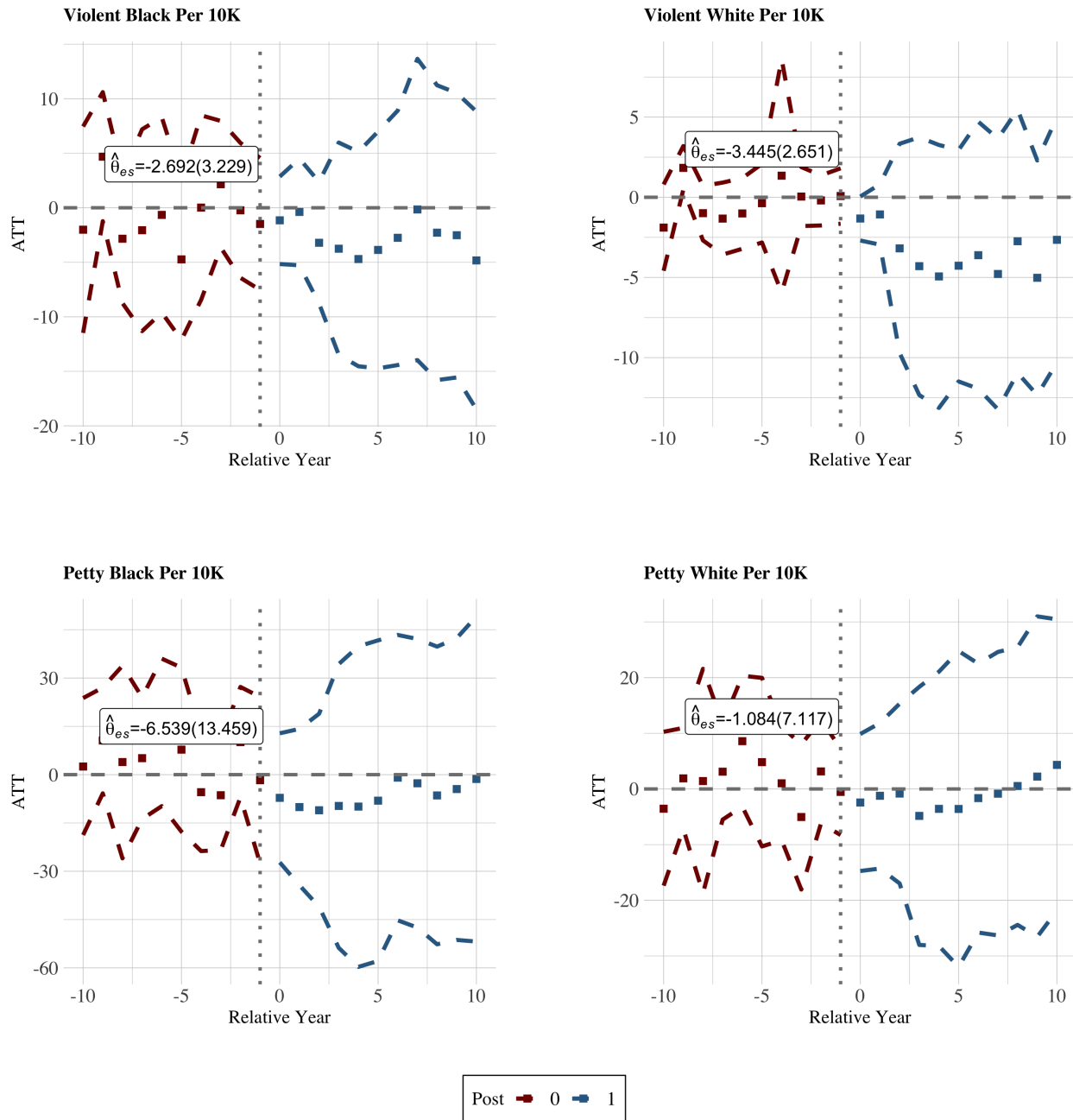


Figure 3.26: Impact of Black Mayor On Race-Specific Arrests

Figure displays estimates of Equation 3.9 of the relationship between breakthrough mayors and race-specific violent and petty crimes. Violent crime is defined as the annual sum of homicide, rape, robbery, and aggravated assault. Petty crime is defined as the annual sum of suspicion, vagrancy, vandalism, gambling, prostitution, liquor, curfew, loitering, and drunkenness. The 10-year event-time overall estimate is posted in the top-left corner of each panel. Dashed lines are 90-percent uniform confidence bands.

Table 3.18 estimates Equation 3.8 wherein Panel A I reprint the baseline results, Panel B restricts analysis to the cities that elect a Black candidate, Panel C excludes treatment cities where the mayor is a council person, and Panel D includes a state-year indicator so the comparison is between cities within the same year but differ in when or if

they elect a Black candidate. Because of the noted imprecision in outcomes, my main focus is on outcome stability, differences between Black and White outcomes, and if outcomes switch sign across specifications. “B” and “W” at the end of each outcome denote Black or White outcomes; “Pet” denotes petty, “V” for violent, “MT” for minor theft, and “Poss” for possession.

The magnitude for petty arrests for Black persons is stable across specifications and never smaller in magnitude than 19 in the baseline specification. For White arrests, they begin positive in the baseline specification and get as low as a 12 arrests reduction in the only Black elections panel. In all cases, the figure is much lower than for petty Black arrests. For violent arrests, the results are stable across specifications and the Black magnitude is larger across specifications. Minor theft is stable across the White category and jumps to a 19 arrest reduction in the within-state comparison. DUIs change sign for both Black and White person and display large variability. For possession arrests in the final two columns, the Black estimate is always negative, whereas the sign for possession arrests are sensitive to the specification.

Figure 3.27 displays treatment effect heterogeneity by timing group from estimating Equation 3.10. Treatment varies considerably across timing groups which supports use of the estimation method. While heterogeneity exists, it does not vary across generally intuitive features such as by period. Reductions in violent arrests for Black persons are consistently found for the pre-1990 group, but otherwise there is no such visible pattern. In the next section informed by the results here, I look to estimate the effects of manpower on crime induced by the election of Black candidates.

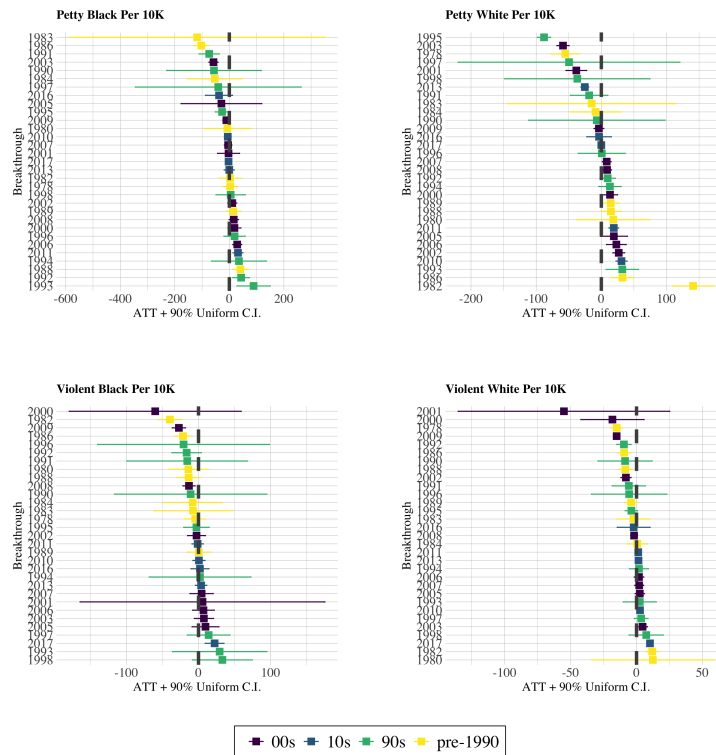


Figure 3.27: Group-Specific Impact of Black Mayors On Race-Specific Arrests

Figure displays estimates of Equation 3.10 of the relationship between the election of a Black candidate and race-specific petty/violent crimes for each timing group. Estimates are sorted by magnitude. Estimates for the pre-1990 groups are yellow, the nineties in green, aughts in purple, and teens in blue. 90-percent uniform confidence bands constructed from multiplier bootstrap are displayed.

<b>Full Sample</b>										
	PetB	VB	MTB	DUIB	PetW	VW	MTW	DUIW	PossB	PossWh
BM × Post	-19.22	-5.45	-5.77	-0.63	0.15	-3.51	-1.35	2.16	-8.98	-2.15
	(20.12)	(5.04)	(6.73)	(3.26)	(9.13)	(2.40)	(3.57)	(4.20)	(8.53)	(2.57)
Cities	140	140	140	140	140	140	140	140	140	140
*** <i>p</i> < 0.01; ** <i>p</i> < 0.05; * <i>p</i> < 0.1										
<b>Black Mayor Cities</b>										
	PetB	VB	MTB	DUIB	PetW	VW	MTW	DUIW	PossB	PossWh
BM × Post	-34.29	-8.04	-8.45	1.64	-12.26	-6.32	-1.87	-0.83	-6.44	0.40
	(23.86)	(7.62)	(12.18)	(4.77)	(10.71)	(5.27)	(4.86)	(6.91)	(10.03)	(2.91)
Cities	140	140	140	140	140	140	140	140	140	140
*** <i>p</i> < 0.01; ** <i>p</i> < 0.05; * <i>p</i> < 0.1										
<b>Chief Executive Cities</b>										
	PetB	VB	MTB	DUIB	PetW	VW	MTW	DUIW	PossB	PossWh
BM × Post	-36.41	-9.23	-8.46	2.61	-2.39	-4.08*	-0.56	7.43*	-18.52	-5.74
	(31.80)	(7.44)	(8.06)	(4.65)	(16.01)	(2.29)	(4.93)	(4.47)	(19.22)	(3.85)
Cities	140	140	140	140	140	140	140	140	140	140
*** <i>p</i> < 0.01; ** <i>p</i> < 0.05; * <i>p</i> < 0.1										
<b>Within-State Comparison</b>										
	PetB	VB	MTB	DUIB	PetW	VW	MTW	DUIW	PossB	PossWh
BM × Post	-33.48	-6.84	-19.08**	3.82	-4.26	-3.72	-4.47	8.30	-6.01	1.33
	(29.26)	(5.45)	(7.51)	(4.01)	(11.71)	(3.57)	(4.84)	(6.92)	(11.02)	(3.44)
Cities	140	140	140	140	140	140	140	140	140	140
*** <i>p</i> < 0.01; ** <i>p</i> < 0.05; * <i>p</i> < 0.1										

Table 3.18: Changes to Comparison Group, Treatment & and Specification

Table displays estimates of Equation 3.8 of the relationship between Black mayors and a range of race specific arrests. "-B" and "-Wh" represent Black and White. "Pet-" are petty arrests, "V-" are violent arrests, "MT-" are arrest for minor theft defined as the sum of burglary and larceny, "DUI-" are arrests for driving under the influence, and "Poss-" are arrests for possession of cannabis. Estimates are reported for the full sample in the first panel, cities that ever elect a Black candidate in the second panel, excluding the council-manager cities in the third panel, and the inclusion of a state-year indicator in the fourth panel to restrict comparisons to cities within the same state but differing treatment timing. Multiplier bootstrap displayed in parentheses.

### 3.5.5 Are Particular Interventions More Effective

In order to estimate the effect of particular kinds of police manpower on petty 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 dataset and estimate an event-specific treatment effect. I estimate

$$Y_{ect} = \alpha_{ec} + \delta_{et} + \tau_e \text{Black Mayor}_{ect} + \beta_e \mathbf{X} + \varepsilon_{ect} \quad (3.11)$$

where  $Y_{ect}$  is a crime outcome (petty 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 either by OLS or least absolute deviation to determine the effect of police manpower on violent or petty crime. Because these objects are related through estimation, I report bootstrapped standard errors for each.

Figures 3.28-3.30 plot the change in police on the x-axis and the change for petty or violent crime on the y-axis. The OLS fit is then plotted on top of the data. Figure 3.28 is consistent with work on the effect of police on crime. Increasing police manpower increases arrests for petty crimes and decreases arrests for violent crime. While the

change for petty arrests are roughly the same by race, changes to the number of officers reduces Black violent crime more than White violent crime.

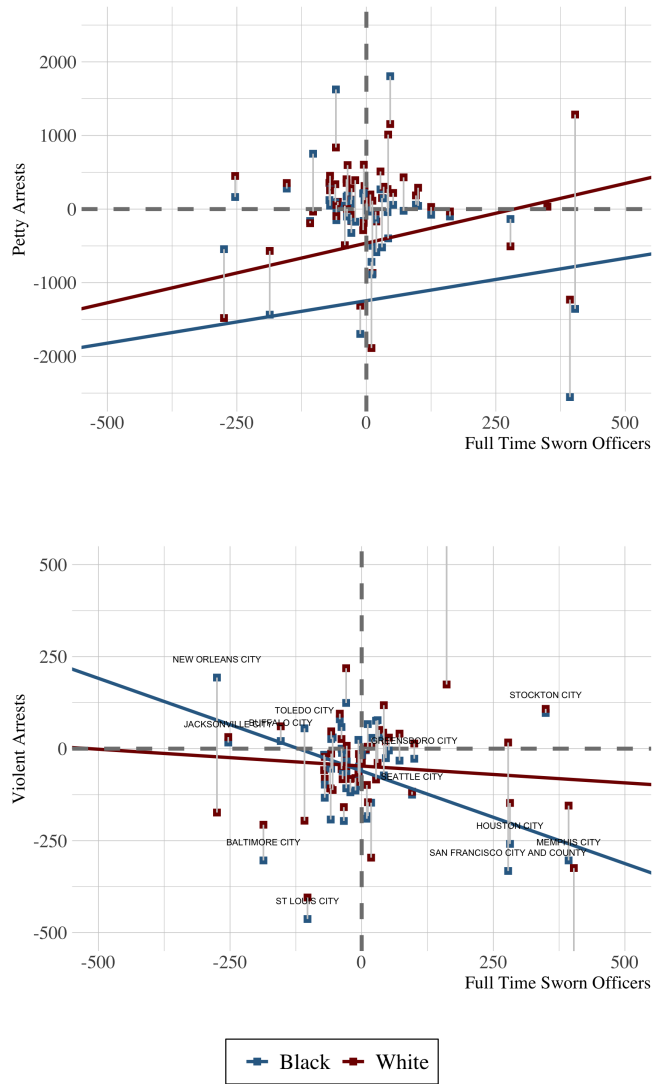


Figure 3.28: Relationship Between Changes In Force Size And Arrests

Figure displays estimates of Equation 3.11 of the relationship between Black mayors and changes in the size of the police force plotted against the relationship between Black mayors and changes to the total of petty (top panel) and violent (bottom panel) arrests. Line fitted through the resulting plot using OLS. Red dots correspond to White arrests and blue dots correspond to Black arrests which are vertically aligned by city. Not all cities are displayed in the figure but are used for fitting. Names of select cities are displayed.

