Federal Reserve Bank of Cleveland Working Paper Series

Black Mayors and Black Communities

Craig Sylvera

Black Mayors and Black Communities[*]

Craig Sylvera[†]

January 2025

Abstract

Do Black communities economically benefit from the election of a Black mayor? I find majority-Black ZIP codes experience gains in all areas of economic activity relative to non-Black communities following the first election of a Black mayor. Across industries, the number of establishments in majority-Black ZIP codes increases, including those that rely on foot traffic. Before breakthrough elections, Black residents are less likely than white residents to identify as self-employed across all cities, but this difference is reduced after an election; however, the cities in which the pre-breakthrough self-employment difference is larger experience no changes to the B–W self-employment gap post-election, suggesting institutional and historical factors may limit Black economic progress in places of higher disparity.

Keywords: Local Government, Mayors, Community Development
JEL Codes: J1, H7, J7, R5

A historical feature of American society has been that the average Black American does not live near many white Americans. Massey, White, and Phua (1996) conclude, "Among all groups, Blacks are uniquely segregated in U.S. urban areas," when reviewing segregation in the 1990s. Even at present, the average Black resident lives in neighborhoods that are composed of mostly Black residents (Frey 2021). Persistent racial segregation can create a drag on the economic mobility of Black Americans and their communities. For instance, businesses in Black communities operate in more precarious positions, having less cash-on-hand and lower profit margins than businesses in other ethnic communities, which contributes to a lower incidence of Black self-employment (Farrell, Wheat, and Grandet 2019; Hipple and Hammond 2016).

For these reasons, Black economic development has been the paramount policy of Black mayors (Bailey 1990). To improve local employment conditions, Black mayors in varied settings have enacted affirmative action and set-aside policies (Chatterji, Chay, and Fairlie 2014; Sugrue 1996), encouraged permissive local development policy (Kruse 2005), increased taxes to boost government services, increased public hiring of minorities (Nye, Rainer, and Stratmann 2014), implemented place-based policies to improve community conditions (Piliawsky 1985), and instituted programs to help disenfranchised and disadvantaged entrepreneurs navigate municipal bureaucracy.[1] A central question is, then, do the policies of Black mayors improve the economic conditions of Black communities?

In this paper, I examine the role Black mayors play in influencing the distribution of economic activity across a city. I study elections of a city's first directly elected Black mayor— hence, breakthrough —between the years 2000 and 2010. Breakthrough elections data are paired with County Business Patterns data on ZIP code totals (U.S. Census Bureau 1996-2020) that measure annual employment, establishment counts, and payroll at the ZIP-code level to determine the effect that breakthrough Black elections have on local economic activity by the racial composition of ZIP codes that I refer to as communities throughout. In order to determine the differential effect between majority-Black and non-majority-Black communities, I use a difference-in-differences (DD) design where I allow for heterogeneous effects between communities that are majority-Black or non-majority-Black. Therefore, I can identify the effect that breakthrough Black mayors have at the city level and for smaller geographies, which has not been studied thoroughly. This strategy also allows me to identify the relative difference in economic activity between Black and non-Black communities.

I find that following a breakthrough Black election, the number of establishments,

---

[1]Numerous cities have programs to assist female and minority business owners, e.g., http://www.city.cleveland.oh.us/CityofCleveland/Home/Business/EqualOpportunity#mbe_open, https://www.nola.gov/economic-development/supplier-diversity/, and https://www.cincinnati-oh.gov/inclusion/registration-certification/business-enterprise-certification/.

employees, and payroll per capita increase in majority-Black areas. The effect is found in cities where the mayor serves as the head of the executive branch (instead of an equal member of the legislative branch) and in cities where the mayor serves more than one term. In addition, the gains are robust to excluding partisan elections—that is, elections that are explicitly between a Republican and a Democrat—only comparing cities within the South or not in the South, or only comparing cities within the same state. Changes to the number of establishments per capita in Black communities are experienced both in industries that potentially interact with the municipality through the contracting process and in industries that rely on foot traffic. I conclude by finding a reduction in the Black–white self-employment gap, the difference between the rate at which Black residents and white residents identify as self-employed, but much of the reduction is concentrated in cities that already had small B–W self-employment gaps.

One difficulty in identifying the impact of Black mayors is that they are not assigned randomly. Traditionally, a researcher might use a regression discontinuity design comparing elections where a Black candidate narrowly wins relative to elections where a candidate narrowly loses, arguing that those outcomes were as good as random. However, there are two reasons to opt against this design in this setting. First, Vogl (2014) analyzes the validity of the regression discontinuity design for interracial elections involving Black candidates and finds evidence for endogenous campaign intensity at the vote share threshold for interracial elections in the South. Second, I lack the power to adequately detect even citywide effects in an RD setting because there are not a sufficient number of interracial elections over the sample period.[2]

I instead opt for a DD design in which I compare economic activity in cities that elect a Black candidate for the first time to a set of comparable cities that have not yet elected a Black candidate. Validity of the design requires that the evolution in economic activity between treatment ZIP codes (within a city) and control ZIP codes would have evolved in parallel absent the breakthrough election. In addition, I require that differences in economic activity between majority-Black and non-majority-Black ZIP codes within a city would have evolved in parallel absent the breakthrough election. I examine the validity of the design by estimating a "stacked" event study in which I treat each breakthrough election as its own sub-experiment where only a "clean" set of control cities is used in each sub-experiment. I align each breakthrough election in event-time beginning in the six years before a breakthrough mayor assumes office and the eight years following.[3] I pool the breakthroughs together and

---

[2]An extended discussion of this can be found in Section 3.1.

[3]Stacked estimation is robust to pitfalls from OLS using conventional two-way fixed effects such as heterogeneous treatment effects. For more see Section 3.

2

estimate the time path of economic activity after a breakthrough election. I, then, summarize my findings into a single post-breakthrough election treatment effect.

A second challenge comes from outcome measurement. If Black candidates are more likely to produce economic gains for Black residents specifically, then effects are obscured by observing outcomes at the city or metro level. I overcome this 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 other 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 and directing policy toward blocs may be more cost efficient and practical than individually targeted policies and may produce stronger political rewards (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 Black communities is an intuitive choice.

After a breakthrough election, I find gains of 0.76 establishments (3.4 percent) and 13.3 employees (3.5 percent) per 1,000 ZIP-code residents in Black communities relative to the pre-breakthrough average. Relative gains in Black communities persist eight years after the breakthrough election. These gains appear to operate through a redistributive channel as non-Black communities experience declines in both areas of economic activity echoing discussions in earlier work (Hopkins and McCabe 2012). In the aggregate, cities experience a 2.5 percent decline in employees per resident and a 2 percent decline in establishments in the baseline estimation, but this effect disappears in cities where the mayor is re-elected or serves as the chief executive.

I then turn to examine whether gains to the number of establishments are experienced in industries plausibly affected by municipal contracting, an area where the mayor exerts direct influence. Using the ZIP Code Business Patterns detail file (U.S. Census Bureau 1996-2020) which contains establishment counts by industry and establishment size measured by the number of employees, I find some evidence to support this conclusion; Black communities observe a 10 percent relative gain to the number of construction firms per capita, but the strongest gains come from establishment counts more reliant on foot traffic such as food services and accommodations. Non-Black communities experience declines in establishment counts across most industry groupings. In addition, I find economically and statistically significant evidence indicating the election of a breakthrough candidate induces relative and total gains to the count of establishments across all establishment sizes. Such an effect is

consistent with a Black mayor and their administration attracting or encouraging a large-employer establishment to locate in a Black community. Then, these large employers have downstream positive spillovers on the local business environment. Of course, this can be done in accordance with policies that accommodate smaller, lower capitalized businesses through purchasing programs, supplier diversity programs, and disadvantaged business programs.

Because I cannot rule out that gains observed in majority-Black communities are being experienced by non-Black residents—Black residents may still benefit from an increased consumption set caused by more establishments—I turn to the decennial census and American Community Survey to provide evidence that gains in self-employment, a proxy for entrepreneurship, are experienced by Black residents, including a set of controls for income, age, and education. I obtain the effect of breakthrough elections on self-employment by race for each breakthrough election.[4] I then relate those estimates with the city-specific Black–white self-employment gap in 2000. Black residents were 3.4 percentage points less likely than white residents to identify as self-employed. On average, I find a reduction in the B–W self-employment gap of 27 percent in the decade after a breakthrough election. I find small or no reductions in the B–W self-employment gap in cities that had large pre-existing B–W self-employment gaps. This suggests historical or institutional factors may limit the mayor's ability to move Black economic progress forward. That being said, 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 the impact of Black policymakers on Black constituent outcomes and racial inequality; however, my work is among the first to leverage where Black Americans live to provide understanding on how Black policymakers influence the distribution of economic activity. Zeng (n.d.) uses location as well to analyze migration decisions using close interracial elections in North Carolina. Nye, Rainer, and Stratmann (2014) address a similar question. Using a sample of Black mayoral elections from large cities from 1973 to 2004, the 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 policy goals that diverged from those of 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,

---

[4]In addition, Appendix Table A.4 shows that business income increases in Black ZIP codes using the Internal Revenue Service's public-use Statistics on Income.

4

the police force.[5] Earlier work from Hajnal (2001) and Browning, Marshall, and Tabb (1999) buttress the conclusion that Black mayors govern no differently than white mayors but could not speak to intra-city variation in race-specific outcomes. In addition, the effect of Black mayors has been researched in the non-American context by Ikawa et al. (2024), so this question is broader than even the American context in which it is often treated (this paper not withstanding).

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 two-way fixed-effects models in the presence of staggered treatment and 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 previous studies 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 1970s and 1980s that inherited cities with eroding tax bases and transitioning employment-sector composition (for example, 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 between 2000 and 2010.[6] 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.[7,8]

My findings add to a broader literature on candidate identity. Beach et al. (2024) 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

---

[5]A similar result is found by Sylvera (2023) and McCrary (2007), who discusses the effect of court-ordered affirmative action mandates on the composition of the police force. In numerous cases (For example, New Orleans, Cincinnati, Philadelphia), Black mayors are acquiescing to the injunctions that were previously ignored by white mayors.

[6]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."