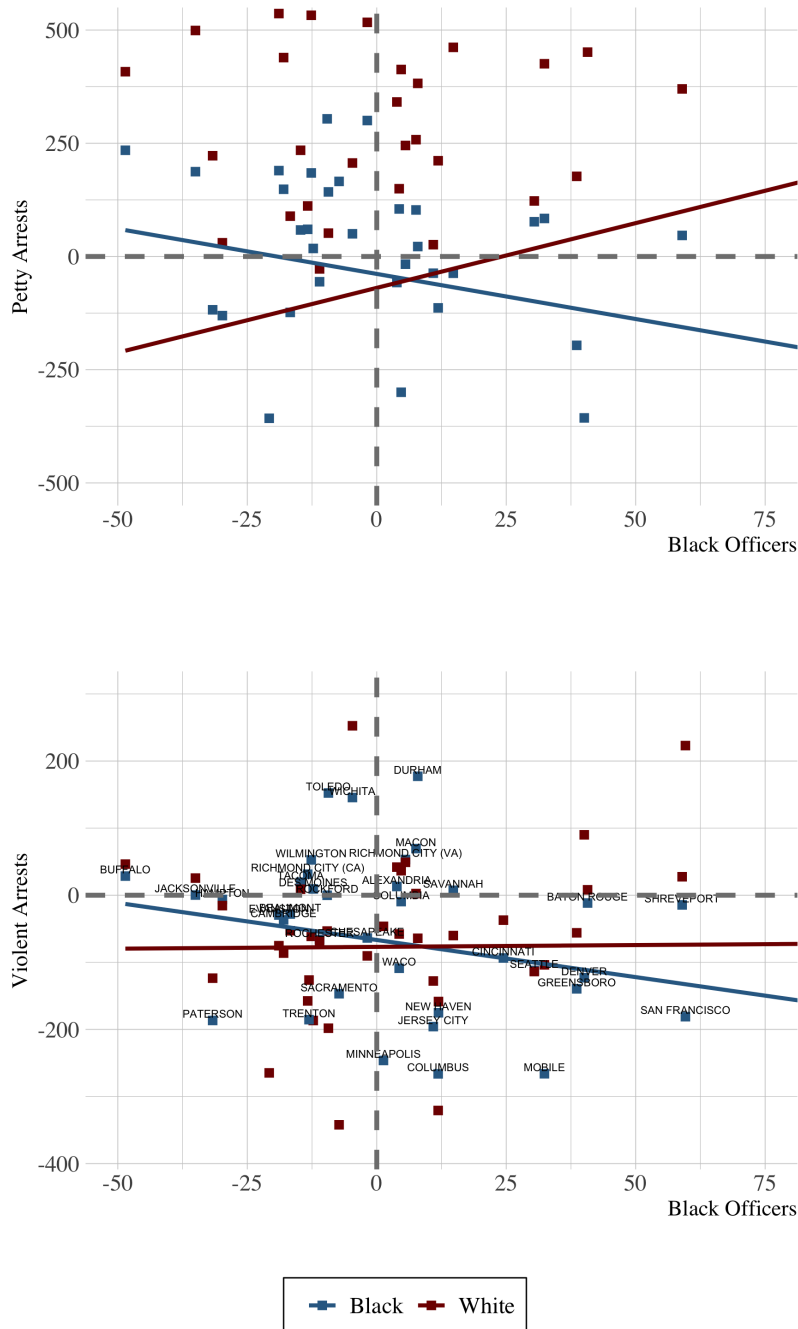


Figure 3.29: Relationship Between Changes In Black Officers And Arrests

Figure displays estimates of Equation 3.11 of the relationship between Black mayors and changes in the number of Black officers plotted against the relationship between Black mayors and changes to the total of petty (top panel) and violent (bottom panel) arrests using the 44 breakthrough elections that occurred after the first recording of LEMAS in 1987. Line fitted through the resulting plot using OLS. Red dots correspond to White arrests and blue dots correspond to Black arrests which are vertically aligned by city. Not all cities are displayed in the figure but are used for fitting. Names of cities are displayed in the lower panel.

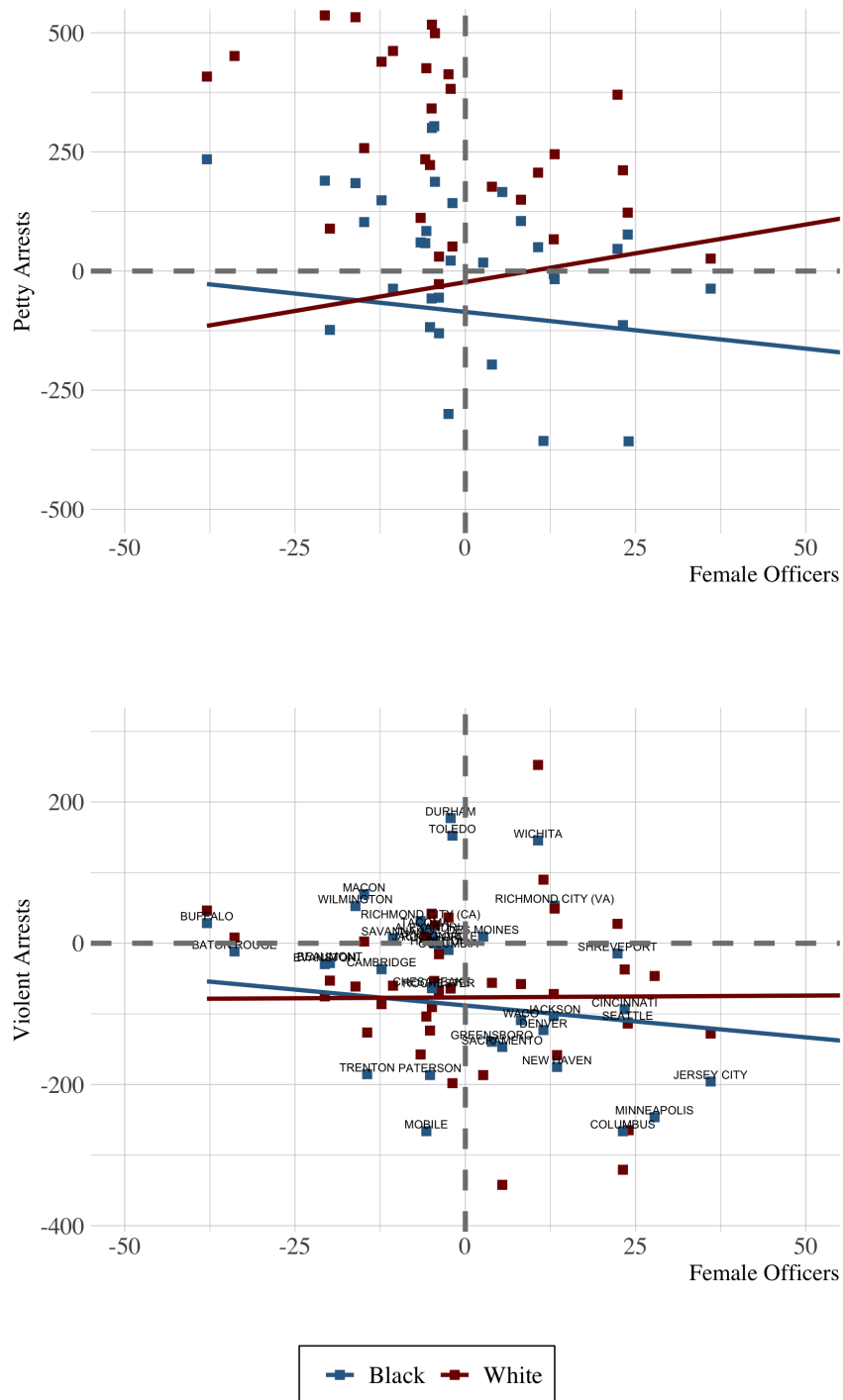


Figure 3.30: Relationship Between Changes In Female Officers And Arrests

Figure displays estimates of Equation 3.11 of the relationship between Black mayors and changes in the number of Black officers plotted against the relationship between Black mayors and changes to the total of petty (top panel) and violent (bottom panel) arrests using the 44 breakthrough elections that occurred after the first recording of LEMAS in 1987. Line fitted through the resulting plot using OLS. Red dots correspond to White arrests and blue dots correspond to Black arrests which are vertically aligned by city. Not all cities are displayed in the figure but are used for fitting. Names of select cities are displayed in the lower panel.

Table 3.19 displays the impact of an additional officer on the number of petty and violent arrests by race. The top panel reports estimation from OLS, and the bottom panel reports results from least absolute deviation (LAD). Bootstrapped standard errors are in parentheses. For petty arrests by either race, point estimates are of similar magnitude, but the range of probable values implies both positive and negative are likely. Increases to force size are significantly related to reductions in violent arrests for both Blacks and Whites. A 10 percent increase in police officers in a city with a population of 150,000 coincides with a reduction of 12 Black arrests and 2 White arrests. For a city as large as New York, Black arrests would be reduced by 1,822 and White arrests by 330 suggesting increasing police presence can have a measurable impact on violent offenses.

Table 3.20 displays the slope of a 1 Black or female officer change on petty and violent crime by race. An increase in Black officers reduces the number of petty black arrests by nearly 2 on average and increases the number of petty White arrests by 2.86. The estimates imply considerable variability across both races. When focusing on violent arrests an increase in Black officers drives a statistically significant decline in violent Black arrests of 1.1. While the result is not statistically significant when estimated via LAD, all but the smallest positive effects can be ruled out suggesting increasing Black officers is a path to reducing violent Black crime. The effect on violent White crime is null.

Female officers increase petty White arrest by 2.42 arrests, however this affect is not robust to estimation via LAD. Across the rest of the crimes of interests, it does not appear female officers induce changes to race-specific arrests patterns.

### **3.6 Discussion & Conclusion**

Across time and space, crime and public safety are one of the most oft-cited concerns in the run-up to breakthrough elections. The relationship, however, 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 combined to create a sense of safety and community. In this sense, my investigation only covers a small portion of how a mayor may cause or influence public safety outcomes. Experiences also play a factor in proposed policy. Some mayors come from policing backgrounds, others attorneys, and even others from business. Adding these hitherto “unobserved” elements enter into the analysis will provide further clarity as to how politics lead to policy.

Another element of this question is selection and attrition of police officers. If police culture is inert, then citizens that select into the academy and remain afterwards likely possess different attitudes than the citizens that they serve. Along that point, tenure possibly effects attitudes and behavior as well. If culture influences officers and is then reinforced by those officers who serve for long periods, then officer treatment of offenders—and by extension the number of arrests—may become more severe. On the other hand, compositional changes in the race of officers may influence attitudes in a way that reduces discriminatory behavior even amongst White officers. All of these questions remain understudied.

Noted shortcomings notwithstanding, the election of breakthrough Black candidates tends to lead to more Black and female officers and a lower share of city revenue devoted to policing. In turn, the increase of Black officers leads to decreases in violent arrests without the same increases in petty arrests for Black persons which means that a potential path to correcting the under-policing/over-policing outcome is through increases in Black officers. While the relationship observed between Black officers and Black violent arrests appears to be an important and attainable policy goal, the relationship is noisier when focusing on Black mayors specifically. Future work may want to dive deeper into specific policy divergences from previous administrations and how that intersects with changes in department composition to best understand how to deploy resources to reduce violent crime but also ensure communities are not negatively affected by changes in police presence.

	<i>Dependent variable:</i>			
	Petty Black (1)	Petty White (2)	Violent Black (3)	Violent White (4)
Panel A: OLS Results				
Full Time Sworn	1.154 (10.165)	1.622 (5.196)	-0.503*** (0.135)	-0.091*** (0.021)
Panel B: LAD Results				
Full Time Sworn	-0.189 (4.882)	1.195 (1.915)	-0.484** (0.198)	-0.065 (0.219)
Observations	58	58	58	58

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01  
Table displays OLS and least absolute deviations (LAD) linear fits for the relationship between police manpower and violent or petty arrests separated by race of offender. Fit is derived from separate estimates of Equation 3.11 of the impact of 58 Black mayoral elections on the levels of officers and arrests. Petty arrests are defined as the annual sum of suspicion, vagrancy, vandalism, gambling, prostitution, liquor, curfew, loitering, and drunkenness. Violent arrests are defined as the annual sum of homicide, rape, aggravated assault, and robbery. Bootstrapped standard errors are presented in parentheses.