[7]For more on changes in trends in post-breakthrough cities, see Appendix A.4

[8]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 face. See Kruse (2005) for a discussion of Maynard Jackson and Andrew Young in Atlanta, Sugrue (1996) for a discussion of Coleman Young in Detroit, and Piliawsky (1985) for a discussion of Ernest Morial in New Orleans.

in minority neighborhoods that 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. [9]

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 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 the racial composition of commuting zones during the Great Migration reduced long-run mobility. Derenoncourt finds that the Great Migration reduced intergenerational mobility for low-income families 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.

1 Background: 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 that grant them authority. Here, 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.[10] In mayor–council governments, the mayor serves as the chief executive of the government. The mayor appoints department

---

[9]For other work on descriptive representation, see Beach and Jones (2017), Pande (2003), and Sances and You (2017).

[10]County commission governments are a less frequent variant of council-manager systems where each elected official, who is referred to as a commissioner, typically operates as the head of a specific administrative function.

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) as well as other boards. In council–manager governments, the mayor is a council person whose powers in general do not differ from those of other council persons. Mayors in this form may preside over council meetings and may have veto power, though this power is not commonly vested. The executive department and day-to-day operations are left to a hired professional denoted as a city manager.[11] Grumbach, Mickey, and Ziblatt (2024) show that there is some endogeneity in municipal organization driven to reduce Black political power, a finding also noted by Aghion, Alesina, and Trebbi (2008) and Shah (2014). This work focuses on an earlier period than mine. Municipal organization is sticky, in the sense that it changes infrequently; so I take the arrangement as given in this context.

Given the differences in attributed power, 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 councilors persons of color when the first Black candidate becomes the mayor, and even this does not guarantee uniform policy agreement. In council–manager forms, this problem is amplified when each council person represents a specific district. In Tiebout's (1956) model 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. Peterson (1981) has described these kinds of policies as "redistributive," which are the set of policies that help disadvantaged residents at the expense of revenue-generating ventures. These ventures, which he calls "developmental," improve the economic status of cities because they potentially increase the tax base without disproportionately burdening wealthier mobile residents. Voters or businesses with limited political power alone can also 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 the mayor's 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, incentives, or resource reallocation from one community to another (also see Rose-Ackerman (1979) and Peterson (1981)). Rae (2003) echoes this constraint, commenting

---

[11]Think Chris Traeger from *Parks and Rec.*

7

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.

2 Data

2.1 Mayoral Data

Data on elections come from two primary sources. The first source is Vogl (2014), whose paper contains data on elections from 1965 to 2000 of US cities that had populations of at least 50,000 in 1960. The data include candidate race and party, year of election, city, and margin of victory. I supplement this with data from Ferreira and Gyourko (2009), who 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 election websites 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."[12] 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 1 displays the set of cities under consideration for this exercise separated by treatment status. The sample consists of all cities with populations greater than 100,000 in 1990 that have a Black population share of 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. I exclude all cities that elected a Black mayor before 2000 because if the election of a Black candidate produces any effects for Black populations, then we should be concerned about differential trends caused by them. In addition, it is also unclear if one should expect similar effects between the election of the first Black mayor and subsequent elections, especially because the subsequent elections are not evenly spaced across time. 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 17 breakthrough elections that cover all four census regions plotted with solid circles. The South is responsible for nine elections, followed by the Midwest with four; the North and

---

[12]For example, neither file contains information on elections for independent cities in Virginia, so I perused the Virginia state elections website which contains all elections beginning in 2000.

West each contain two breakthrough elections respectively. Control cities are plotted using hollow shapes. Cities that observe at least one interracial election after 1994, which are the primary comparison cities, are depicted with hollow circles, and cities that did not observe an interracial election but met the 10 percent Black population criteria are depicted with hollow squares.

2.2 ZIP Code-Level Data

The ZIP Codes Business Patterns (ZBP) provides annual data on the number of establishments, employees, and annual payroll for each ZIP code beginning in 1994. Establishments are physical locations where business is conducted and may not be identical to an enterprise.[13] The establishment count represents the total number of locations with paid employees at any time during the year in a ZIP code. 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. The Census Bureau asserts that the data "cover most of the country's economic activity," though there is a small amount of undercoverage (U.S. Census Bureau, n.d.). ZBP data are not subject to sampling error, since they are taken from the Business Register, a file of all known single- and multi-establishment companies. All ZIP codes with populations over 1000 residents in 1990 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 provides an industry-by-establishment-size breakdown of establishments in the ZBP detail file. 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 no similar NAICS category.

Demographic data at the ZIP-code level are taken from the IPUMS national historical graphical information system (NHGIS) which provides 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,

---

[13]An enterprise is a business with one (single-unit company) or more physical establishments (multi-unit company), for instance, Walmart.

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 choose 50 percent to obtain as many ZCTAs within a city as possible while staying above the majority within the ZIP code itself. 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.[14]

The ZIP code data are merged with 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 and 2010. 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 because they potentially serve as comparison units. The sample includes 17 treatment cities and 32 control cities or 623 ZIP codes, 109 of which are majority Black.

2.3   Summary Statistics

Table 1 separates the sample by treatment status and reports the mean and standard deviation for select city-level and zip-code-level characteristics. The final two columns contain the difference between treatment and control means and reports the p-value for significant differences in means between treatment and control. The first panel presents statistics at the city (treatment) level in 1990. If treatment cities collect more revenue than control cities, then this allows them to more effectively implement policies or provide government services that contribute to local productivity. Examining revenue measures—property tax, sales tax, and total revenue per capita in thousands of dollars—treatment and control cities are not statistically different when comparing property and sales taxes per capita. For both, the differences are about $30 per resident, and the difference across property and sales taxes nets to zero. For total revenue collected, control cities on average collect more revenue but the variation within treatment status is large, leading to a non-significant difference. Each group devotes roughly the same amount of resources to police protection. As should be expected, treated cities have higher Black populations and, in turn, lower white populations. This motivates a design that inspects areas within a city that are closer in terms of Black population. Important to the design, nonetheless, is that there are no differential trends in

---

[14]"ZIP" refers to a lapsed USPS servicemark meaning "Zone Improvement Plan."

Black population growth between treated and control. I show in Appendix Figure A.9 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 arrangements, as shown by the share of cities with mayor–council governments, and their Black–white dissimilarity score, a measure of segregation. Dissimilarity measures the evenness of dispersal between two Black residents and white residents. The index can be interpreted as the percentage of either Black residents (or white residents) 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. Untreated cities have more outstanding debt on average, but also exhibit substantial dispersion as indicated by the standard deviation.

The second panel summarizes ZIP-code characteristics from 1994-1999 by treatment status. There are a similar amount of majority-Black ZIP codes between treatment and control groups, which is important in this context because I want to compare similar ZIP codes to each other. The average ZIP-code populations are also close before any elections. Overall, there are no statistically significant differences between treated and untreated ZIP codes beyond the Black and white population that the city-level average identified. In general, economic activity is greater in the untreated ZIP codes on average, but the estimates cover a range of values that likely reflect differences in land use rules and resident preferences across geographies. In the subsequent analysis, unit fixed effects will capture much of this variation on the presumption that land use policies are sticky and neighborhood preferences are not changing that much over time.

Appendix Table A.1 contains non-exhaustive data on breakthrough mayors and their opposing candidates before the breakthrough candidate was elected. Breakthrough mayors assume the mayoralty with more than 12 years of legislative experience. This reflects elected positions in municipal, county, and state government.[15] This is about 3 years more than the candidates they face, but this number is skewed by some candidates with more than 20 years of public service. 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.

---

[15]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.

City council experience is not different between the two groups, with each averaging slightly more than a traditional four-year term, but the runner-up white candidates were more likely to ever have served on the city council beforehand.[16]

3 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 where 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 Appendix Figure A.6, 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 17 breakthrough elections. Each event consists of one breakthrough election in which a Black candidate is elected mayor for the first time. 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 17 breakthrough elections aligning them in event time.[17] Stacked estimation has two critical advantages over traditional two-way fixed effects estimation that the 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

---

[16]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, and 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.

[17]This procedure is not dissimilar to what is proposed by Wing, Freedman, and Hollingsworth (2024).

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.[18] These are salient concerns because policy— if it works—is unlikely to resolve in a single period, likely operates on a lag, and can compound. [19]

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 is changing differently than in 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

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

where y_{ezct} are economic outcomes for breakthrough election (event) e in ZIP code z in

---

[18]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 A.1. I use the R function `bacon` from the `bacondecomp` package to estimate these weights.

[19]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 deChaisemartin and D'Haultfœuille as well), the probability limit of the two-way fixed effects estimator can be expressed as $plim_{N\to\infty}\hat{\beta}^{DD} = VWATT + VWCT - \Delta ATT$ where $VWATT$ is the variance-weighted average treatment effect on the treated, $VWCT$ is the variance-weighted common trend, and ΔATT 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 β^{DD} is attenuated, though in other cases the estimated treatment effect may even be negative. Appendix Table A.7 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.

city c in years $t = 1994, ..., 2018$ such as the natural log of establishments, employment, and annual payroll per capita. α_{ez} are a set of ZIP code–event fixed effects that capture observable and unobservable time-invariant differences between ZIP codes. δ_{et}^r are a set of year–event by race$\in$\{majority Black, non-majority 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 of majority-Black ZIP codes in 1990 to majority-Black ZIP codes in 2000 is .84 and to majority-Black ZIP codes in 2010 is .69, so there is persistence in the explanatory variable. Because the dichotomous indicator will not capture relative effects for three cities whose Black populations do not meet the majority threshold, I also report in Table 3 results where the explanatory variable is the 1990 Black population share interacted with treatment and results by quartile in Appendix Figure A.8. I conduct my analysis across 17 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 city-by-event (breakthrough election) level and additionally report 95 percent sup-t critical values, uniform confidence bands that adjust for multiple testing.

Estimation of equation (1) provides a description of the effect the election of a breakthrough Black candidate has on non-Black communities, captured by λ_k, and the effect it has on the racial gap between communities in the same city, captured by τ_k. The absolute effect on Black communities is found by the linear combination of λ_k and τ_k. The leads on these coefficients (λ_k, τ_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 report Wald tests results that explicitly test for this.

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 that 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 modal stay in office is 8 years. Thus, this specification closely captures the political lifespan of a mayor, and the resultant effect on the local economy.