Table 3.19: Relationship Between Police Manpower and Race-Specific Crime

	<i>Dependent variable:</i>							
	Petty Black		Petty White		Violent Black			Violent White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS Results								
Black	-1.992 (5.609)		2.859 (8.959)		-1.107** (0.497)		0.054 (1.134)	
Female		-1.542 (10.470)		2.420*** (0.339)		-0.902 (1.323)		0.051 (0.732)
Panel B: LAD Results								
Black	-1.866 (2.757)		3.035 (2.968)		-1.114 (0.707)		0.060 (0.492)	
Female		-1.517 (4.142)		2.459 (8.219)		-0.904 (1.257)		0.049 (0.850)
Observations	45	45	45	45	45	45	45	45

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01  
Table displays OLS and least absolute deviations (LAD) linear fits for the relationship between Black/female officers and violent or petty arrests separated by race of offender. Fit is derived from separate estimates of Equation 3.11 of the impact of 45 Black mayoral elections on the levels of Black/female officers and arrests. Petty arrests are defined as the annual sum of suspicion, vagrancy, vandalism, gambling, prostitution, liquor, curfew, loitering, and drunkenness. Violent arrests are defined as the annual sum of homicide, rape, aggravated assault, and robbery. Bootstrapped standard errors are presented in parentheses.

Table 3.20: Relationship Between Black & Female Officers and Race-Specific Crime



### 3.7 Appendix

#### 3.7.1 Additional Figures

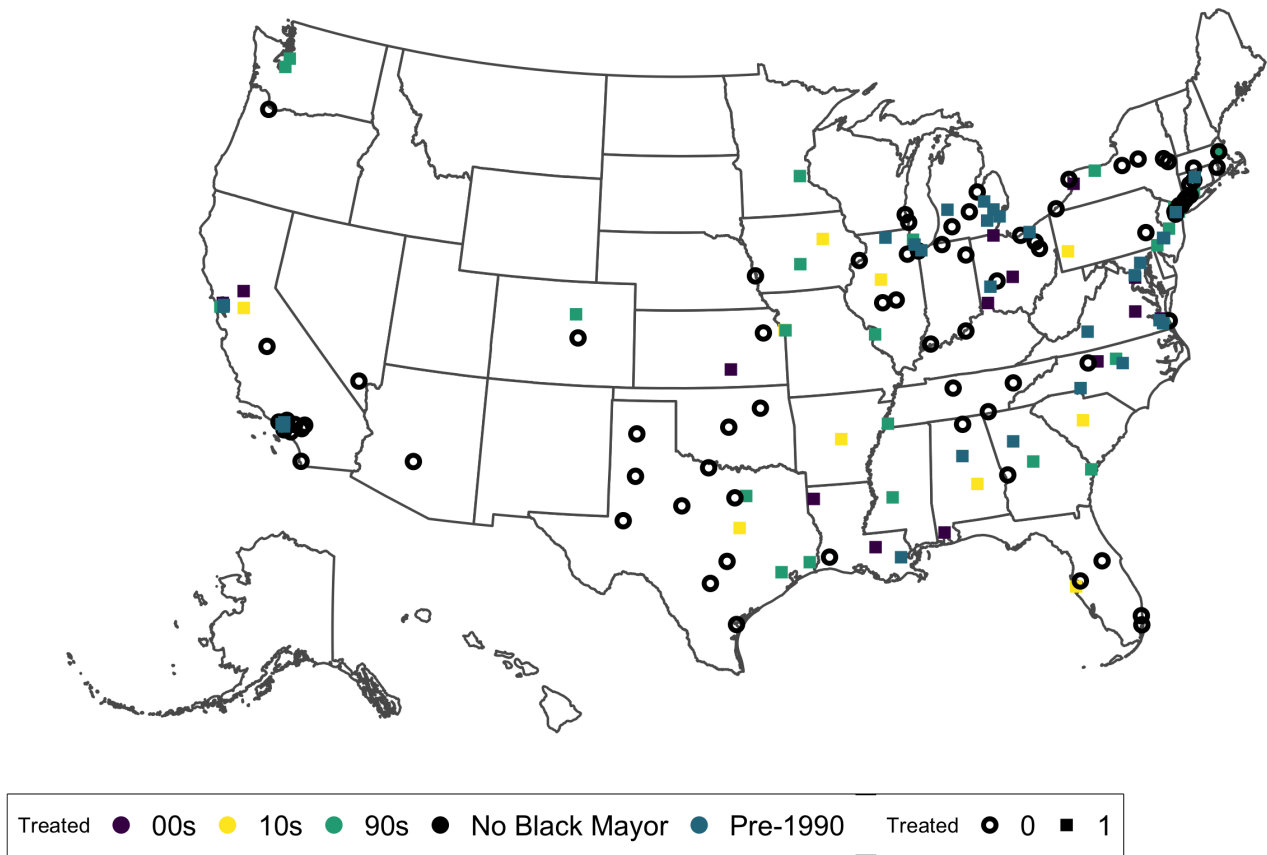


Figure 3.31: Map of Sample Cities

Figure displays cities with populations over 75,000 and Black population share greater than 4 percent in 1970. Hollow circles show cities that never elect a Black candidate. Solid squares show cities that have elected a Black candidate further categorized by the period in which the city first elects a Black candidate. Election data comes from Vogl (2014), Ferreira & Gyourko (2009), and manual collection.

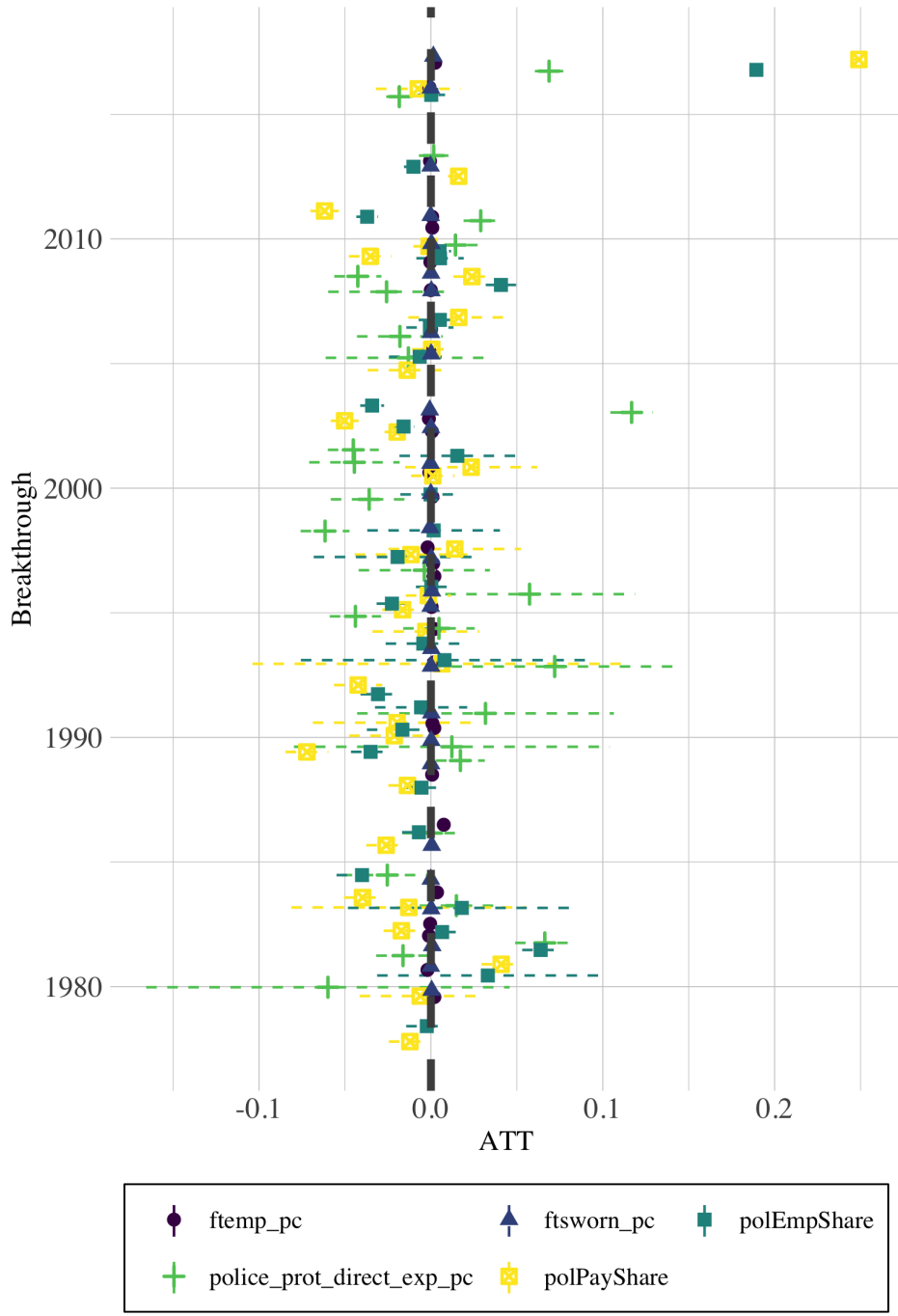


Figure 3.32: Group-Specific Impacts on Public Finance and Force Size Outcomes

Figure displays estimates of Equation 3.10 of the relationship between the election of a Black candidate and race-specific arrests for each treatment group. Hollow shapes describe impacts for White arrests and solid shapes are for corresponding Black arrests. Estimates are plotted with 95-percent multiplier bootstrap standard errors.



Figure 3.33: Group-Specific Impacts on Other Race-Specific Policing Outcomes

Figure displays estimates of Equation 3.10 of the relationship between the election of a Black candidate and race-specific arrests for each treatment group. Hollow shapes describe impacts for White arrests and solid shapes are for corresponding Black arrests. Estimates are plotted with 95-percent multiplier bootstrap standard errors.

## Chapter 4

### A Historical Perspective of Black Mayors A Focus on Four Cities in Alabama

Arthur Shores home was bombed multiple times in 1963 because of his work to integrate Birmingham schools. Five years later in the city that earned its title of “Bombingham,” Shores would become the first Black city council person in a large city in Alabama after being appointed by the conservative George Seibels. From that point across time and space, Black citizens would come to occupy positions of power in municipal government. This essay focuses on the four largest cities in Alabama: Birmingham, Huntsville, Mobile, and Montgomery. What follows is a brief description of the cities and how Blacks rose to power in each with particular focus on the mayor’s office, except in the case of Huntsville. The principal question of this article is how much have Black residents benefitted from their gains in political power? Alabama is unique in the sense that a Black resident has become the chief executive at different periods and one city that has never had a Black resident. In this way and in a limited sense, one can observe differences in Black political efficacy perhaps driven by period specific effects.

In the eighties when Richard Arrington would preside over Birmingham, cities were in the grips of the “end of urbanism,” transitioning to post-industrial economies, and reorganizing government revenues caused by the massive drop in inter-governmental transfers caused by ascent of the Reagan administration. The early aughts were a relatively prosperous period for Blacks which was quickly reversed during the run-up to the Great Recession and the long, slow recovery afterwards. In this period, Sam Jones would become the mayor of Mobile while also dealing with the fallout of Hurricane Katrina. During the teens, Blacks closed the unemployment gap with Whites bringing it to an all-time low in late 2019 before the coronavirus pandemic caused massive rises in unemployment. Steven Reed of Montgomery finds himself navigating these challenges which also brought about renewed revenue challenges for municipal governments everywhere.

In each case, including Huntsville, before the mayoralty was a council person and to get there required either the support of liberal whites, court action, or both. The aforementioned Shores would become the first Black on the Birmingham council, but only a few years before, Birmingham was led by three commissioners perhaps most notable of them the staunch segregationist and public safety commissioner, Eugene “Bull” Connor. Connor’s violent enforcement of segregation led to another White man, David Vann, to push for a mayor–council form of government with district representation. With support of progressive Whites and the growing Black population, Vann’s referendum was passed, and Blacks began to have representation in municipal government.

Ten years after “Bloody Sunday” in the “Cradle of the Confederacy” and the “Birthplace of Civil Rights,” Joe Reed—father of current mayor Steven Reed— became the first Black city council person in Montgomery, Alabama. After runoff elections in three other districts, Reed was joined by Catheryne Caswell, Luther Oliver, and Herman Harris.<sup>63</sup> The effort to reorganize Montgomery city government began with paper executive, C. James Robinson. Robinson was elected to the city commission in the election of 1971, and soon became the most powerful commissioner after the other two White commissioners, Lamar Wagner and Jack Rucker, follied in passing a one percent occupational tax to fund a city employee pay raise which was reversed within a month (Thornton 2002). Sensing his new position of power, Robinson petitioned the state to change the form of government to mayor–council where Blacks would gain representation but not proportional as districts were drawn to limit gains. Reed and other Black leaders challenged the districts taking their concerns to the Justice Department on the grounds that the districts violated the Voting Rights Act. Ultimately, Rucker and Wagner, supported the Black leaders’ districting proposal in hopes of tanking the measure. Their gambit backfired, and Reed and Black residents would account for four-ninths of the newly formed council, the same proportion they currently hold. Reed became a political kingmaker being courted

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<sup>63</sup>“Four Montgomery Council Seats Won,” *Afro-American*, October 4, 1975. pg. 7.

by many legislators for the Black vote including George Wallace. Reed served on the council for more than twenty years paving the way for the next wave of Black leaders.

Mobile was structured under the commissioner form of government from 1911 until 1985. Black residents had not experienced any form of representation since Reconstruction. The first effort to gain representation came from the court case, *Mobile v. Bolden* City of mobile, Alabama, et. Al. v. Wiley I. Bolden, et. al. (1980). The case challenged that the commissioner form of government breached the Voting Rights Act by diluting Black voter efficacy. The Burger court ruled in favor of the city claiming that because Blacks could freely exercise their right to vote without prejudice or exclusion, Mobile's government did not purposefully discriminate against Black residents. Justice Potter Stewart opined, "the (15th) Amendment does not entail the right to have Negro candidates elected." The court ruling eventually led state legislators to propose an amendment to Mobile's government that was overwhelmingly approved by state voters. Clinton Johnson, Charles Tunstall, and Irmateen Watson were the first Black residents elected to the new Mobile city council in 1985. Three Black residents currently sit on the Mobile city council.

Huntsville, named for the first White to settle in the area, was among the first cities in Alabama to adopt the mayor-council form of government. Not quite rising to Atlanta's "The City Too Busy To Hate," race relations in Huntsville were described as calm.<sup>64</sup> Relations were unusual enough that a Black resident ran for city council as early as 1952.<sup>65</sup> While relations were less contentious comparatively, Huntsville would not elect a Black city council person until 1988 when Richard Showers won a seat that he would retain for seven terms. Over the course of thirty years, the Black population in Huntsville grew, and Huntsville now has two Black councillors (one of which is the son of James Meredith.)

Across the late, twentieth century each city arrived at a similar point. What follows is a comparison of Black residents' welfare acknowledging each city's history, and when Blacks rose to central power. I take time to focus on issues that have been econometrically hard to identify because of a lack of centralized data, the need for administrative data, or because we perhaps have not answered the question sufficiently. Overall because of the one state focus, the answer to the question of Black political efficacy is not necessarily a limpid one, but that should not be mistaken for Blacks not making gains through self determination.

## 4.2 The Mayors

### 4.2.1 Richard Arrington

Richard Arrington Jr. was born during the Depression in October of 1934 in the Alabama Black Belt. Born to a religious family who had long ties to the South from slavery to sharecropping to tenant farming, Arrington Jr.'s family instilled the value of community into him at an early age. Before 10, Arrington's family moved to Birmingham suburb, Fairfield, where they would regularly attend church, observe strict Christian values, and his father would make a point of patronizing Black-owned businesses (Franklin 1989).

Arrington excelled in school, graduating from Fairfield Industrial High, a Black high school which offered vocational and academic training as well as required course in Black history until integrated in 1969 (Franklin 1989, 31).<sup>66</sup> He matriculated to Miles College and supported himself through a dry-cleaning job, graduating *cum laude* in biology. Shortly after he moved to Detroit to pursue his Master's and then to the University of Oklahoma for his doctorate where the moving of his wife and him would have tripled the Black population of Norman (Franklin 1989, 43).<sup>67</sup>

<sup>64</sup>"Huntsville Cites Race Gains," *Washington Post & Times Herald Post*, June 20, 1956. pg. 20.

<sup>65</sup>"Principal to Run for Huntsville City Council," *Afro-American*, August 23, 1952. pg. 21.

<sup>66</sup>Which also produced Willie Mays around the same time.

<sup>67</sup>Here's his dissertation for one to peruse or glance at: <https://shareok.org/bitstream/handle/11244/2005/6610165.PDF?sequence=1&isAllowed=y>

While Arrington did not plan a life of political service, he was quickly drawn into the political arena in a rapidly changing Birmingham. Arrington rebuffed calls to run in the 1971 mayoral campaign but agreed to run for a council seat. He became the first elected, Black member of the city council on a platform reducing employment discrimination and improving city services in Black communities. Concerned with then Mayors Seibels's and (to a lesser extent on the first charge) Vann's inactivity on affirmative action and police brutality paired with an inert city council, Arrington would move to become Birmingham's first Black mayor in 1979 amid the calls for justice for Bonita Carter, a 20 year old, Black woman who was slain by a police officer a few months before the mayoral primary.

At the time of his first term, the council was one-third Black, so any efforts toward racial advancement were moderated by the majority-White, racially-conservative city council. Arrington's policy tentpoles were not dissimilar to that of other mayors: "personal safety, secure property, and effective delivery of city services (Franklin 1989, 192)." Understanding the end of urbanism, Arrington pushed to attract high-tech jobs to complement the University of Alabama at Birmingham's Medical Center while pushing through downtown development and public construction much to the surprise of White business owners.<sup>68</sup> Upon leaving office twenty years after first assuming office, Arrington Jr. reversed the recession Birmingham experienced, revitalized the tax base, and reduced crime. Though crime reductions were evident across all locales at the end of the nineties, many interventions were made in the early- and mid-eighties such as increased lighting in communities, increased street personnel, and recalcitrance in the face of the Birmingham personnel board upon appointing a new police chief in 1981.<sup>69</sup>

#### 4.2.2 Sam Jones

Jones, the self described "Obama of Mobile"<sup>70</sup> was born in Mobile in 1947. After serving in the navy as an officer of race relations, Jones became a longtime public servant of Mobile. Jones was elected as a Mobile County Commissioner in 1987 after a stint in community activism, a position he would hold for four terms. Jones ran for mayor in 2005 against city councilman John Peavy in an election that produced the largest turnout in Mobile history. Jones outpointed Peavy to become Mobile's first Black mayor.

Jones was immediately thrust into crisis upon assuming office. He dealt with the impact of Hurricane Katrina, both its physical toll on the city and the influx of evacuees it brought. Interviewed after Katrina, Jones said, "A lot of things we were planning to do will actually take a back seat to hurricane relief and a lot of things that confront the city—such as law enforcement issues, public safety issues, the funding for those sources— will certainly be strained a lot more."<sup>71</sup> While the hurricane strained the city, Jones was able to come through on some of the issues he mentioned. Jones was able to increase the size of the police force (Hopkins and McCabe 2012) and improve city services. During his administration, the city of Mobile implemented a "311" service that improved civilian-government interactions, and allowed the city to use a CompStat-like procedure, Mobile CitiSmart, across all city departments. Jones was defeated for re-election in 2012 by Sandy Stimpson. Reflecting on the experience Jones remarked, "I think there is a price you pay for being the first Black of anything."<sup>72</sup>

#### 4.2.3 Steven Reed

Reed, the most recently inaugurated breakthrough mayor, was born in Montgomery in 1974, one year before his father, the aforementioned Joe Reed, would become the first Black city councillor. Seemingly born for the position, Reed began his career as a financial analyst after earning his MBA (at Vanderbilt). Reed later moved to public service

<sup>68</sup>Future mayor William Bell was quoted as saying "He wants it to be said that Richard Arrington, a black mayor, had a tremendous positive effect on white business!" (Franklin 1989, 195)

<sup>69</sup>[africanamerican.com/2014-honorees/richard-arrington-jr/](http://africanamerican.com/2014-honorees/richard-arrington-jr/)

<sup>70</sup>[https://www.al.com/news/2018/07/im\\_the\\_obama\\_of\\_mobile\\_sam\\_jon.html](https://www.al.com/news/2018/07/im_the_obama_of_mobile_sam_jon.html)

<sup>71</sup><https://www.npr.org/templates/story/story.php?storyId=4853585>

<sup>72</sup>*ibid*

becoming the first Black probate judge in Montgomery county.<sup>73</sup> Reed's quick rise to the mayoralty likely began because of his decision to issue same-sex marriage licenses in defiance of Chief Justice Roy Moore.<sup>74</sup> Reed ran for office in 2019 and won by an overwhelming 2-1 margin against local businessman, David Woods who campaigned on a crime reduction platform.

Reed is halfway through his first term and had to deal with the pandemic and the incumbent pressures that brings along; however, Montgomery's fiscal condition is healthy as described during the 2021 State of the Community. Reed has been a vocal advocate for improving Montgomery Public Schools though he does not possess direct power over them. His support of amendment 382 led to a much needed increase in the county property tax rate through a special district school tax. In acknowledgement of the issue of crime and the pandemic's effect on it, Reed and Chief of Police, at the time, Ernest Finley would implement hotspot policing tactics and the violent crime task force, Tactical Impact and Deterrent Enforcement (TIDE). These policies led to a 13 percent reduction in violent crime year over year (Reed 2021).<sup>75</sup> Montgomery also oversaw the clearance of 130 blighted structures in 2012 with a plan of exceeding that total in the next year (Reed 2021). As Montgomery moves past the pandemic, Reed expects his Economic Impact Recovery Task Force to assist businesses with the transition to the post-pandemic economy.

### 4.3 Public Finances and Tax Rates

Mayors who desire to improve government services must first designate where the funds will come from in the budget. In general, the city can divert resources from other programs/departments or raise revenues from taxes or charges (assuming no changes to intragovernmental transfers). In pursuit of lowering the tax burden on low-income and minority residents (namely, their constituents), Black mayors may have personal and political incentive to increase tax rates from less regressive sources such as property taxes relative to sales taxes or use charges (Suits 1977). However, even when mayors forward or adopt raises to property taxes, they encounter recalcitrance from interested voters (i.e. homeowners) and city councillors who themselves are homeowners as discussed by Piliawsky (1985) in the case of New Orleans and Ernest Morial who attempted to raise property taxes before being rebuffed and blocked by the mostly White city council. Morial eventually resigned to increasing the city's sales tax rate to fund the city's bus service.<sup>76</sup>

An additional problem in observational data is that the researcher often observes the total revenue collected from the tax and not the bundle of factors that lead to it. For example, Hopkins and McCabe (2012) find that the share of property and sales taxes as a share of total tax revenue does not change after the election of a Black candidate. Interestingly the share of sales tax revenue increases by an economically significant but statistically imprecise 5 percentage points which is almost exactly offset by the change in the property tax share. These results do not speak to if the tax rates change during a Black mayor's administration, if revenue from either tax increases, resident income increases, or changes to renter-homeowner composition lowers property tax revenue. Table 4.21 presents tax rates to begin to parse out these factors. The left panel describes the addition and changes to the general sales tax rate for each sample city. Beginning in early 1989 in Montgomery each city had levied a new general sales tax within two years. Birmingham at the time had the only Black mayor and was the last city to adopt the tax, and the two cities with the highest Black populations enacted the lowest rates. Perhaps because these cities' initial rates were low, they were also the first to change the rate beginning in Montgomery in fall of 2002. Montgomery increased the rate one percentage point to 3.5 percent which was still lower than the highest rate by half a point. Perhaps in response to the incipient recession and its effect on housing values, Birmingham increased their rate by one p.p. to 4. Interestingly these changes paired with the county and state rates placed Birmingham and Montgomery as the highest in the country for

<sup>73</sup><https://apnews.com/article/07a386d297434a70a5b5bf7444d856a0>

<sup>74</sup>The Supreme Court of Alabama ruled against Reed, so he was forced to stop issuing licenses.

<sup>75</sup>March of 2022 saw the resignation of Chief Finley and the hiring of Darryl Albert, the commander of the special operations division in New Orleans and a Black man himself.

<sup>76</sup>In addition, see Einstein, Ornstein, and Palmer (2021) for a description of the overrepresentation of homeowners in all levels of government.

General Sales				Property, 2000-		
City	Begin Date	End Date	Rate	City	County	Millage
Birmingham	12/1/1991	12/31/2007	3.0	Birmingham	Jefferson	28.5
Birmingham	1/1/2008	–	4.0	Birmingham	Shelby	36.2
Huntsville	11/1/1989	2/28/2014	3.5	Huntsville	Limestone	35.0
Huntsville	3/1/2014	–	4.5	Huntsville	Madison	19.5
Mobile	4/1/1991	5/31/2010	4.0	Mobile	Mobile	7.0
Mobile	6/1/2010	–	5.0	Montgomery	Montgomery	12.5
Montgomery	2/1/1989	9/30/2002	2.5			
Montgomery	10/1/2002	–	3.5			

Table 4.21: Municipal Sales & Property Tax Rates For Select Cities In Alabama

Source: Alabama Department of Revenue Local Sales, Use, Rental, & Lodgings Tax Rates Text File and Alabama Department of Revenue Property Tax page

cities with populations over 200,000.<sup>77</sup> Mobile was next to raise their already highest rate one additional point to 5 percent capitalizing on its tourism industry during the Sam Jones administration. Huntsville was the last city in the sample to raise their rate in 2014. No city has raised their general sales tax since then.

Panel B in Figure 4.34 shows changes to sales tax collections per capita for each city from 1970 to 2018. Changes to the general sales taxes are evident in the time series. Compared to property taxes in panel A, sales taxes account for a larger portion of total revenue and have grown faster than property taxes over the period a salient feature in Southern states. The increase in collection is especially noticeable in Montgomery’s subsequent change whereby the city was able to collect an additional \$150 per person after the change. Notably given the differences in the initial rates for each city’s tax changes, additional revenue from each change is notably different. For example, Montgomery’s similar 1 p.p. tax change netted them less than \$100 per person of additional revenue because of the already high baseline rate. While each *municipality* sets a different rate, the only *total* rate (state + city + county) that currently differs is Huntsville (9% vs 10% for the others). Huntsville and Mobile collect similar revenue from sales tax per resident, but a Montgomery resident that pays a similar total rate to a Mobile resident and has similar median individual income (\$25,453 in Mobile and \$26,465 in Montgomery as measured in 2020.) which possibly reflects differences in sales taxes in other areas. *e.g.* sales taxes designated as amusement are taxed 1.5 p.p. less in Montgomery than in Mobile.

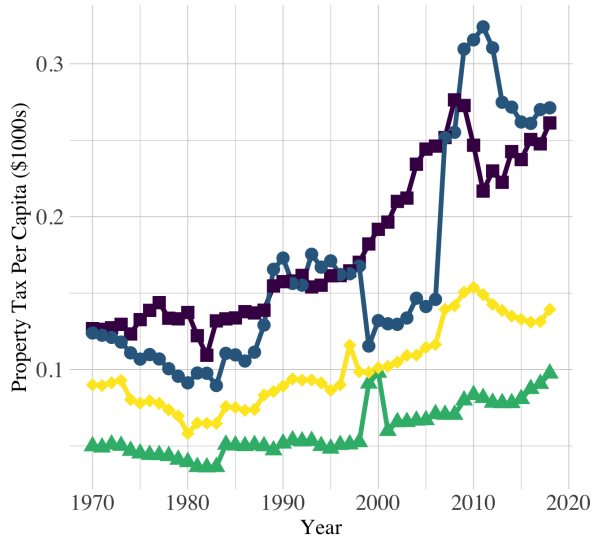
The right panel contains property tax millages by city and county which may differ in different districts in the city because of belonging to different school districts. Millages are available beginning in 2000, and unlike sales tax rates, none of the cities have changed their rates over the more than two decades since data has been made available. Despite no changes to the rate, there is considerable variation in property tax collection per capita as is evident in Figure 4.34. The levels for each are consistent with the millages where Birmingham and Huntsville have the highest per capita collection. Property taxes per capita rises for each city, but the gains to collection are especially pronounced for Birmingham. The series drops for a few years immediately after the Arrington election consistent with a story of racially-motivated outmigration, however collections rise by the end of the decade and by the end of Arrington’s administration by approximately \$60 per resident and continues to sharply increase until the recession.