I then summarize the results by examining the effect from the election of a breakthrough

14

candidate by collapsing the post-treatment effect into one coefficient and capturing the slope of the pre-election effects to improve precision. I estimate the following equation

$$
\begin{aligned}
y_{ezct} =& \alpha_{ez} + \delta_{et}^r + \tau_1 BM_{ect} + \tau_2 BM_{ect} \times BZ_{ezc} + \gamma_1(t - B_{ec}) \times \mathbf{1}\{t < B_{ec}\} \\
& + \gamma_2(t - B_{ec}) \times \mathbf{1}\{t < B_{ec}\} \times BZ_{ezc} + \varepsilon_{ezct},
\end{aligned} \tag{2}
$$

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. γ_1 shows the pre-breakthrough trend in non-Black communities, and γ_2 shows the Black-to-non-Black community difference in the 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, τ_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 τ_2 describes the relative effect a breakthrough election has on majority-Black communities as a deviation from any pre-breakthrough trend. Together, $\tau_1 + \tau_2$ describe the total effect the election of a breakthrough candidate has on majority-Black communities as a deviation from a 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.

3.1 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—that is, the share of votes a Black candidate received greater (less) than the next closest non-Black candidate, for example—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 (2007) 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 that did elect 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 restriction that no Black candidate had been elected before 2000, includes 45 cities encompassing 99 elections between a Black and a white candidate over the period. In order to identify an effect in the RD setting with that sample, the effects would need to be, depending on the dependent variable, 1.75 to 7.1 times larger than the DD estimates. This is even before considering identifying heterogeneous effects.

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 that 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 assumes that policy priorities are efficiently implemented. In the second case, no dynamics in economic activity is a dubious assumption that 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 in Appendix Figure A.11. Then using the restricted sample, I provide explicit power calculations that show that RD estimates are underpowered to detect even the aggregate effects that I find in the DD context in Appendix Table A.5. Finally, in Appendix Tables A.6 and A.7, I show that estimates using both conventional two-way fixed effects estimation and a lagged independent variable lead to incorrect inference. When I present estimates using the lagged independent variable, the Black mayor indicator is not treated as an absorbing state in contrast to my stacked estimation; so it turns on the year after taking office and turns off the year after leaving office. TWFE tends to be near the aggregate effect, but the heterogeneous effect is attenuated, and the precision of lagged estimation is greatly improved when accounting for dynamics in the dependent variable rejecting case (2).

4 Breakthrough Candidate Effect of Majority-Black Communities

Local governments and, in turn, the mayor exert control over the built environment through land use policies. Given that the data pertain to private businesses, it is likely that mayors have a stronger effect over the number of businesses than they do over private-sector employment, though they can exert influence over that as well. Figure 2 presents event-study estimates of equation 1 weighted by 1990 ZIP-code population. Panel 3a plots λ_k and τ_k, and panel 3b plots the sum of the coefficients for each year, which is the total effect in Black communities.[20] The red lines depict the common effect, λ_k, which I interpret as the effect for white (non-majority Black) ZIP codes, and the blue lines depict the relative effect, τ_k, that is the difference between (majority) Black and white ZIP codes. In each panel, I print the pre-breakthrough, ZIP-code-level average establishments per capita for all ZIP codes and majority-Black ZIP codes, respectively. The solid horizontal lines show the average treatment effect on the treated after a breakthrough election. The bottom right of each panel shows p-values from Wald tests for differential pre-trends. Dashed lines are 95 percent pointwise confidence intervals, and the dotted lines are 95 percent uniform confidence bands using sup-t critical values, and both are clustered at the city-breakthrough level.

Establishments per capita in non-Black communities exhibit no pre-trends, which is also evidenced by the Wald test p-value. Prior to a breakthrough election, estimates for establishments per capita are precise nulls, and then establishments per capita experience a slow decline in the eight years following breakthrough elections. 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. Establishments exhibit no growth in the first period and then sharply jump 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 0.5 businesses per 1000 ZIP-code residents in non-Black communities and a gain of roughly 1 business per 1000 ZIP-code residents in Black communities at the end of the first term. The dynamics for the total effect in Black communities indicate that the effect slightly diminishes over the sample window. By the eighth year after a breakthrough Black candidate assumed office, establishments per capita are 3 percent higher than they were the year before election.

---

[20]Alternatively, one can estimate equation 1 with only the Black ZIP-code interaction and find equivalent results.

Figure 3 presents event-study estimates of equation 1 weighted by 1990 ZIP-code population of changes to employment per capita after a breakthrough election. In panel 3a, employment in non-Black communities exhibits no pre-trends, a finding that is supported by the non-significant Wald test p-values, and the point estimates on the leads are close to zero. In the post-period, there is a small decline in employment per capita in non-Black communities, reaching approximately 2 percent after four years. Black communities gain relative to non-Black communities. 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 5 percent absolute gain at the end of a presumed four-year term shown in panel 3b, though I cannot rule out no gains in employment per capita. In the initial year where a breakthrough Black mayor takes office, employment spikes 3 percent, which may be related to exuberance in Black communities in the wake of a breakthrough election. Though estimates become noisier over the sample window, employment gains eight years after the breakthrough remain similar to the initial spike.

Figure 4 plots the event-study results for payroll per capita. Consistent with the previous results for non-Black communities, payroll per capita experiences small declines in non-Black communities over the first term, and this decline continues into the subsequent four years, as depicted in panel 4a. The pre-trends p-value for non-Black communities is marginally significant, but the point estimates themselves are precise nulls. The blue line in panel 4a displays relative growth in annual payroll per capita in Black communities. Over the first four-year term, payroll in Black communities relative to non-Black communities steadily grows to 10 percent more than the year before breakthrough, and relative gains remain at slightly more than 10 percent eight years after a breakthrough election. In addition, the pre-trends p-value for the relative difference is not statistically different from zero, and the point estimates are also close to zero. When we observe total gains in Black communities shown in panel 4b, payroll in Black communities grows nearly 7 percent over the first term, and moderates to 5 percent eight years after a breakthrough Black mayor takes office; however, I cannot rule out small negative effects on payroll per capita throughout the window.

Table 2 estimates equation 2 summarizing the findings from the event studies into one post-treatment coefficient in columns (1)-(9). Columns (1), (4), and (7) show the citywide effect—the average effect for each treatment ZIP code without allowing for heterogeneous effects—for the log of establishments, employees, and payroll per capita. Columns (2), (5), and (8) estimate a standard DD allowing for heterogeneous effects by ZIP-code racial composition, and columns (3), (6), and (9) estimate a DD model that allows for heterogeneous effects and estimates a pre-election trend for all treatment ZIP codes and a differential trend between treated majority-Black ZIP codes and non-majority-Black ZIP codes. Below those estimates I

print the pre-breakthrough averages for each dependent variable in columns (1), (4), and (7). Then, in the columns that allow for heterogeneous effects, I display the linear combination of the non-Black ZIP-code effect and the relative Black ZIP-code effect which, produces the total effect in Black ZIP codes.

Beginning with the citywide effects of breakthrough Black mayors on economic activity, Table 2, column (1) shows that ZIP codes in cities that elect a breakthrough Black candidate experience a 2 percent reduction in establishments per capita, which evaluates to a 0.45 establishment per capita reduction. Employees per capita in treatment ZIP codes, shown in column (4), decline by 2.5 percent after a breakthrough election, which evaluates to a loss of 9.5 workers per 1000 ZIP-code residents at the pre-breakthrough election mean. Confidence intervals for employment and establishments per capita rule out any positive effects for the aggregate effect. Payroll per capita, shown in column (7), declines by 1.4 percent but the confidence interval cannot rule out an effect as large as a 3.3 percent increase in payroll.

The losses in both cases are borne by non-majority Black communities, which is evinced in columns where I allow for impacts to differ by the Black population of the ZIP code. I begin the analysis with establishments and find robust growth in establishments per capita in Black communities, possibly 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 (2), non-Black communities observe a 3.1 percent decline, and majority-Black communities observe a 6.2 percent relative increase in establishments relative to non-Black communities. Both estimates are statistically significant. Combining the estimates, the total effect in Black communities is a 3 percent increase in establishments per capita, which is also statistically significant. Adding the pre-breakthrough trend in column (3), the trends are not significant and do not qualitatively change the ZIP-code point estimates. The effect in non-Black communities decreases in magnitude some, but the effect is a statistically significant decline of 2.2 percent, implying a reduction of 0.5 businesses per 1000 residents, and the relative effect in Black communities is 5.5 percent, implying a gain of 1.23 establishments per 1000 residents relative to non-Black communities. The absolute effect on Black communities is a statistically significant 3.4 percent, which indicates an increase of 0.76 businesses per capita in Black communities (differences between linear combination due to rounding) over the eight-year period after a breakthrough candidate. The results can neither confirm nor deny that there is increased survival among existing firms or that there is increased entry (exit) in Black (non-Black) communities, because I cannot identify and follow individual firms.

Employees per capita, shown in column (5), in non-Black communities decreases approximately 3.8 percent in the wake of a breakthrough election, and majority-Black communities

19

gain 7 percent relative to non-Black communities, though I cannot rule out a zero effect for the relative gain. The effect on employees per capita in non-Black communities is virtually unchanged after the inclusion of pre-election trends at a 3.5 percent decline, and both pre-election trends are near zero, lending credence to the design. The point estimate for the relative effect in Black communities is identical to the estimate excluding the pre-election trends, and the total effect in Black communities is slightly larger with the inclusion of the pre-election trends in which I estimate a statistically significant 3.5 percent increase in employees per capita, though the absolute gain remains not statistically different from zero. This is consistent with the findings from Figure 3b, where all the point estimates are positive but not different from zero.

Table 2, columns (8) and (9) display estimates for changes in annual payroll per capita allowing for effects to differ in majority-Black ZIP codes. Across both columns, payroll decreases in non-Black communities—2.9 percent for column (8) and 3 percent in column (9)—but neither effect is statistically different from zero, and the confidence intervals cannot rule out positive effects as large as 2 percent. Payroll per capita declines in non-Black communities are somewhat offset by payroll gains experienced in majority-Black communities, which gain 8.4 percent relative to non-Black communities, which is identical in columns (8) and (9). The relative payroll gains in column (9) are marginally statistically significant, and the pre-election trends are effectively zero. Black communities experience a total gain of 5.3 percent, but I cannot rule out a payroll per capita decline as large as 1.6 percent.