Property taxes owed are a combination of residential rate, millage rate, appraisal value, and exemptions. Table 4.22 explores changes to homeownership and property values for Black residents in the Birmingham and Mobile metro areas using OLS and includes a treatment interacted with the respondent identifying as Black and year–race and metro-area–race fixed effects thus the comparison is between Black residents in each metro area, one that elects a

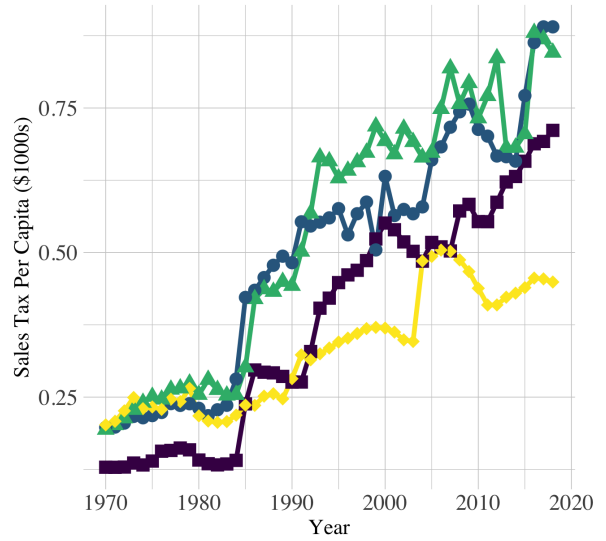
<sup>77</sup>[https://www.al.com/birmingham-news-commentary/2011/05/post\\_27.html](https://www.al.com/birmingham-news-commentary/2011/05/post_27.html)



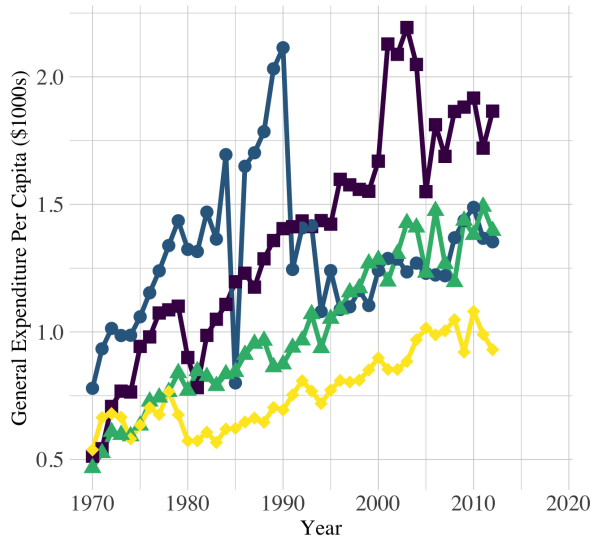
A



B



C



D

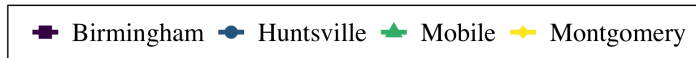
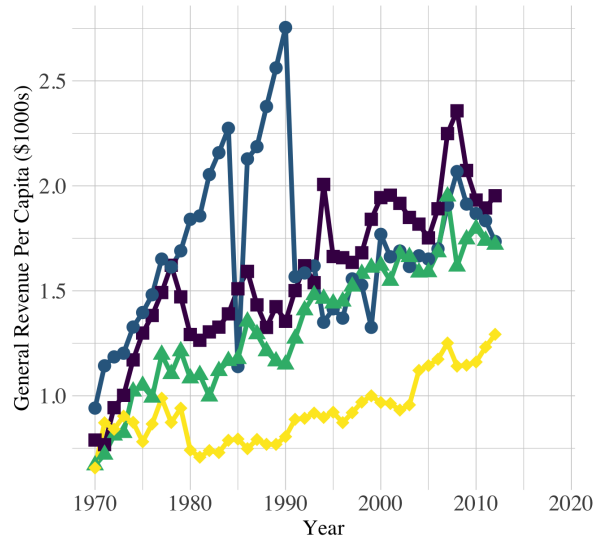


Figure 4.34: Public Finance Outcomes 1970-2018

Source: Annual Survey of Governments Individual Unit Finance Historical File 1970-2012 and State and Local Government Finance Historical Dataset 2013-2018

Black candidate and the others that do not. Each treatment is measured separately for a different set of years. That is, the Birmingham treatment is for census years 1970 to 2000 and includes all four cities. The Mobile treatment is for years 1990 to 2010 and excludes Birmingham from the estimation because Birmingham elects a Black mayor earlier and still had one. My primary interest is what happens to Black residents within city limits, but in some cases this is infeasible because there is no way to consistently identify city versus suburban residents. However, the time period for the Mobile sample includes a response for residents living in the central city, so I separately estimate a Mobile “in-city” and MSA specifications to determine if results differ by the codified reach of the mayor.

The first column of Table 4.22 shows the change to the homeownership rate for Black residents in the Birmingham MSA relative to Black residents in other large cities in Alabama. In the 20-year period after the Arrington election the Black homeownership rate increased one percentage point relative to the comparison cities. The 1980 homeownership rate for Black residents was 58.69% which was approximately 18 p.p. lower than the White homeownership rate. The Birmingham increase constitutes a 1.7 percent gain to the Black homeownership rate. Homeownership rates for both Black and White residents increased by 2000, but the White rate grew faster increasing the homeownership gap by 2.54 p.p. (White rate: 80.62, Black rate: 60.54). Columns 2 and 3 repeat the exercise for the Mobile MSA and Mobile proper. The Black homeownership rate for the Mobile MSA decreases after the Sam Jones election by 1.7 p.p. though this number is not statistically different from zero, and restricting the comparison to residents that reside in each MSA’s central city, the sign reverses and the rate increases by 2.5 p.p.. Similar to the preceding result, the estimate is also not statistically different from zero. Taken together this suggests, however, the mayor’s jurisdiction is important, and the Birmingham figure may be understating the change to central city residents.

Column 4 estimates the change in housing values for Black homeowners in Birmingham relative to control cities before and after the Arrington election. Similar to the homeownership rate, there is a substantial gap between Black and White housing values. In the 1980 census sample, the average value for a home owned by a White resident was \$139,667.80 and the average value for a Black resident was \$79,580.56 constituting a value gap of over \$60,000 or three-quarters of the value of a Black home. Home values for Black, Birmingham residents increased 5.9 percent after the Arrington election or by \$4700 relative to Black residents in other cities. Over the period home values increased everywhere for both races, and the change mirrors the homeownership rate where Whites increased the gap relative to Black residents. White home values increased 24.6 percent to \$174,079, and Black home values increased 16.66 percent to \$92,835.94 increasing the gap by \$21,155.82. Turning to the Sam Jones election, home values in the Mobile MSA increased by a noisy 2.8 percent, and home values in Mobile proper increased by an imprecise 1.2 percent. The estimates indicate that Black representation in the early period was especially important to improve Black outcomes. Additionally, the estimates provide evidence that the increase in property tax collection can be attributed to increases in the number of homeowners and and home values.

	Birm. Own	Mob. Own	Mob. City Own	Birm. Value	Mob. Value	Mob. City Value
Black Mayor x Black	0.010 ** (0.005)	-0.017 (0.011)	0.025 (0.017)	0.059 *** (0.008)	0.028 (0.025)	0.012 (0.032)
N	141242	49776	15691	96159	36821	10326

\*\*\* p < 0.01; \*\* p < 0.05; \* p < 0.1.

Table 4.22: Black Mayor Impact On Home Ownership And Value

Notes: This table presents estimates of the relationship of Black, MSA resident outcomes and the election of Richard Arrington (Birmingham) and Sam Jones (Mobile). For Birmingham results, residents from all four MSAs are included and the time period is restricted to 1990 to 2010. For Mobile, residents in the Birmingham MSA are excluded and the time period is restricted to 1990 to 2010. The estimates are from canonical two-way fixed effects regressions with a Black mayor indicator, a quadratic an age, education, age–education interaction, and MSA and census-year fixed effects. Estimation restricted to those above 18 and weighted using census weights. Robust standard errors are reported.

## 4.4 Employment & Businesses

### 4.4.1 Employment

Figure 4.35 describes the unemployment rate by race for each city in the sample for the years where each city is observed. The Black unemployment rate is always higher than the White unemployment rate for each city and in many cases is more than double the White unemployment rate. The Black unemployment rate decreases in the civil rights decade with the exception of Montgomery. The seventies brought high inflation and unemployment which is more evident for Black laborers than White ones. For example in Birmingham, the Black unemployment rate reaches a low of 6 percent in 1970 and spikes to nearly 12 percent at the beginning of the Arrington term. Whites over the same period experience a much smaller increase to their unemployment rate. Interestingly, Richard Arrington's time in office coincides with a large reduction in Black unemployment that is not observed in the other cities. Nye, Rainer, and Stratmann (2014) in a paper about the race-specific employment impacts of Black mayors find exactly this result. A less understood, but ultimately important part of the narrative is how these Black mayor effects change by the powers available to the mayor and when the mayor is elected. In Table 4.23, I reexamine the authors' findings from the scope of Alabama and their two differently timed elections.

Column 1 measures the change in labor force participation rates (L.F.P.) for Black residents after the Arrington and Jones elections. In both cases, Black residents are more likely to be in the labor market after a Black election. The Birmingham increase of 0.8 p.p. outpaces the 1980 to 2000 increase for Black residents in Alabama. Black L.F.P. in 1980 was 65.31 percent and increased 0.48 p.p. by 2000. The Black–White L.F.P. gap in 1980 was 5.98 p.p. and grew an additional 3.34 points by 2000 emphasizing the role Black representation plays in mitigating the growth in these gaps. Column 2 restricts attention to respondents that identified as being in the labor force and then asks if Black residents are less likely to be unemployed after a Black election. This question differs from the first because a Black election may induce excitement about potential labor market prospects causing an increase in L.F.P. but this may not translate to employment gains if Black elections are purely symbolic. Column 2 sheds light on this question providing estimates on the change in the Black unemployment rate (U.R.) for each election. Negative numbers imply the unemployment rate went down, whereas positive numbers imply an increase. For Birmingham, the Black U.R. decreased 1.1 p.p. which is larger than the increase in L.F.P. implying the rate of employment accelerated more quickly than those returning to the labor force for Blacks in Birmingham. In Mobile, the U.R. increase 0.7 p.p. suggesting employment growth for Blacks was outpaced by Black residents returning to the labor force.

A couple of questions immediately arise when probing into Black employment growth. First is where are Black residents finding employment. Nye, Rainer, and Stratmann (2014) find large gains in local government. Black Americans were employed at all levels of government after the Civil Rights Act, so part of the question is disambiguating the Black mayor effect from the civil rights effect. In any event, increases in representation and from legislative measures were so effective that Black Americans were overrepresented in government employment by 2000; thus, one may surmise that the representational effect may be starkly different across periods. Before turning to regression results, Figure 4.36 depicts time series trends in local government employment and self-employment rates for the four cities. The solid line is the self-employment rate and the dashed lines are local government employment. By the time it is recorded in 1970, the rate at which Black residents are employed in local government is already greater than that of White residents. This pattern remains with little exception over the entire sample period. Though these figures are only descriptive, this casts doubt onto the ability of Black mayors to boost Black employment in local government, though they have substantial latitude to make hires within their own staff and to a lesser extent at the top of departments.

Column 3 reports estimates to changes in local government employment. Both cities observe small, noisy, and positive changes to local government employment. While the absolute change is noisy and small, the confidence interval cannot rule out relatively sizeable gains to Black employment in Birmingham as large as 10 percent from the 1980 average (10.8 percent) and nearly 25 percent in Mobile from the 2000 average (8.3 percent). That being said

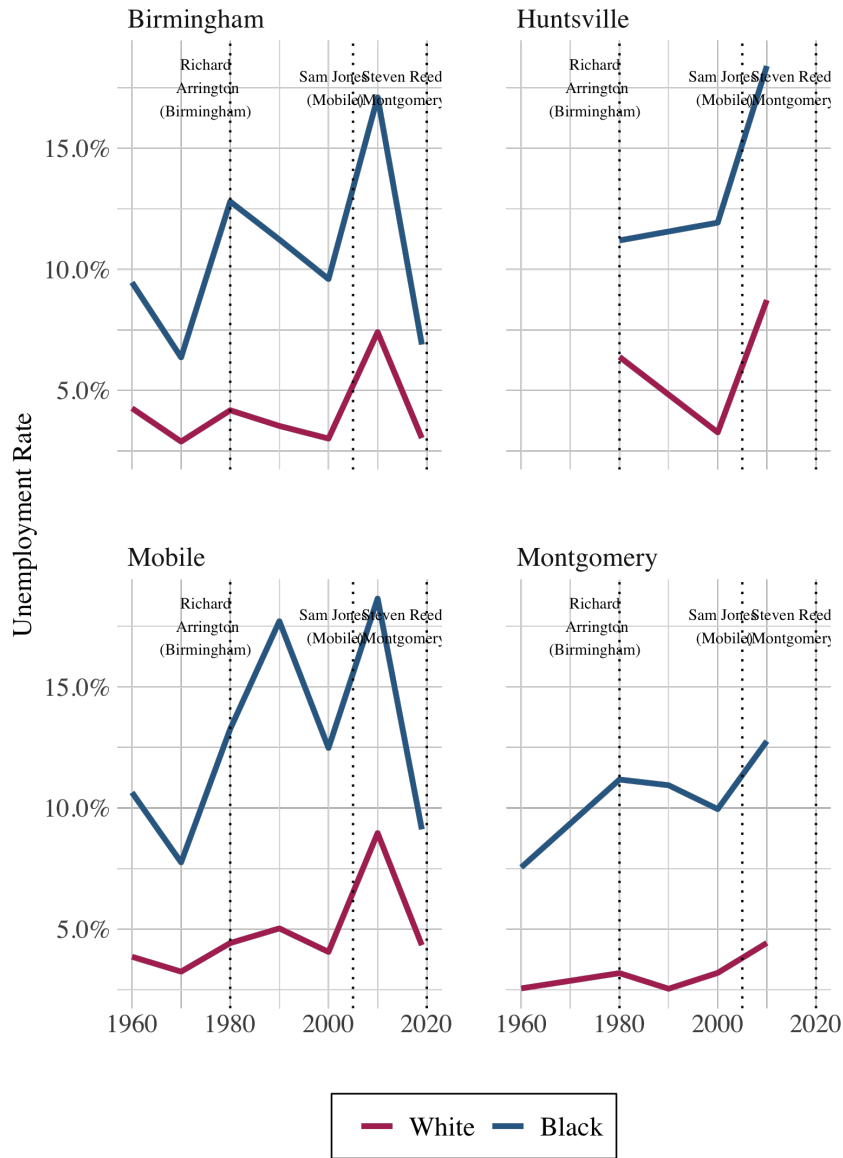


Figure 4.35: Unemployment Rate By Race: 1960-2019

This figure plots the average unemployment rate by race for residents of the four Alabama MSAs. The blue line tracks unemployment for Black residents, and the red line tracks unemployment for White residents. Dotted vertical lines indicate the year in which each of the three, city-first mayors assumed office. Data are missing for Huntsville (1960, 1970, 2019) and Montgomery (2020) due to lack of city or MSA identifying information.

the bounds imply relatively sizeable losses are possible as well. Each point estimate is of similar magnitude and not statistically significant suggesting that at least in the case of Alabama cities, increasing Black employment in local government through representational channels may be of limited use.

Self employment is another area where one might expect increases for Black residents. Governments play a large role in encouraging entrepreneurship through programs that provide resources or instruction for potential entrepreneurs, they are purchasers of goods and services through set-aside and government purchasing programs (Chatterji, Chay, and Fairlie 2014), and they set the rules for the potential uses of residents’ homes through land-use restrictions that can serve to inhibit entrepreneurial ambition (Enchautegui et al. 1997). Opposite to government employment, Black self-employment rates are always lower than White self-employment, often being half the White rate. Each city except for Mobile observes increases to self-employment over the period for both race groups. The Black–White gap in self-employment in 1980 was 5.6 p.p. and in 2000 remained there. Column 4 reports estimates of changes in self employment for Black residents in treated cities relative to comparison cities. Black self employment increased 0.8 p.p. in Birmingham after the Arrington election. Considering the 2.3 Black self-employment rate in 1980, the effect is statistically significant and economically large constituting a 35 percent gain in Black self-employment. However, a similar effect is not present in Mobile where Black self employment decreased 0.5 p.p. after the Jones election.

Column 5 asks the companion question to column 4: are Black residents more likely to be employed in occupations that disproportionately interact with the municipality. This may arise because cities institute diversity in employment requirements for local vendors to meet thus boosting minority hiring in relevant industries. Residents are labeled “affected workers” if their occupations are construction, office and administrative support, legal service, and building and ground maintenance. Birmingham experiences no changes to affected worker status after the Arrington election, and Mobile observes a gain of 1.5 p.p. gain in Black residents in affected occupations; however, the point estimate overlaps with zero. The lack of effect is possibly related to the fact that other factors may have larger influences than the election of a Black candidate. The targeted areas comprise nearly one-quarter of total employment for both races in 1980 and does not change much in the twenty-year period.

	L.F.P.	U.R.	Local Emp.	Self Employed	Affected Worker	Below P.L.
Birmingham						
Black Mayor x Black	0.008 *	-0.011 ***	0.003	0.008 ***	-0.002	0.006
	(0.004)	(0.003)	(0.004)	(0.003)	(0.006)	(0.005)
N	144773	104231	69444	69444	80049	78387
Mobile						
Black Mayor x Black	0.019 **	0.007	0.004	-0.005	0.015	0.004
	(0.010)	(0.009)	(0.008)	(0.006)	(0.011)	(0.011)
N	51623	37717	33509	39054	43643	41948

Table 4.23: Black Mayor Impact On Black Labor Market Outcomes

Notes: This table presents estimates of the relationship of Black, MSA resident outcomes and the election of Richard Arrington (Birmingham) and Sam Jones (Mobile). For Birmingham results, residents from all four MSAs are included and the time period is restricted to 1990 to 2010. For Mobile, residents in the Birmingham MSA are excluded and the time period is restricted to 1990 to 2010. The estimates are from canonical two-way fixed effects regressions with a Black mayor indicator, a quadratic an age, education, age–education interaction, and MSA and census-year fixed effects. Estimation restricted to those above 18 and weighted using census weights. Robust standard errors are reported.

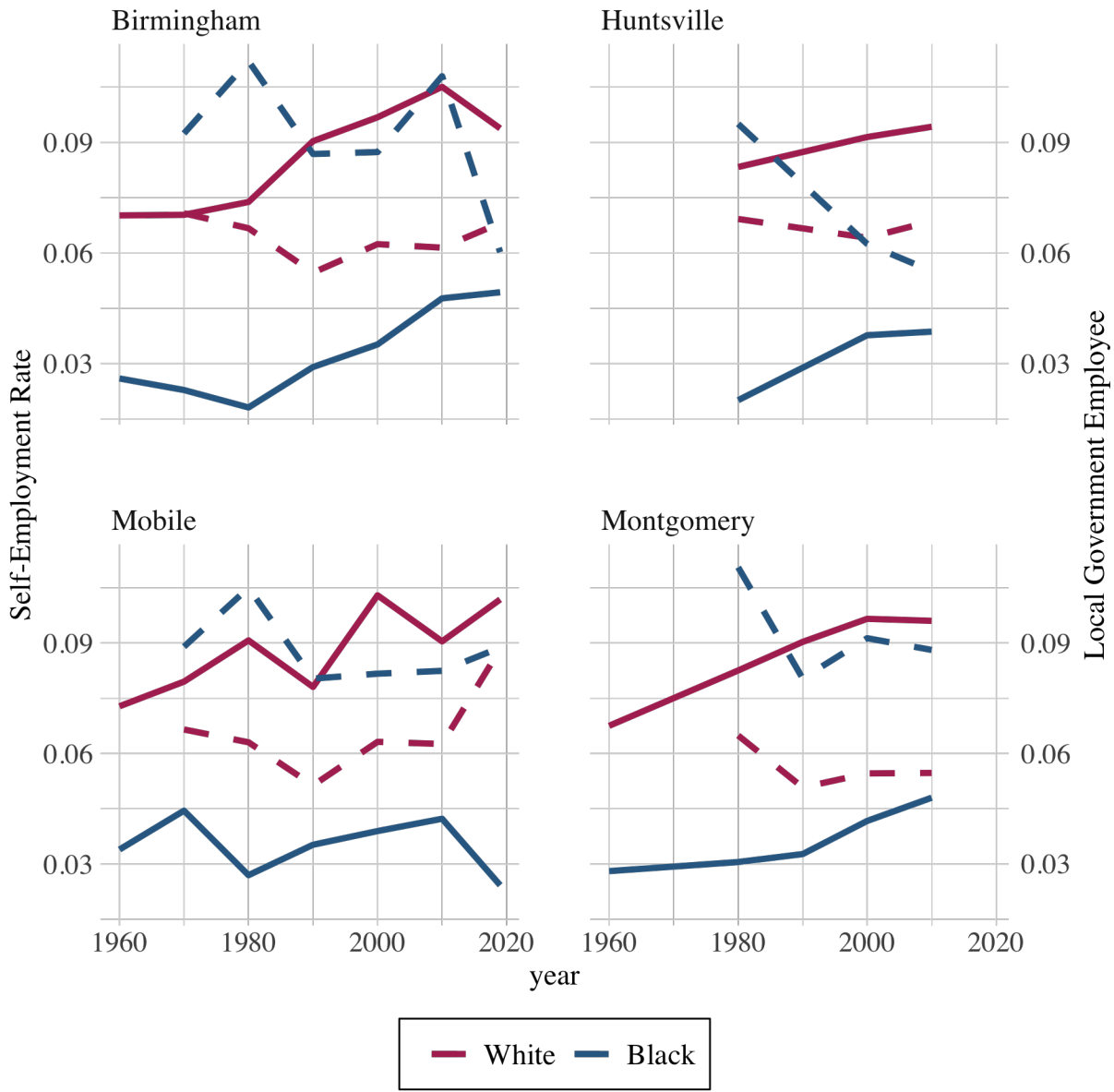


Figure 4.36: Self-Employment and Local Government Employment Rate: 1970-2019

This figure presents averages for self-employment and local government employees rate for four large cities in Alabama. The solid line depicts the self-employment rate, and the dashed line depicts MSA average for local government employees. Data are missing for Huntsville (1960, 1970, 2019) and Montgomery (2020) due to lack of city or MSA identifying information.

#### 4.4.2 Income

So far I've spoken to employment-related outcomes of Black residents in the wake of Black elections; however, improvements to employment prospects do not necessarily translate to higher income. One way in which we can test for this is to see if fewer Black residents live below the poverty line. The poverty line is a statistic used as a means test for welfare programs, so one can lose welfare benefits after earning income above the means threshold. This implies there is a range of earnings in which a resident would not be any better off— though the worker may derive other benefits psychic, financial, or otherwise— than from the previously unemployed outcome. Column 6 of Table 4.23 reports changes to residents that are below the poverty line after Black elections. In both the Birmingham and Mobile cases, the point estimate is positive but not statistically significant. Black residents living below the poverty line in Birmingham increases 0.6 p.p. which is lower than the number of residents that reenter the labor force. For Mobile, there is 0.4 p.p. increase which is also dwarfed by the estimate of Black residents that re-entered the labor force suggesting re-entry led to better outcomes for many residents.

The marginal resident in column 6 were those living near the poverty line; another way one might ask the question is what happens to the distribution of income for residents after the election. Figure 4.37 plots a kernel density function of income earned from wages for Black and White MSA residents for each of the cities by decade from 1970 to 2010. Huntsville is missing data in 1970 and 1990, and Montgomery is missing data in 1970. Black residents in all decades in all cities earn substantially less income than White residents. The gap in the difference in earned income shrinks some in Birmingham and Huntsville. This pattern is less visible in Montgomery and Mobile, though the gap in Mobile does shrink some. Interestingly, the highest average earned income for Black residents is in the earliest adopter, Birmingham. The reduction in Huntsville can likely be explained by their smaller Black population, and race relations being better relative to the other cities in Alabama.

Finally, a point made by Piliawsky (1985) is that not all Black residents benefit from the election of Black candidates. Wealthier, Black business owners may have connections to the mayor which prove useful in the post period, and members of the Black professional and educated class may find greater opportunities for employment within the administration or businesses reliant on government goodwill. Due to lack of skills or previous opportunity members of the Black underclass may be left behind and not enjoy tangible spoils of victory. Figures 4.38 and 4.39 report results from conditional quantile regressions at each decile of the positive earned income distribution by race for the Birmingham and Mobile samples. Results from each regression should only be taken as suggestive and not causal. Callaway and Li (2019) show that in order for quantile treatment effects in a difference-in-differences (DD) setup to be identified, researchers need at least three time periods of panel data where two periods are untreated and a stronger DD assumption, the distributional difference-in-differences assumption which is similar to the parallel trends assumption but for the entire distribution. Nonetheless, the figures provide a basis for judging who benefits the most from Black elections and a potentially fruitful avenue for future research.

Figure 4.38 shows the results for the Black and White wage distributions where inference is conducted using Huber sandwich estimates. OLS results are plotted with their 95-percent confidence interval for comparison. Black residents in Birmingham earn more income on average after the election as is evident from the OLS fit. This does mask some heterogeneity by income decile however. Black residents at the lower income deciles tended to earn more after the Arrington election relative to Black residents in other cities. The estimates decline as one moves up the income distribution which indicates that Blacks at the higher end of the Black income distribution were likely to do well regardless of who is in office. A perhaps odd finding, though consistent with narratives of the Arrington administration and consistently high polling numbers is that White residents did just as well if not better after the Arrington election as well. The estimates for the White earnings distribution are positive at all points of the distribution and the OLS estimate is similar to the Black estimate and more precise.