We can also take the difference across corresponding columns to understand firms' employment behaviors after a breakthrough election; 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, while the reverse is true if employment declines exceed establishment declines. This gives another metric by which to gauge economic improvement within a city across communities. Combining columns (6) and (3), non-Black communities experience a non-statistically-significant reduction in employees per establishment of 1.4 percent (s.e. = 2.7%). Turning to Black communities and remaining in columns (3) and (6), the total gains in employment exceed the gains from establishments by .4 percent, though these within-establishment gains are indistinguishable from zero. Another transformation we can take is the difference between columns (9) and (6), which describes the change in wages within communities after a breakthrough election. In non-Black communities, there is no change in the average wage (τ_1=.4%, s.e.=1.2%), while in majority-Black communities we observe a total effect of 1.8 percent (s.e. = 2.0%). Given that each community is not an independent labor market, cities themselves are a part of broader labor markets, and the city-level results suggest wages do not change much ((7)-(4)),

20

it is not surprising that there is not precise movement in zip-code level wages.

4.1 Robustness

Table 3 shows effects using alternative specifications, subsetting the data by municipal government structure, and subsetting the data according to electoral rule. Column (1) reprints my preferred specification for each outcome of Table 2, that is, the specification including pre-breakthrough trends. Column (2) excludes all events where the breakthrough mayor is not the head of the executive branch; in other words, I exclude all council–manager and commissioner forms of government and retain all mayor–council governments. If there is direct transmission from executive policy to outcomes, one would expect it to be stronger 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), which is composed of nine breakthroughs in which the mayor is chief executive, 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. Though not included here, doing this same exercise but with the set of mayors who do not have executive powers produces generally noisy results of the opposite sign (except for the relative effect on payroll per capita). 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 establishments per capita as, the municipal government has direct say on land use within jurisdiction, and this is conveyed in the results in panel A, especially in the total effect in Black communities presented at the bottom of the table, which experience a statistically precise 3.8 percent gain on average after a breakthrough election. In addition, total Black community employment and annual payroll per capita, displayed at the bottom of the table, also increase more in mayor–council breakthroughs, but those estimates are not statistically different from zero.

Column (3) subsets the data retaining only non-partisan elections which excludes those elections, definitively between candidates of differing parties. While non-partisan elections do not guarantee that 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 in panel A, the relative point estimate is similar to the base estimate, and the non-Black community estimate is 1 percentage point lower than baseline. Across employment in panel B and payroll in panel C, the relative results are not statistically

significant; however, the magnitudes are quite similar to the baseline estimates. 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. The absolute Black community effects are not statistically significant, but remain positive and near the base results.

Column (4) retains the 11 events where the mayor was re-elected. Logan (2020) demonstrates that after the election of Black policymakers during Reconstruction, 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 is perhaps intuitive to believe that mayors who spend more time in office are more likely to get more done. More time allows for policies to work and not be reversed or disregarded; mayors can accrue greater influence, which has the ability to influence down ballot elections; and mayors have the opportunity to appoint more people to boards, which can ease policy paths. The results from column (4) are consistent with this intuition and of larger magnitude (for the relative and total Black effects) than even the mayor–council estimates. Notably, the effects in non-Black communities are not statistically different from zero across all outcomes, and the total effect in Black communities is 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 not in the South are different, and Black communities not in the South serve as poor controls. I specifically address this by including a South×year fixed effect that allows for differential effects by South-census versus non-South-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 relative effects in Black communities are positive and statistically significant. The total Black community effects remain positive, but I cannot say economic activity in Black communities is different from zero.

Column (6) presents, perhaps, the strictest test in which I restrict the comparison group to cities within the same state with the inclusion of state-by-year fixed effects. Here,

22

identification comes from cities in the same state, one that elects a Black mayor in a given year and the others that had not yet elected a Black candidate during the event window. This is a restrictive model since 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, regional 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. The magnitudes of declines in non-Black communities are larger across categories. The total effects at the bottom of column (6) remain positive but are smaller in magnitude. 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 for each ZIP code in 1990. Therefore, τ_1 (BM in the table) still captures the common effect across all ZIP codes, but τ_2 (BM × BZ in the table) now describes the linear relationship of going from no Black residents in a ZIP code to all Black residents in a ZIP code. The linear combination is then the total effect in a ZIP code populated only by Black residents. Both the absolute and relative effects are unchanged by this addition. Estimates in column (7) tend to be noisier than the baseline specification, lending support for the non-linear indicator, yet the estimates are qualitatively consistent with the results from the baseline specification.

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 provide evidence against this interpretation, 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 that have 1990 populations greater than 100,000, that have 1990 Black population share of at least 10 percent, and that 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 had they finished first. 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 criterion, I choose the election where the vote share for the Black candidate is the highest.[21] I estimate

---

[21]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.

equation 2 and report the results in Appendix Table A.2. In each column, we observe that the estimates are negative or small and always imprecise. This contrasts with the results from Table 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 on Black communities.

5 Channels

5.1 By Industry

Minority businesses encounter numerous impediments to forming and growing businesses such as lack of financial, social, and human capital in addition to a lack of access to markets outside of their immediate geographic location. Minority firms also face barriers to participating in the government contracting process due to capital limitations and limited information on contract competitions to offset the 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 examining 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 in *Croson v. Richmond (1989)*, these were ruled unconstitutional under the 14th Amendment's equal protection clause, and now hew to very narrow definitions (hence disadvantaged). In addition to DB programs, mayors and (potentially appointed) 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.

I test for evidence of this by using the ZIP Code Business Patterns detail file. The file includes data on the count of businesses by industry, which is further broken down by establishment employment size, for example, the number of businesses in manufacturing with employment between 45 and 49 in a given ZIP code. To begin, I aggregate the total count of businesses by ZIP code and then by industry. Because in many cases the number of establishments per capita for some industries will be small, I group sets of industries together according to how likely they are to directly interact with the government through municipal contracting, but I also include a separate category for construction because it is the largest recipient of government funds through contracting (Katz 2015). I follow Chatterji,

Chay, and Fairlie (2014) and construct a group of industries that are "affected" by city government contracts: construction; professional services; transportation and warehousing; retail trade; other services; utilities; and administrative, support, and waste services. They identify these industries using sources that have confidential data on businesses that sell goods and services to the government. I label this group as "broadly affected." I narrow the set of potential firms affected, including only those firms most frequently listed on accessible government vendor lists (construction; professional services; and administrative, support, and waste services), and I label these as "affected - narrow." I label "Unaffected" firms as those that are not listed in the broadly affected category, which includes manufacturing, wholesale trade, accommodation and food services, and establishments in industries not frequently located in urban centers (for example, agricultural services and mining).[22] 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 establishments in accommodation and food services, which may rely on the daytime population driven by other employers. I then construct a category of industries that may reflect changes in local conditions and label those as "foot-traffic" industries in consultation with the Bureau of Labor Statistics' "Industries at a Glance" briefings (retail trade; accommodation & food services; other services; and arts, entertainment, and recreation). 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.

Figure 5 reports estimates of equation 2 of the effect on breakthrough Black elections on establishments per capita by primary industry. The red bars correspond to estimates of the non-Black community effect (listed as "White") τ_1. The gray bars correspond to the relative effect τ_2 in Black communities as compared to white communities in the same city, and the blue bars represent the linear combination of τ_1 and τ_2 which is the total effect in Black communities. In addition, I report the 90- and 95-percent confidence intervals clustered at the city–breakthrough election level. Next to each industry category, I print the pre-breakthrough averages for the industry categories.

Figure 5 provides some evidence for the city-contracting narrative, but it also points to establishment gains across all industry groupings. First, looking at the set of bars for construction establishments per capita, I observe an imprecise 4 percent decline in construction

---

[22]The full category is 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.

establishments per capita in non-Black communities. Black communities experience relative gains of 11.5 percent in the number of construction firms, which constitutes a difference of .22 construction establishments per 1000 residents in Black relative to non-Black communities. The total gains in Black communities are 7.5 percent, but the 95 percent confidence interval indicates a negative effect as large as 2.3 percent.

Below construction are the estimates for establishments in unaffected industries. The count of establishments in unaffected industries in non-Black communities declines by 2.4 percent and is statistically different from zero. The relative gain in Black communities is a precisely estimated 7 percent in which I can rule out effects lower than 4 percent. Much of this gap is explained by the total gain in Black communities, which increases by 4.6 percent. Evaluated at the pre-treatment mean, this indicates an increase in the number of unaffected establishments of 0.64 establishments per 1000 residents.

The estimates for unaffected firms contain industries that are not affected by municipal contracting, but they could be affected by improvement in local conditions, which can take a variety of forms such as improved public services, better relationships with various public agents (for example, the police), increased monitoring of property standards, and so on. Estimates for the change in the count of foot-traffic industry establishments per capita remove industries that are infrequently found in cities and add back in retail trade and other services establishments. The count of foot-traffic establishments per capita declines 2.1 percent in non-Black communities after a breakthrough election, and the relative gain in Black communities is 6.6 percent, nearly equivalent to the unaffected relative gain. The total gain in Black communities is 4.5 percent which is also nearly equivalent to the unaffected gain. However, there are fewer foot-traffic firms per capita, so the estimate indicates an increase of 0.48 foot-traffic establishments per capita.

The final two rows in Figure 5 display the estimates for the set of establishments that are potentially affected by competition for city contracts or increased commitment toward disadvantaged business and supplier diversity programs. The count of narrowly affected establishments in non-Black communities declines by 3.2 percent but I cannot rule out a positive effect as large as 1.3 percent. The relative gain in Black communities for narrowly affected establishments is 6.8 percent and can also rule out gains smaller than 2.7 with 95 percent confidence. The gain indicates a Black–non-Black establishment difference of 0.54 establishments per capita. While the relative gains are statistically significant, the total gain in Black communities is 3.6 percent but the confidence interval implies a potential negative effect as large as 1.3 percent.

The broadly affected category uses the traditional Chatterji, Chay, and Fairlie (2014) definitions for affected establishments. Broadly affected establishments in non-Black commu-

nities decline by 2.4 percent, but the estimate is not statistically different from zero. The relative gain in non-Black communities is 5.7 percent, which evaluates to a difference of 0.94 establishments per capita between Black and non-Black communities. Of that difference, Black communities experience a gain of 0.56 (3.4 percent) establishments per capita after a breakthrough Black election. Overall, 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 gain establishments across the board relative to non-Black ones after the election of a Black candidate.