The pattern for Mobile is slightly different. Black residents' income at the lower deciles of the income distribution is higher than similar residents from comparison cities. At the median of the earnings distribution, the point estimate

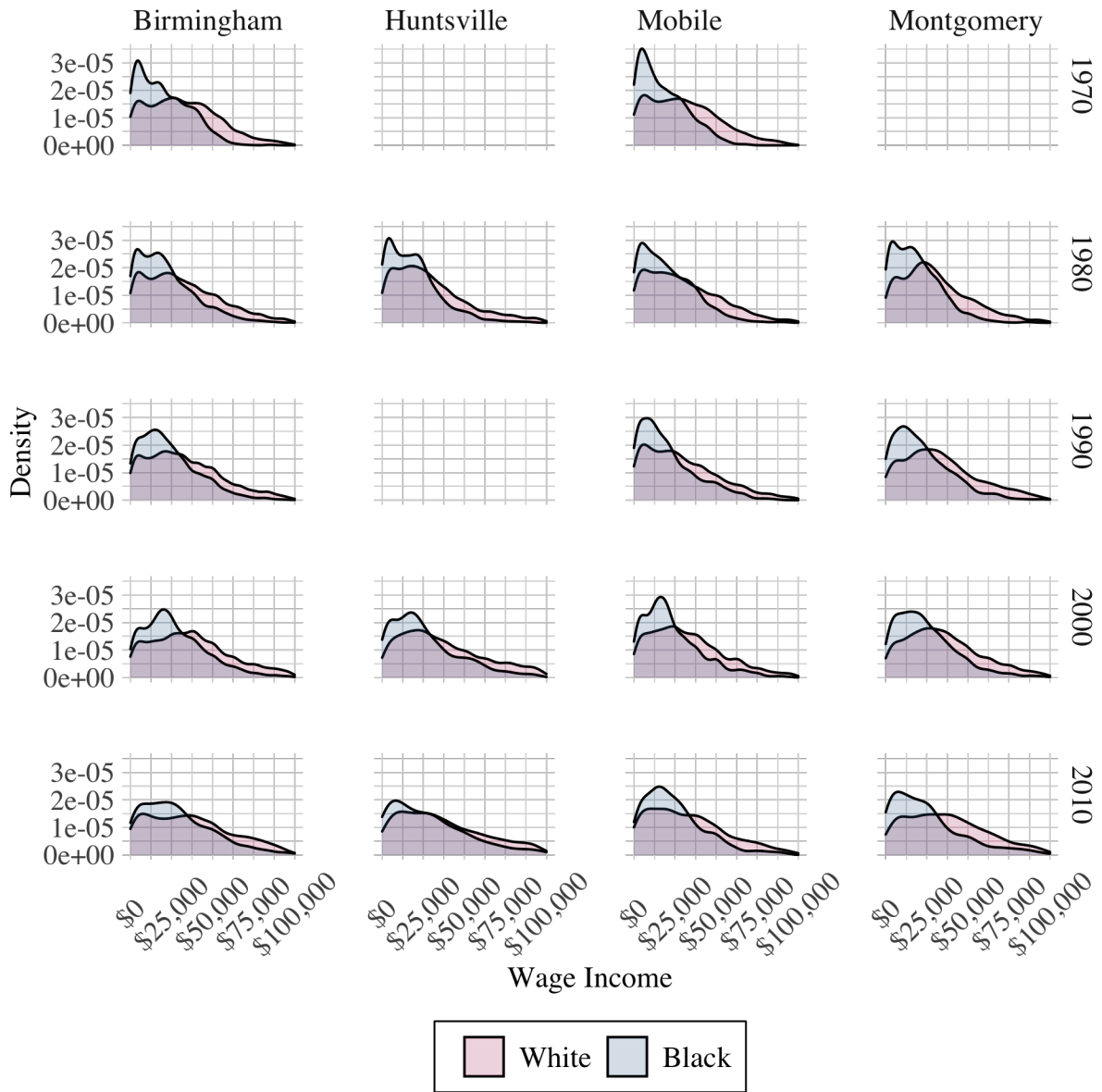


Figure 4.37: Distribution of Wage Income Over Time

This figure presents kernel density estimates for earned wage income by census year, MSA, and race. Income is deflated to 2010 dollars.



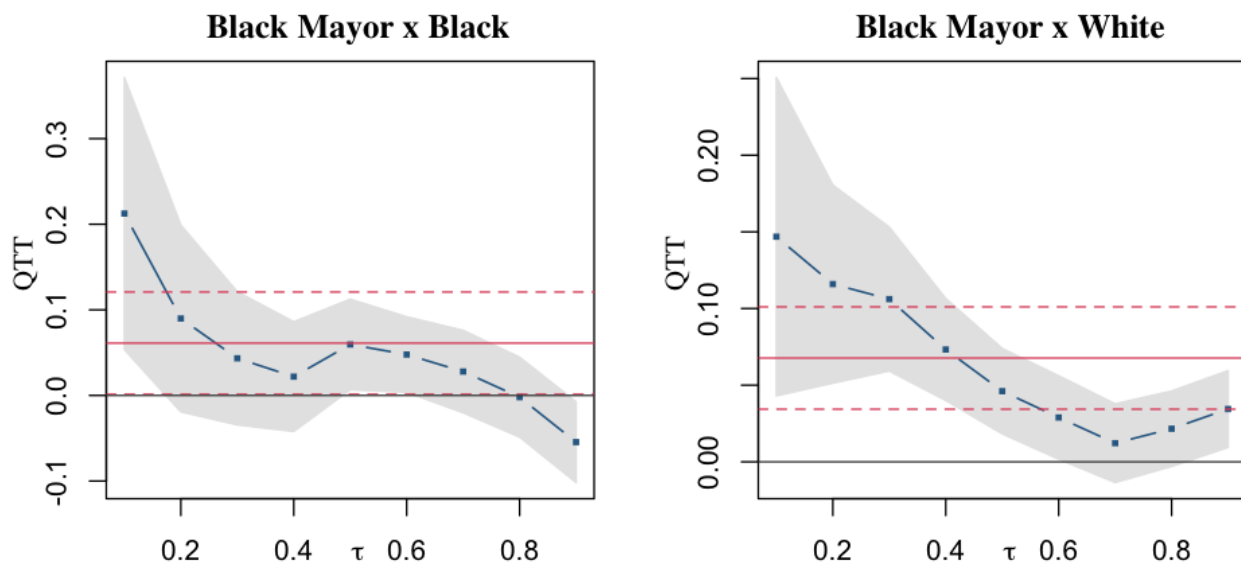


Figure 4.38: Quantile Treatment Effects of Richard Arrington (Birmingham) Election

Figure displays quantile regression estimates of the effect of the election of Richard Arrington in Birmingham on the Black wage income distribution in the left panel and the White wage income distribution in the right panel relative to cities in Alabama that had not elected a Black candidate by 2000. Estimates begin at the first decile and end at the 9th decile. Red solid line displays OLS estimate with red dotted lines and 95-percent robust confidence intervals. Results are for income distribution by each race and may look differently if estimated by jointly. Confidence intervals for quantile regression constructed from Huber sandwich estimates.

is negative but statistically insignificant and remains negative for the rest of the distribution. The OLS point estimate is similar to Birmingham at 8%, but the estimate is imprecise. Unlike White residents after Arrington, White residents at each White earnings decile after the Sam Jones election did worse than White residents at similar deciles in Montgomery or Huntsville. The OLS estimate is similar to the quantile regression results indicating Whites in Mobile earned less after the Sam Jones election. The results from both cities tend to contradict the findings from earlier, though this investigation does not speak to those unemployed or not in the labor force, a substantial portion of the Black population.

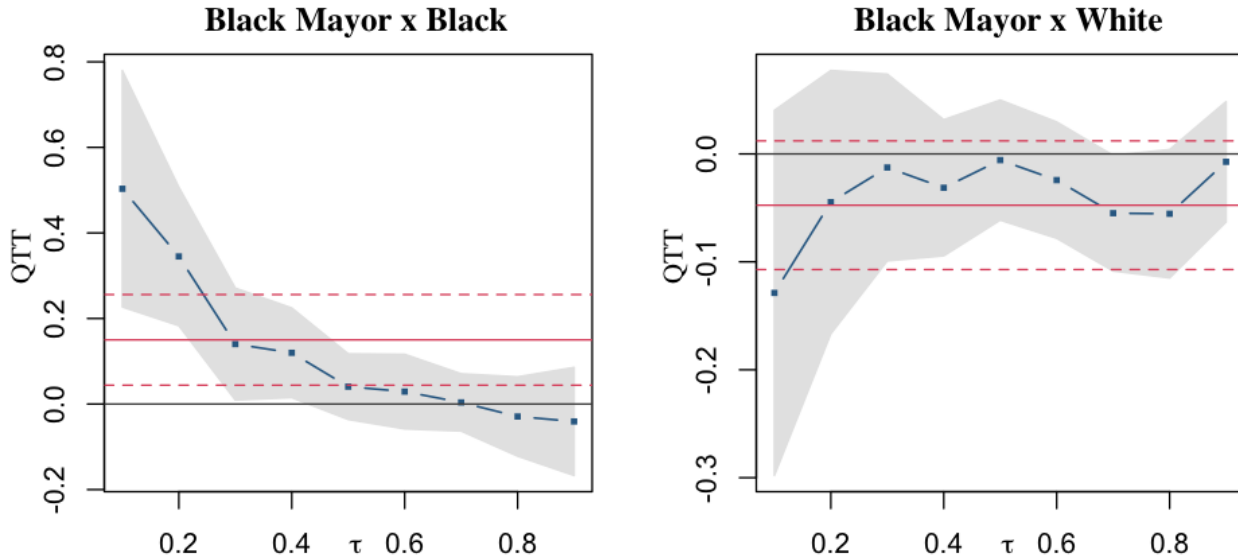


Figure 4.39: Quantile Treatment Effects of Sam Jones (Mobile) Election

Figure displays quantile regression estimates of the effect of the election of Sam Jones in Mobile on the Black wage income distribution in the left panel and the White wage income distribution in the right panel relative to cities in Alabama that had not elected a Black candidate by 2000. Estimates begin at the first decile and end at the 9th decile. Red solid line displays OLS estimate with red dotted lines and 95-percent robust confidence intervals. Results are for income distribution by each race and may look differently if estimated by jointly. Confidence intervals for quantile regression constructed from Huber sandwich estimates.

### 4.4.3 Businesses

An area where Black mayors are most likely to intersect on policy is Black business ownership. Business ownership has long been touted as the way to self sufficiency in the Black community and as DuBois (1899) states, “For a Negro then to go into business means a great deal. It is, indeed, a step in social progress worth measuring.” Figure 4.40 describes changes in Black business formation and revenue from 1972 to 2012. Data are from the Survey of Minority-Owned Business Enterprises which then became the Survey of Business Owners. The surveys collect information on owner’s race, the number of employees at each business, and sales/revenue from the enterprise. Panel A show the number of Black-owned businesses per Black resident in each city, and panel B shows the number of Black-owned employer firms per Black resident. For both time series the number of businesses are low consistent with low levels of Black business owners found in other cities. Black-owned employer firms are especially low rarely reaching 3 Black-owned firms per 1000 Black residents. The number of Black firms per capita is lowest in Birmingham until 2007 when Huntsville assumes the lowest spot. Huntsville, however, consistently has the most Black-owned employer firms per capita. The panels do not tell a consistent story on the effect of representation as

Birmingham and Mobile tend to have low rates of Black businesses, though the number of Black businesses does spike to its highest and Alabama's highest rate during the Jones term.

Panels C and D repeat the previous panels but take the survey-to-survey differences in the number of businesses to shed light the rate of change in Black business formation and if that is related to Black elections. The story is slightly different where Birmingham does exhibit the largest changes which abruptly halts after Arrington steps down from office in 1999. Mobile also exhibits a similar spike in formation around the Jones election spiking from a zero business change in 2002 to more than 2500 in 2007 which suggests that Black mayors are influencing, enticing, or encouraging Black business formation. This pattern is less evident when viewing it through the lens of employer firms, but there are so few firms that drawing strong conclusions may be impossible, and the cyclical nature of the series suggests that Black employer firms regress to their mean.

Panel E describes changes to the average number of employees at Black-owned firms. There are not large differences to the number of employees in each city, but Huntsville jumps in 1992. The number of employees reaches as many as 14 on average which suggest a large Black business is present in Huntsville. Data limitations prevent me from being able to identify the business and if the business is new or growing in success. At the end of the period, Mobile reaches a similar figure. The consistency of when the Black business figures spikes in Mobile strongly suggest the Jones administration succeeded in boosting Black enterprise. Lastly, panel F shows log annual receipts per firm. The panel seems to confirm the intuition of a large Black-owned business in Huntsville as average revenue in Huntsville paces all the cities in the state by a comfortable margin. The other cities in the sample appear similar in average revenue. Mobile's average Black firm revenue goes down after the Jones election which is consistent with new Black firms entering and facing typical obstacles new business face in order to grow.

#### 4.5 Crime & Police

Hopkins and McCabe (2012) finds that Black mayors tend to operate similarly to White mayors with the noted exception of hiring more Black officers. Sylvera (2022) finds a similar result noting that female officers increase as well. Indeed, Arrington certainly increased diversity within the ranks where the number of Black officers went from 166 in 1987, the first year in which the Law Enforcement Management and Administrative Survey reports officer race, to 478 in 2000. Department across Alabama increased their Black officer shares, but the increase in Birmingham was stark. In the three other cities, the share of Black officers increased by fewer than 10 p.p. in all cases. In Birmingham, the Black officer share increased by 31 p.p. Birmingham increased its share of female officers by 4 p.p. as well also outpacing the other cities.

Sylvera (2022) extends Hopkins and McCabe (2012) by asking if those changes result in different arrest outcomes for Black residents and finds largely that even with increased diversity in police ranks, Black arrests do not change in an appreciable way. Perhaps such a result should be expected; for instance, Reed's new hire for police chief, Darryl Albert, had a "hard on crime, kind on community" policy.<sup>78</sup> Pithy comment notwithstanding the position was no different than any of his predecessors despite Albert also being a Black man. While it does not appear that crime for *adults* changes, it remains underexplored if outcomes for *juveniles* changes. This is partly related to poor record-keeping by the departments. Departments across Alabama verify this problem as Birmingham reports arrests to the FBI from 1974 to 1990 and then for 2016 and 2018 using Kaplan (2020). Huntsville and Mobile report arrests from 1977 to 1989 and 2015 to 2018. Lastly, Montgomery reports from 1976 to 1986, 1988 to 1990, and 2015 to 2018.