5.2 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 establishment's number of employees and if there are community-specific gains. Using the ZIP Code Business Patterns detail file, I aggregate the total number of establishments into three bins: establishments with fewer than 10 employees, establishments with at least 10 employees but fewer than 50, and establishments with more than 50 employees. These bins are defined as "small," "medium," and "large." My previous results, particularly Figure 2, cannot strongly speak to this dimension because smaller businesses constitute 69 percent of businesses within a ZIP code in the sample; so the previous results tend to reflect gains in the number of small firms (as I've defined them here) per capita. A particular plank of almost any mayor's stated economic development policies is attracting large firms. Because attracting large firms provides larger employment gains, one might expect gains in Black communities to be more 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. In order to provide support for any or all of these claims, I estimate equation 2 where the dependent variable is the natural log of establishments per capita by size of the establishment. This provides the effect of breakthrough mayors on establishment gains by the employment size within each bin.

Table 4 reports the results broken out by number of employees within an establishment. For small firms, shown in column (1), non-Black communities experience a 3.4 percent decline in small establishments per capita. Black communities experience a relative increase of

6.7 percent, which, when combined with the common effect, produces a 3.2 percent total increase in Black-community small establishments per capita. This is a 0.48 increase in small establishments per capita evaluated at the pre-breakthrough mean.

Column (2) displays estimates for medium-sized establishments per capita, defined as establishments with between 10 and 49 employees. Medium-sized establishments per capita in non-Black communities decline by 2.6 percent after a breakthrough election, which is 0.14 fewer establishments per 1000 residents. The relative effect of Black mayors on medium-sized establishments per capita in Black communities is identical to the small establishment effect at 6.7 percent. The total increase in medium-sized establishments is 4.1 percent, which is an increase of 0.22 establishments per capita. Column (3) displays the estimates for large establishments, defined as establishments that have 50 or more employees. After a breakthrough election, Black communities experience large gains in large establishments per capita. The total effect in Black communities is an 8.1 percent increase in large establishments per capita, which is partially driven by the fact there are fewer large establishments. This indicates an increase of 0.11 large establishments per capita after a breakthrough election, which would be two additional large businesses at the average population of a treated ZIP code from 1994-1999. I cannot say with certainty whether the gains in large firms constitute the entrance of new firms or the transitioning of medium-sized businesses to large businesses; however, the estimates taken together once again indicate a general increase in establishments in Black communities.

5.3 Do Black Mayors Boost Entrepreneurship Among Black Americans? Evidence from the ACS

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, these data do not exist. Even the Survey of Business Owners, which reported the characteristics of business owners, employee counts, and firm revenue, only reported this every five years, was 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.

To determine if gains accrue to Black residents, I use the 1990 and 2000 censuses, and the ACS from 2005 to 2019 (Ruggles et al. 2024),[23] 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 who report

---

[23]The ACS did not report location-specific attributes before 2005, the first year of its full implementation.

being white or Black and are between the ages of 25 and 54 inclusive, which are prime working ages. PUMAs are the lowest level of geography in census and ACS samples and are redrawn every 10 years. PUMAs in 1990 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 no fewer than 100,000 residents. PUMAS from 2000 forward do not cross state lines and are groupings of no fewer than 100,000 residents. I match PUMAs to cities using PUMA crosswalks and composition files that identify the city (place) to which a PUMA corresponds. To determine the effect of breakthrough Black-mayors on race-specific changes in self-employment, I estimate

$$y_{iect} = \alpha_{ec} + \delta_{et}^r + \tau_{e1}\mathbf{1}[BM_{ect}] + \tau_{e2}\mathbf{1}[BM_{ect}] \times Black_{iect} + \Gamma\mathbf{X}_{iect} + \varepsilon_{iect}, \tag{3}$$

where y_{iect} is a measure of self-employment for person i for event e in city c in year t. The estimation of 3 represents two departures from earlier estimations. 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 that 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 potentially unattractive feature. In equation 3, I denote the event-specific treatment effects on each coefficient. For example, τ_{e1} measures the common difference in the 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 τ_{e1}, which represents the common difference in self-employment between treatment and control after a breakthrough Black election, which I interpret as self-employment changes for white residents, and τ_{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, I estimate the gap in two stages. First, I regress the individual self-employment indicator on the vector of controls $\mathbf{X}_{iect}$ and a city fixed effect in each stack. I obtain the residuals from the first regression, and then regress the residuals on an indicator for being black interacted with a treatment indicator, which yields

the amount of the self-employment residual explained by being Black within a treatment city. 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 6.[24] The x-axis displays the city-specific, 2000 Black–white self-employment gap, which I obtain from the procedure outlined previously using the 2000 census. The y-axis displays the point estimates by city and race obtained from estimating equation 3 where self-employment is the dependent variable. The red points are the estimates of τ_{e1}, which is the self-employment effect for white persons. The blue points are the estimates of τ_{e2}, which is the relative self-employment effect of Black relative to white persons. I also include the lines of best fit, which describe the relationship between the 2000 self-employment gap and the effect for white persons and the relative effect for Black persons. I print the averages for the 2000 B–W gap for the treated group, the white estimate, and the B-W estimate in the top-left of Figure 6.

The average B–W gap in self employment in 2000, derived from the procedure described above, is 3.4 percentage points, which is almost identical to the 3.3 percentage point gap in 2015 found by 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 we look at the aggregate self-employment results, the pattern is similar to the previous results for ZIP codes. Black residents experience a relative gain of nearly 1 percentage point and white resident reductions in self-employment are only slightly smaller. The estimates suggest that a large portion of self-employment gains come from fewer white persons identifying as self-employed. Taking the average relative effect $\overline{\tau_{e2}}$ and dividing it by the 2000 gap gives the reduction in B–W self-employment in percent in the treatment cities. The average reduction after a breakthrough election is 27 percent; however, the results in Figure 6 point to heterogeneity in the effect of Black candidates.

In places where the B–W gap in self-employment was already large, those places experienced small and often negative effects on self employment. There is not much of a relationship between white self-employment effects and the existing self-employment gap, but in places where the gap was smaller, an election of a Black candidate led to even larger reductions in the B–W self-employment gap as shown by the positive slope for the blue line. In addition and largely anecdotal given my limited number of treatments, many (but not all) of the areas with greater gaps and negative relative effects are located in the South suggesting the

---

[24]I report the results for those who identify as self-employed with an incorporated business in Appendix Figure A.2. This is a useful check on the initial results, as policies to address gaps in entrepreneurship would affect business owners more likely to incorporate or to have incorporated businesses. In addition, this removes the concern that breakthrough elections are causing disemployment at lower income levels which triggers a transition from an employed wage worker to self employed.

mayor may be of limited assistance in places with longer histories and established patterns of discriminatory behavior.

Work in earlier periods has found employment in municipal government to be one of the main pathways through which Black residents are rewarded, whether this be through changes in descriptive representation or electoral representation caused by civil rights legislation (Aneja and Avenacio-Leon 2022; Nye, Rainer, and Stratmann 2014). Appendix Table A.3 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 in both the private and the public sector. Notably, there is no change for Black Americans employed by the local government. Ignoring the statistical imprecision of the estimate, the average implied effect, that is, the relative change in employment in local government divided by the share of Black residents in local government, would be less than 4 percent, far less than the 55 percent found in earlier elections in Nye, Rainer, and Stratmann (2014). This is mainly because in the time period of my elections, Black Americans are already overrepresented in local government. 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 the set of elections examined by Nye, Rainer, and Stratmann (2014).

6   Conclusion

Thirty-five years after the Voting Rights Act, Black Americans in many large cities with substantial Black populations have never been represented by someone of the same race at the executive level. I show that cities that elect a Black candidate for mayor for the first time experience large and positive changes in economic activity in majority-Black communities. The number of establishments in majority-Black communities grows in the eight years following the election of a breakthrough candidate both absolutely and relative to non-Black communities. Similarly, relative gains are observed in private-sector employment and payroll, areas where the mayor exerts indirect influence. 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 establishments grouped by their potential to be affected by government contracting, I observe gains in establishments per capita that are both frequently affected by government

participation and establishments that benefit from improved local conditions, which suggests a second channel whereby mayoral policy is improving the local business environment in Black communities. This notion is supported when observing that the strongest gains are to the number of establishments with more than 50 employees, which are the types of businesses that create positive spillovers for businesses that require people to be nearby. Because my initial exploration is place-specific, I turn to individual data from the ACS and decennial censuses to examine if Black residents are more likely to identify as self-employed in the wake of a breakthrough election. Before breakthroughs and across all cities, Black residents are less likely to be self-employed relative to white residents. After breakthroughs, the B–W difference in the likelihood to be self-employed shrinks, but how much the difference shrinks is sharply related to the initial difference within each city. The B–W self-employment gap remains unchanged in cities where the Black–white gap in self-employment is widest, suggesting that mayors in these areas may be constrained by local conditions. Compared to previous work that 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 by researchers and journalists on issues of representation and identity (Eligon and Burch 2020; Politico Staff 2021) especially as it pertains to persistent economic and social disparities, further research into longer-term effects to determine if gains are reversed is warranted. Research on other minority groups or examining different areas of government with different administrative authority will help determine the areas where representational parity will effect 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 any candidate given sufficient institutional power or time and credibly committed to policies reducing disparities can play a role whereby the governments they helm eliminate structural barriers to participation and improve access to markets for minority communities and business owners.

7 References

Aghion, Phillipe, Alberto Alesina, and Francesco Trebbi. 2008. "Electoral Rules and Minority Representation in US Cities." *The Quarterly Journal of Economics* 123 (1): 325–57. https://doi.org/10.1162/qjec.2008.123.1.325.

Aneja, Abhay, and Carlos Avenacio-Leon. 2022. "The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Act." Working Paper.

Bailey, Michael Stewart. 1990. "The Role of Black Mayors in the Agenda Politics of American Cities." PhD thesis, The Ohio State University.

Bartik, Timothy. 2020. "Using Place-Based Jobs Policies to Help Distressed Communities." *Journal of Economic Perspectives* 34 (3): 99–127. https://doi.org/10.1257/jep.34.3.99.

Beach, Brian, and Daniel B. Jones. 2017. "Gridlock: Ethnic Diversity in Government and the Provision of Public Goods." *American Economic Journal: Economic Policy* 9 (1): 112–36. https://doi.org/10.1257/pol.20150394.

Beach, Brian, Daniel Jones, Tate Twinam, and Randall Walsh. 2024. "Racial and Ethnic Representation in Local Government." *American Economic Journal: Ecomomic Policy* 16 (2): 1–36. https://doi.org/10.1257/pol.20200430.

Bellemare, Marc, Takaaki Masaki, and Thomas Pepinsky. 2017. "Lagged Explanatory Variables and the Estimation of Causal Effect." *Journal of Politics* 79 (3): 949–63. https://doi.org/10.1086/690946.

BlackFuturesLab. 2019. "More Black Than Blue: Politics and Power in the 2019 Black Census." Demos; Color of Change; Socioanalitica. https://blackfutureslab.org/wp-content/uploads/2019/06/Digital-More-Black-Than-Blue-2.pdf.

Browning, Rufus, Dale Rogers Marshall, and David H. Tabb. 1999. "Taken in or Just Taken? Political Incorporation of African Americans in Cities." In *Minority Politics at the Millenium*, edited by Richard A. Keiser and Katherine Underwood, 131–56. Taylor & Francis Group.

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2020. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics*, December. https://doi.org/10.1016/j.jeconom.2020.12.001.

Cattaneo, Mattias D., Michael Jansson, and Xinwei Ma. 2020. "Simple Local Polynomial Density Estimators." *Journal of the American Statistical Association* 115: 1449–55. https://doi.org/10.1080/01621459.2019.1635480.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs." *The Quarterly Journal of Economics* 134 (3): 1405–54. https://doi.org/10.1093/qje/qjz014.

Chatterji, Aaron, Kenneth Chay, and Robert Fairlie. 2014. "The Impact of City Contracting Set-Asides on Black Self-Employment and Employment." *Journal of Labor Economics* 32 (3): 507–61. https://doi.org/10.1086/675228.

City of richmond v. J.a. Croson co. 1989.

de Benedictis-Kessner, Justin, and Christopher Warshaw. 2016. "Mayoral Partisanship and Municipal Fiscal Policy." *Journal of Politics* 78 (4): 1124–38. https://doi.org/10.1086/686308.

deChaisemartin, Clément, and Xavier D'Haultfœuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review* 110 (September). https://doi.org/10.1257/aer.20181169.

Derenoncourt, Ellora. 2022. "Can You Move to Opportunity? Evidence from the Great Migration." *American Economic Review* 112 (February). https://doi.org/10.1257/aer.20200002.

Deshpande, Manashi, and Yue Li. 2019. "Who Is Screened Out? Application Costs and the Targeting of Disability Programs." *American Economic Journal: Economic Policy* 11. https://doi.org/10.1257/pol.20180076.

Einstein, Katherine Levine, David Glick, and Maxwell Palmer. 2020. *Neighborhood Defenders*. New York, New York: Cambridge University Press. https://doi.org/10.1017/S1537592720001802.

Eligon, John, and Audra D. S. Burch. 2020. "After a Summer of Racial Reckoning, Race Is on the Ballot." *New York Times*, October. https://www.nytimes.com/2020/10/30/us/racial-justice-elections.html.

Enchautegui, María E., Michael Fix, Pamela Loprest, Sarah C. von der Lippe, and Douglas Wissoker. 1997. "Do Minority-Owned Businesses Get a Fair Share of Government Contracts?" The Urban Institute. https://www.urban.org/sites/default/files/publication/67046/307416-Do-Minority-Owned-Businesses-Get-a-Fair-Share-of-Government-Contracts-.PDF.

Fairlie, Robert W., and Bruce D. Meyer. 2000. "Trends in Self-Employment Among White and Black Men During the Twentieth Century." *Journal of Human Resources* 35 (4): 643–69. https://doi.org/10.2307/146366.

Farrell, Diana, Christopher Wheat, and Carlos Grandet. 2019. "Place Matters: Small Business Financial Health in Urban Communities." J.P. Morgan Chase & Co Institute.

Ferreira, Fernando, and Joseph Gyourko. 2009. "Do Political Parties Matter? Evidence from U.S. Cities." *The Quarterly Journal of Economics* 124 (1): 399–422. https://doi.org/10.1162/qjec.2009.124.1.399.

———. 2014. "Does Gender Matter for Political Leadership? The Case of U.S. Mayors."

Journal of Public Economics 112 (April): 24–39. https://doi.org/10.1016/j.jpubeco.2014.01.006.

Frey, William H. 2021. "Neighborhood Segregation Persists for Black, Latino or Hispanic, and Asian Americans." Brookings Institution. https://www.brookings.edu/articles/neighborhood-segregation-persists-for-black-latino-or-hispanic-and-asian-americans/.

Gerber, Elisabeth, and Daniel Hopkins. 2011. "When Mayors Matter: Estimating the Impact of Mayoral Partisanship on City Policy." *American Journal of Political Science* 55 (2): 326–39. https://doi.org/10.1111/j.1540-5907.2010.00499.x.

Goodman-Bacon, Andrew. 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics* 225 (2): 254–77. https://doi.org/10.1016/j.jeconom.2021.03.014.

Goodman-Bacon, Andrew, and Lucie Schmidt. 2020. "Federalizing Benefits: The Introduction of Supplemental Security Income and the Size of the Safety Net." *Journal of Public Economics* 185 (May). https://doi.org/10.1016/j.jpubeco.2020.104174.

Grumbach, Jacob M., Robert Mickey, and Daniel Ziblatt. 2024. "The Insulation of Local Governance from Black Electoral Power: Northern Cities and the Great Migration." https://www.dropbox.com/scl/fi/r54lw8sa6rlekxp611j4b/grumbach_mickey_ziblatt_insulation.pdf?rlkey=kaxb79n7u102j025mwm1m1s18&raw=1.

Hajnal, Zoltan. 2001. "White Residents, Black Incumbents, and a Declining Racial Divide." *American Political Science Review* 95 (3): 603–17. https://doi.org/10.1017/S000305540100303033.

Hipple, Steven F., and Laurel A. Hammond. 2016. "Self-Employment in the United States." U.S. Bureau of Labor Statistics. https://www.bls.gov/spotlight/2016/self-employment-in-the-united-states/.

Hopkins, Daniel J., and Katherine T. McCabe. 2012. "After It's Too Late: Estimating the Policy Impacts of Black Mayoralties in U.S. Cities." *American Politics Research* 40 (4): 665–700. https://doi.org/10.1177/1532673X11432469.

Ikawa, Jorge, Clarice Martins, Pedro C. Sant'Anna, and Rogerio Santarrosa. 2024. "Elections That Inspire: Effects of Black Mayors on Educational Attainment." https://pedrosant-anna.github.io/files/BlackMayors_May9.pdf.

Katz, Eric. 2015. "The 10 Categories Where Federal Agencies Spend the Most on Contracting." *Government Executive*, January. https://www.govexec.com/management/2015/01/10-categories-where-federal-agencies-spend-most-contracting/102498/.

Kruse, Kevin Michael. 2005. *White Flight: Atlanta and the Making of Modern Conservatism.* Politics and Society in Twentieth-Century America. Princeton, N.J: Princeton University Press. https://doi.org/10.1515/9781400848973.

Logan, Trevon D. 2020. "Do Black Politicians Matter? Evidence from Reconstruction." *The Journal of Economic History* 80 (1): 1–37. https://doi.org/10.1017/S0022050719000755.

Manson, Steven, Jonathan Schroeder, David Van Riper, and Steven Ruggles. 2019. "IPUMS National Historical Geographic Information System: Version 14.0 [Database]." Minneapolis, MN. https://doi.org/10.18128/D050.V14.0.

Massey, Douglas, Micahel White, and Voon-Chin Phua. 1996. "The Dimensions of Segregation Revisited." *Sociological Methods and Research* 25 (2): 172–206. https://doi.org/10.1177/0049124196025002002.

McCrary, Justin. 2007. "The Effect of Court-Ordered Hiring Quotas on the Composition and Quality of Police." *American Economic Review* 97 (1): 318–53. https://doi.org/10.1257/aer.97.1.318.

Mello, Steven. 2019. "More COPS, Less Crime." *Journal of Public Economics* 172 (April): 174–200. https://doi.org/10.1016/j.jpubeco.2018.12.003.

Nye, John V. C., Ilia Rainer, and Thomas Stratmann. 2014. "Do Black Mayors Improve Black Relative to White Employment Outcomes? Evidence from Large U.S. Cities." *Journal of Law, Economics, and Organization* 31 (2): 383–430. https://doi.org/10.1093/jleo/ewu008.

Pande, Rohini. 2003. "Can Mandated Political Representation Increase Policy Influence for Disadvantaged Minorities? Theory and Evidence from India." *American Economic Review* 93. https://doi.org/10.1257/000282803769206232.

Perry, Andre M., and Carl Romer. 2020. "To Expand the Economy, Invest in Black Businesses." Brookings Institution. https://www.brookings.edu/articles/to-expand-the-economy-invest-in-black-businesses/.

Peterson, Paul. 1981. *City Limits.* University Of Chicago Press.

Piliawsky, Monte. 1985. "The Impact of Black Mayors on the Black Community: The Case of New Orleans' Ernest Morial." *The Review of Black Political Economy* 13 (4): 5–23. https://doi.org/10.1007/BF02899887.

Politico Staff. 2021. "Help Us Spotlight Who's Shaking up Race, Politics, and Policy." *Politico*, December. https://www.politico.com/news/2021/12/02/the-recast-help-us-find-race-politics-policy-power-players-523516.

Rae, Douglas W. 2003. *City: Urbanism and Its End.* The Yale ISPS Series. New Haven: Yale University Press.

Ray, Rashawn, and Mark Whitlock. 2019. "Setting the Record Straight on Black Voter Turnout." How We Rise. Brookings Institution. https://www.brookings.edu/articles/setting-the-record-straight-on-black-voter-turnout/.

Rose-Ackerman, Susan. 1979. "Market Models of Local Government: Exit, Voting, and the Land Market." *Journal of Urban Economics* 6 (3): 319–37. https://doi.org/10.1016/0094-

1190(79)90034-2.