With that being said I will focus on making comparisons between the Arrington administration and the other Alabama cities since data are mostly available then. Arrington has been trumpeted as lowering crime in his first terms as mayor. Juveniles can be handled by the police department in a number of ways: handled by the department and

<sup>78</sup><https://www.montgomeryadvertiser.com/story/news/2022/03/21/darryl-albert-named-new-police-chief-montgomery-alabama/7119873001/>

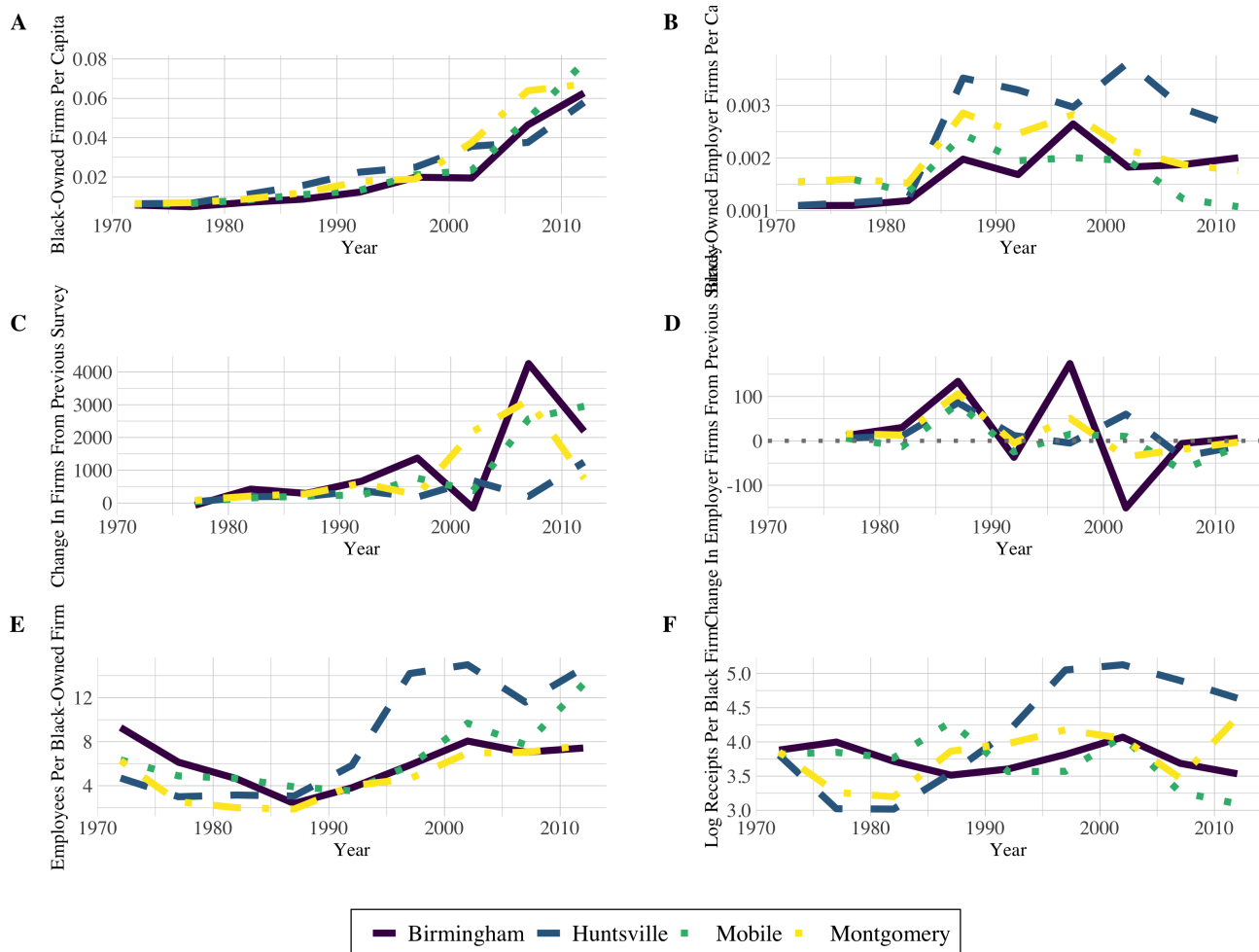


Figure 4.40: Select Black Business Statistics 1972-2012

This figure displays quinquennial, time-series data of Black-owned businesses extracted from the Survey of Minority-Owned Business Enterprises and the Survey of Business Owners from 1972 until 2012 when the survey was discontinued.

released, referred to juvenile court, referred to welfare and social services, and referred to criminal court. I'll focus on the first two, because they are reported by at least two cities. Juveniles handled within the department, those cases where the juvenile is released without charges, decreased by 105 cases in Birmingham and 553 in Montgomery in Arrington's first decade. The initial case levels were much higher in Montgomery where 1795 cases were handled within the department in 1980 and 496 cases were handled within the department in Birmingham; thus, there is little evidence on this dimension of policy separation. Juveniles referred to juvenile court are reported by all four cities across the decade ending in 1989 for Huntsville and Mobile. Similar to juveniles handled within department, there are not significant differences between the cities in their treatment of juveniles. Birmingham and Montgomery each increase by a small amount—271 for Birmingham from a 1980 base of 858 and 530 for Montgomery from a 1,911 baseline. Huntsville and Montgomery decreased from 1980 but the decadal trends do not contrast those found elsewhere.

Birmingham's Black population was already the highest in 1980 and grew over the decade; however, the total arrest rate (per 100K) for Black juveniles remained steady and lower than all but Huntsville despite having a Black population greater than three times Huntsville likely because of the increase in Black officers as well increased economic opportunity. Birmingham's juvenile arrest rate for Blacks was 315 per 100,000 in 1980 and decreased by one arrest over Arrington's first two terms. Over the same period, the Black population share grew 13 percentage points. Only Mobile observed a decrease in juvenile Black arrests but began at a rate of 631 Black arrests per 100,000. Montgomery, a city with a similar Black population to Mobile, arrested Black youths at almost twice the rate. Interestingly, Montgomery's department had 15 more Black officers (147) and fewer total officers though a history of civil unrest may affect policing culture in Montgomery. Black youths fared no worse in Birmingham than elsewhere in Alabama though the evidence hardly indicates Arrington provided panacea for Black youths. Police culture is inert and difficult to influence even with compositional and management changes, but the short timespan of data leaves hope that outcomes improved especially after the Great Crime Drop in the early nineties.

#### **4.6 Land Use, Outmigration, Annexation, Permits**

Authority over the use of land is within the purview of local authorities. The federal government rarely intercedes on issues of land use within municipal borders (Fischel 2015). However, issues of land use and annexation are oft-overlooked areas in electoral cycles. To some extent, we understand that White residents have been migrating out of cities after the second World War and because of changes to Black political power (Boustan 2010; Trounstein 2018), but it is less understood if politicians try to recapture or consolidate adjacent territories. Other work such as Collins and Shester (2013) and Rae (2003) recognize the role of local actors in clearing blighted areas and its effects on home values and local productivity. Arrington, Jones, and Reed all pursued policies of development, and Arrington and Reed, in particular, used the auspices of the government to seize or clear blighted land (Franklin 1989; Reed 2021). It is hitherto unclear if pursuing these particular policies varies differential by the race of the executive and limited data are available to uncover this relationship.

Using the census gazetteer files one can shed light on differences, if any, exist between cities in annexing land. Ten years after the beginning of the Arrington administration in 1990, Birmingham was composed of 148.5 square miles of land. From there it increased by 1.5 square miles, and in the 20 years after the Arrington administration ended, Birmingham gained 3.5 square miles of land area. While I do not have observations for other cities, over the course of the Arrington administration increased the size of Birmingham by 60 square miles, so much was likely done in the first decade.<sup>79</sup> Mobile's land area did not change from 1990 to 2000, but acquired 21 square miles of land in the decade after and nothing in the teens. This land acquisition pales in comparison to Huntsville which began as the largest city in Alabama by land area, and acquired nearly 60 additional square miles of land by 2020 to make it 220 square miles. Montgomery, on the other hand, grew 20 square miles from 1990 to 2000 to make it 155 square miles

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<sup>79</sup>[africanamerican.com/2014-honorees/richard-arrington-jr/](http://africanamerican.com/2014-honorees/richard-arrington-jr/)

and grew little from there.

While no definitive conclusion can be drawn from the size of the land, the cities with the longest histories of Black representation grew at the slowest rate. This could be because outmigration makes incorporating additional land a politically risky endeavor. Another reason is that annexation under Alabama law requires consent from 60 percent of the persons or firms that own the proposed land (Alabama League of Municipalities 2017). White residents are likely to oppose any effort of the municipality to be acquired, and Black residents may even oppose annexation if city services are substandard or their political will would be diluted.

Figure 4.41 shows the Black population by census tract in each metro area excluding the city limits from 1980 to 2010 as defined by their 1980 city boundaries. In general, there are few areas with a large Black population outside of the city. Montgomery is the exception to this, but this is related to Montgomery being positioned in the soil rich Black Belt, and nothing specific to the city itself. When Blacks do live outside of the corporate limits, they tend to occupy the inner ring suburbs like Fairfield and Bessemer in the Birmingham metro case. Whites were migrating out of most cities long before a Black candidate assumed the mayoralty. Birmingham lost 50,000 White residents in the decade before Arrington, and then lost an additional 65,000 White residents during the 20 years Arrington was mayor. Mobile lost 17,000 White residents from 1990 to 2000, and then another 12,000 by the end of Sam Jones' first term. Huntsville, as well, lost White residents until 1990, and then resumed losing them but at a much slower pace than Birmingham or Mobile over the decades. Montgomery gained residents of both races until the nineties when it began quickly losing White residents going from 106,000 in 1990 to 77,000 in 2010. The 2020 census reveals that this pattern continued, so in each case Black political power rose as White residents migrated.

While Birmingham lost population over the entire period, the other cities gained or maintained their population over the period. In both cases, issues of affordable housing, especially for Black residents, plague Black candidates. Single-family homes require little to no oversight in planning or construction. Multifamily homes are typically subject to review from planning and zoning boards and are subject to civilian approval as well as studies for traffic impact, environmental impact among other concerns citizens may conjure. Mayors can grease the bureaucratic wheel through appointment of citizens amenable to development policies. Review of the Building Permits Survey suggests Black mayors do have a pro-development bent. Data are available starting in 1988. Birmingham consistently produces multifamily units, but the largest amount approved is at the end of the Arrington administration with a farewell gift of 1,895 approved units in the last three years dwarfing the preceding and subsequent three-year periods by 1,741 and 1,491 units respectively. The Mobile case provides an even starker example where in the four-year term before Sam Jones was elected 432 units of buildings with 5 or more units were approved for construction. In Jones' first term which includes the beginning of the Great Recession 1,387 units in large buildings were approved. Only the rapidly growing Huntsville matched that output but also had a similar approval rate in the years before the Jones election. If that serves as any indication of racial differences in building output, Montgomery may expect to approve more than the 80 units in large buildings approved in the four years preceding Steven Reed.

#### 4.7 Conclusion

Civil Rights leader Ralph David Abernathy stated that "Blacks would not stop pushing until there was a Black in the mayor's office," after the first council election in Montgomery." Abernathy having been a major figure in the movement a decade earlier likely knew the difficulty behind this statement, and would not live to see Steven Reed become the mayor. Over the course of five decades, Birmingham, Mobile, and Montgomery would all elect Black mayors. After those elections, Black residents did enjoy some gains. Black labor force participation increased, unemployment decreased, and Black resident income increase especially for those at the lower end of the earnings distribution.

But in other areas were likely left disappointed. On the creation of Black-owned businesses, we observe some growth, but not dissimilar to Blacks in Huntsville. In some areas such as revenue, Black businesses in Huntsville

top the state. Black led cities tend to hire more Black and female police officers, but the changes in arrest rates are minimal. When focusing specifically on youth crime, Black juveniles are arrested more often after Black elections in some cases. The value of Black-owned homes increases in the case of Birmingham, but across all cities the value of Black homes is significantly lower than the value of White homes.

It should not be expected that Black mayors and legislators provide panacea upon their seating. While not a perfect quaesitum, descriptive political representation is oft-considered a necessary condition for political equality. Until Blacks have opportunities to achieve every office, political decisions made in the highest offices may be to the exclusion of Black residents which may foment resentment and backlash (Arnesen and Peters 2017). Huntsville, for example, has never had a Black candidate as a runner-up in a mayoral election. Tuscaloosa, not discussed here but another major city in Alabama, had a Black candidate finish second in the most recent election, but never a Black mayor despite nearly equal Black and White population shares. How that change to descriptive representation affects Black welfare and race relations remains an interesting question though this essay may provide some hints. Joe Reed upon leaving office in 1999 effectively summarized Blacks struggle for political equality: “It’s 20 years down, 100 to go.”

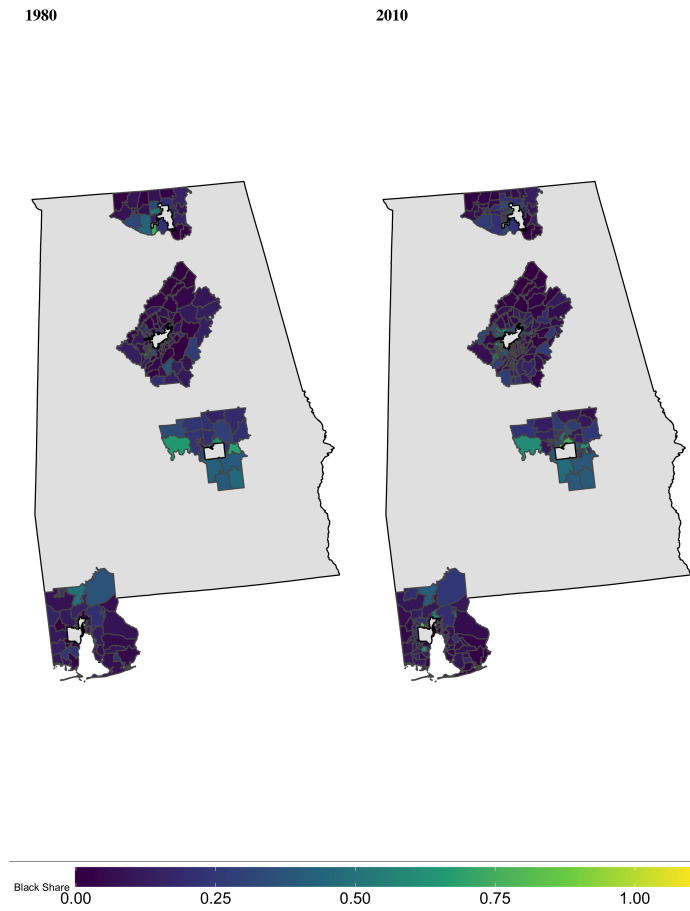


Figure 4.41: Census Tract Black Population Shares 1980 & 2010

This figure maps Black population shares to census tracts in 1980 and 2010 outside of four Alabama cities as defined by their 1980 boundaries. Metropolitan statistical areas are defined according to their 2000 definition. Moving from north to south the cities are Huntsville, Birmingham, Montgomery, and Mobile.

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