Ruggles, Steven, Sarah Flood, Matthew Sobek, Daniel Backman, Annie Chen, Grace Cooper, Stephanie Richards, Renae Rodgers, and Megan Schouweiler. 2024. "IPUMS USA: Version 15.0 [Database]." Minneapolis, MN. https://doi.org/10.18128/D010.V15.0.

Sances, Michael W., and Hye Young You. 2017. "Who Pays for Government? Descriptive Representation and Exploitative Revenue Sources." *The Journal of Politics* 79 (May). https://doi.org/10.1086/691354.

Saunders, Pete. 2014. "The Three Generations of Black Mayors in America." Edited by Aaron M. Renn. https://www.urbanophile.com/2014/11/25/the-three-generations-of-black-mayors-in-america-by-pete-saunders/.

Schochet, Peter Z. 2009. "Statistical Power for Regression Discontinuity Designs in Education Evaluations." *Journal of Educational and Behavioral Statistics* 34 (June). https://doi.org/10.3102/1076998609332748.

Shah, Paru. 2014. "It Takes a Black Candidate: A Supply-Side Theory of Minority Representation." *Political Research Quarterly* 67. https://doi.org/10.1177/1065912913498827.

Sugrue, Thomas. 1996. *The Origins of the Urban Crisis: Race and Inequality in Postwar Detroit.* Princeton, New Jersey: Princeton University Press.

Sylvera, Craig. 2023. "Black Mayors and Crime." Working Paper 23-27. Federal Reserve Bank of Cleveland Working Paper Series. Federal Reserve Bank of Clevland. https://doi.org/10.26509/frbc-wp-202327.

Tiebout, Charles. 1956. "A Pure Theory of Local Expenditures." *Journal of Political Economy* 64. https://doi.org/10.1086/257839.

Trounstine, Jessica. 2018. *Segregation by Design: Local Politics and Inequality in American Cities.* New York: Cambridge University Press.

U.S. Census Bureau. 1996-2020. "County Business Pattern: 1994-2018: ZIP Code Industry Detail File." https://www.census.gov/programs-surveys/cbp/data/datasets.html.

———. 1996-2020. "County Business Patterns: 1994-2018: ZIP Code Totals File." https://www.census.gov/programs-surveys/cbp/data/datasets.html.

———. n.d. "County Business Patterns Methodology." https://www.census.gov/programs-surveys/cbp/technical-documentation/methodology.html#:~:text=CBP%20covers%20most%20of%20the,employees%2C%20and%20most%20government%20employees.

Velez, Yamil Ricardo, and Grace Wong. 2017. "Assessing Contextual Measurement Strategies." *The Journal of Politics* 79 (July). https://doi.org/10.1086/691281.

Vogl, Tom S. 2014. "Race and the Politics of Close Elections." *Journal of Public Economics* 109 (January): 101–13. https://doi.org/10.1016/j.jpubeco.2013.11.004.

Washington, Ebonya. 2006. "How Black Candidates Affect Voter Turnout." *The Quarterly Journal of Economics* 121 (August): 973–98. https://www.jstor.org/stable/25098814.

Wing, Coady, Seth M. Freedman, and Alex Hollingsworth. 2024. "Stacked Difference-in-Differences." Working Paper 32054. Working Paper Series. National Bureau of Economic Research. https://doi.org/10.3386/w32054.

Zeng, Jiangnan. n.d. "Racial Representation, Segregation, and Sorting." https://github.com/jeaninezzz/Documents/blob/main/jobmarketpaper.pdf.

● Black Mayor ○ 10% + Interracial □ 10%
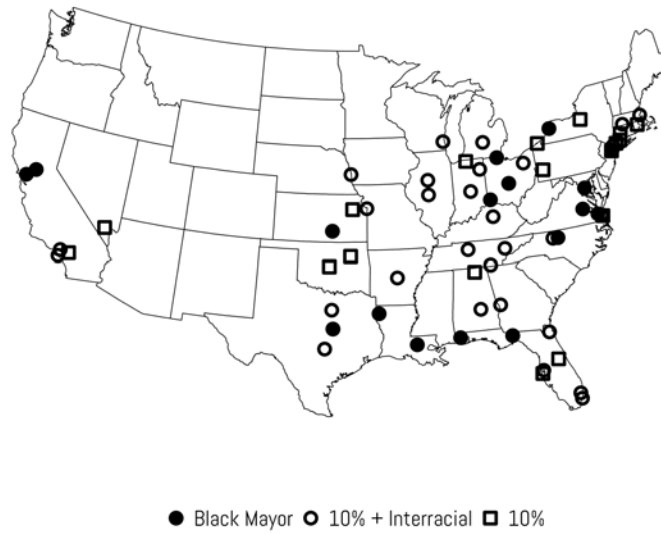
Figure 1: Sample Cities by Breakthrough Election

Sources: Vogl (2014), Ferreira and Gyourko (2009), author
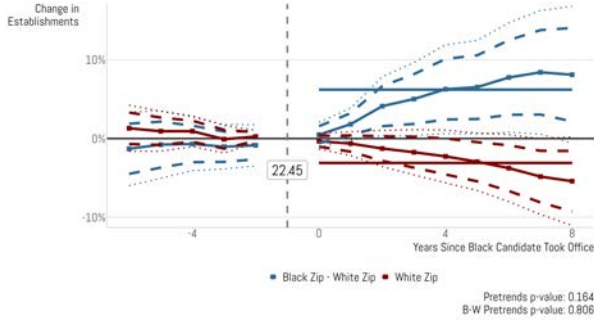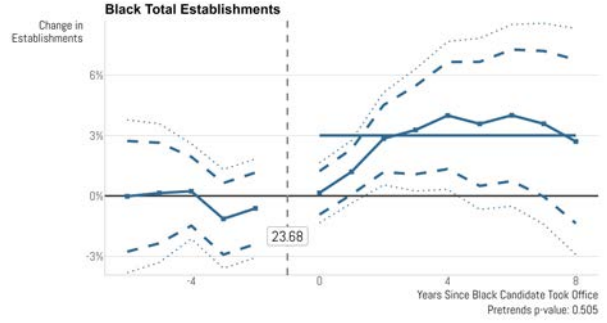
(a) Common and Relative Effect   (b) Total Black Community Effect

Figure 2: Stacked Regression Effect of Black Mayors on Local Establishments

The dependent variable is the natural log of establishments per 1,000 ZIP-code residents. Coefficients are stacked weighted least-squares estimates of equation 1 weighted by 1990 ZIP-code population. The top panel displays estimates of λ, the total effect in white ZIP codes in red, and τ, the relative effect between Black and white ZIP codes, in blue. The bottom panel is the linear combination of λ and τ, which shows the total effect in majority-Black ZIP codes. Dashed lines are 95 percent confidence intervals clustered at the city-by-breakthrough-election level. Dotted lines are similarly defined but adjusted for multiple testing using the sup-t critical value. Sample includes 17 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 eight 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) Common and Relative Effect

(b) Total Black Community Effect

Figure 3: Stacked Regression Effect of Black Mayors on Local Employment

The dependent variable is the natural log of employment per 1,000 ZIP-code residents. Coefficients are stacked weighted least-squares estimates of equation 1 weighted by 1990 ZIP-code population. The top panel displays estimates of λ, the total effect in non-Black ZIP codes in red, and τ, the relative effect between Black and non-Black ZIP codes in blue. The bottom panel is the linear combination of λ and τ, which shows the total effect in majority-Black ZIP codes. Dashed lines are 95-percent confidence intervals clustered at the city-by-breakthrough-election level. Dotted lines are similarly defined but adjusted for multiple testing using the sup-t critical value. Sample includes 17 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 eight 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.
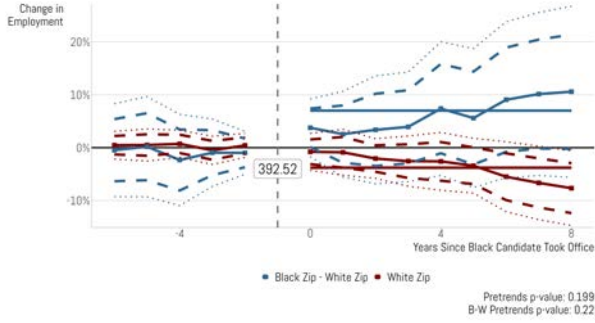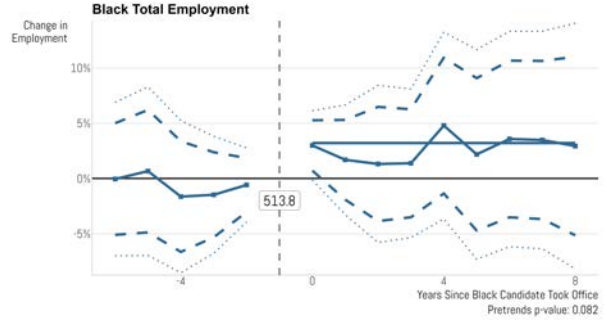
(a) Common and Relative Effect   (b) Total Black Community Effect

Figure 4: Stacked Regression Effect of Black Mayors on Payroll

The dependent variable is the natural log of annual payroll per 1,000 ZIP-code residents. Coefficients are stacked weighted least-squares estimates of equation 1 weighted by 1990 ZIP-code population. The top panel displays estimates of λ, the total effect in white ZIP codes, in red, and τ, the relative effect between Black and white ZIP codes, in blue. The bottom panel is the linear combination of λ and τ, which shows the total effect in majority-Black ZIP codes. Dashed lines are 95 percent confidence intervals clustered at the city-by-breakthrough-election level. Dotted lines are similarly defined but adjusted for multiple testing using the sup-t critical value. Sample includes 17 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 eight 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.
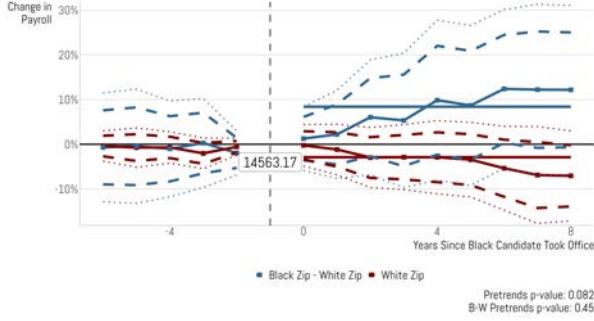
Figure 5: Industry-Specific Estimates of Breakthrough Mayor Effect

The dependent variable is the natural log of the count of establishments per 1,000 ZIP-code residents by industry groupings according to broad NAICS categories. The industry groupings are listed along the y-axis. The coefficients are stacked weighted least-squares estimates of equation 2 of τ_1 and τ_2 weighted by 1990 ZIP-code population. The red bars represent τ_1, the total effect in non-Black ZIP codes, the gray bars represent τ_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 17 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 experience an interracial election or elect a Black candidate more than eight 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 cand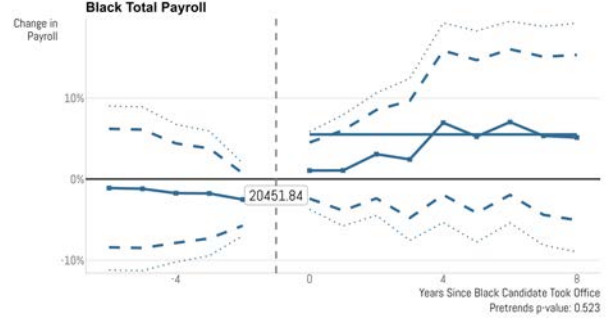idate and the eight years following the election. Standard errors are clustered at the city-by-breakthrough-election breakthrough election level. Dotted line represents 95 percent confidence interval, and solid line represents 90 percent confidence interval.

43
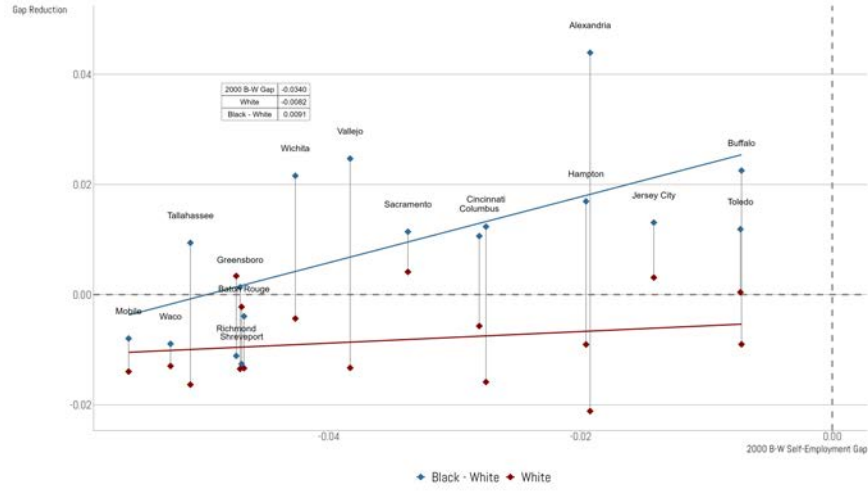
Figure 6: Breakthrough Mayors, Self-Employment Outcomes, & City-Specific Changes in the B-W Gap

The figure displays results from the estimation of equation 3 using census and ACS PUMAs matched to corresponding city 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 τ_{e1}, the breakthrough effect on white self-employment. Blue points show relative self-employment gains for Black residents relative to white residents. The table at the top of the panel shows the average effect across events, *e.g.*, $Gap = \overline{gap} = \frac{1}{17} \sum_{e=1}^{17} gap_e$.

Table 1: Balance of Characteristics Between Treated & Untreated

	Treated Mean	Treated Std. Dev.	Untreated Mean	Untreated Std. Dev.	Diff. in Means	p
N (City)	17.00		32.00			
Population	246898.82	125978.28	283115.78	174239.10	36216.96	0.41
Property Tax (per capita)	0.45	0.52	0.48	0.37	0.03	0.82
Sales Tax (per capita)	0.12	0.14	0.09	0.14	-0.04	0.39
Total Revenue (per capita)	2.27	1.24	2.84	1.55	0.57	0.17
Police Exp. (per capita)	0.22	0.07	0.23	0.10	0.01	0.66
Median Home Value (1990)	82929.41	46970.07	85512.50	59161.55	2583.09	0.87
Median Rent (1990)	355.41	110.29	360.69	85.52	5.28	0.86
Mayor-Council	0.53	0.51	0.72	0.46	0.19	0.21
Black Pop. Share (1990)	0.30	0.12	0.24	0.08	-0.06	0.07
White Pop. Share (1990)	0.62	0.10	0.69	0.09	0.07	0.02
Dissimilarity (1990)	58.62	12.90	63.00	8.70	4.37	0.23
Total Debt (per capita)	2.84	1.54	4.22	5.07	1.39	0.16
N (Zip Code)	201.00		422.00			
Majority Black	0.20	0.40	0.16	0.36	-0.05	0.16
Population	21827.11	12304.75	21582.17	12983.74	-244.94	0.82
Establishments (per 1000)	35.22	58.86	43.50	82.14	8.27	0.15
Employees (per 1000)	747.44	1597.64	911.79	2259.79	164.35	0.30
Annual Payroll (per 1000)	31257.94	79550.00	44443.58	174520.14	13185.65	0.20
Black Share	0.28	0.27	0.22	0.24	-0.07	0.00
White Share	0.66	0.26	0.72	0.25	0.07	0.00

Sources: IPUMS NHGIS, Census Bureau, Vogl (2009), Ferreira and Gyourko (2009),

Table 2: Stacked Event Regression of Breakthrough Mayor's Effect on Economic Activity

	Log Est Per Capita			Log Emp Per Capita			Log Payroll Per Capita		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black Mayor	−0.020	−0.031	−0.022	−0.025	−0.038	−0.035	−0.014	−0.029	−0.030
	(0.010)	(0.012)	(0.011)	(0.012)	(0.016)	(0.015)	(0.019)	(0.024)	(0.025)
Black Mayor × BZ		0.062	0.055		0.070	0.070		0.084	0.084
		(0.023)	(0.017)		(0.050)	(0.037)		(0.066)	(0.049)
Pre-election Trend			−0.003			−0.001			0.0003
			(0.002)			(0.002)			(0.003)
Pre-election Trend × BZ			0.002			0.0002			0.0001
			(0.003)			(0.006)			(0.009)
Pre-Breakthrough Dep. Var. Mean	22.44			380.58			13330.88		
$\tau_1 + \tau_2$		0.03	0.034		0.032	0.035		0.055	0.053
		(0.016)	(0.013)		(0.038)	(0.027)		(0.053)	(0.035)
Observations	104,865	104,865	104,865	104,865	104,865	104,865	104,865	104,865	104,865

Notes: The dependent variable is the natural log of establishments per capita in columns 1-3, the natural log of employees per capita in columns 4-6, and the natural log of annual payroll per capita in columns 7-9. Coefficients are stacked weighted least-squares estimates of 2 weighted by 1990 ZIP-code population. Sample includes 17 cities that directly 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 eight 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 and are clustered at the city-by-breakthrough-election level.

Table 3: Robustness Of Establishment Growth To Alternative Specfications

	Base	Mayor–Council	Intraparty	Re-elected	South × Year FEs	State × Year FEs	Dose
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Establishments							
BM × BZ	0.055	0.069	0.050	0.060	0.049	0.044	0.075
	(0.017)	(0.018)	(0.020)	(0.022)	(0.016)	(0.020)	(0.040)
BM	−0.022	−0.031	−0.034	−0.012	−0.025	−0.040	−0.043
	(0.011)	(0.013)	(0.011)	(0.014)	(0.009)	(0.013)	(0.017)
Panel B: Employment							
BM × BZ	0.070	0.075	0.081	0.080	0.080	0.069	0.076
	(0.037)	(0.046)	(0.052)	(0.049)	(0.037)	(0.042)	(0.074)
BM	−0.035	−0.034	−0.051	−0.028	−0.043	−0.052	−0.050
	(0.015)	(0.022)	(0.018)	(0.022)	(0.017)	(0.016)	(0.024)
Panel C: Payroll							
BM × BZ	0.084	0.076	0.101	0.095	0.100	0.089	0.126
	(0.049)	(0.064)	(0.065)	(0.065)	(0.050)	(0.057)	(0.108)
BM	−0.030	−0.024	−0.056	−0.013	−0.042	−0.054	−0.051
	(0.025)	(0.037)	(0.030)	(0.034)	(0.025)	(0.021)	(0.034)
$\tau_1 + \tau_2{}^{Est}$	0.034	0.038	0.016	0.047	0.025	0.004	0.032
	(0.013)	(0.012)	(0.013)	(0.014)	(0.018)	(0.023)	(0.029)
$\tau_1 + \tau_2{}^{Emp}$	0.035	0.041	0.03	0.052	0.037	0.018	0.026
	(0.027)	(0.029)	(0.036)	(0.032)	(0.027)	(0.042)	(0.055)
$\tau_1 + \tau_2{}^{Pay}$	0.053	0.051	0.046	0.081	0.058	0.035	0.076
	(0.035)	(0.039)	(0.04)	(0.04)	(0.052)	(0.052)	(0.083)
Observations	104,865	56,490	56,940	67,395	104,865	104,865	104,865

Notes: Models are weighted least-squares estimates of equation 2 where the dependent variables are the natural log of establishments per 1,000 ZIP-code residents in panel A, the natural log of employees per 1,000 ZIP-code residents in panel B, and the natural log of annual payroll per ZIP-code 1,000 residents in panel C. Column (1) replicates my base specification from Table 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 eight 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-breakthrough-election level.

Table 4: The Relationship Between Breakthrough Black Mayors and Establishment Gains by Employment Size

	< 10 Employees	$10 \leq$ Employees < 50	≥ 50 Employees
	(1)	(2)	(3)
BM	−0.034	−0.026	−0.032
	(0.012)	(0.010)	(0.014)
BM × BZ	0.067	0.067	0.113
	(0.020)	(0.021)	(0.028)
Pre-Election Trend	0.006	0.003	0.003
	(0.007)	(0.006)	(0.003)
Pre-Election Trend × BZ	−0.008	−0.004	−0.002
	(0.009)	(0.008)	(0.012)
Pre-Breakthrough Dep. Var. Mean	14.89	5.33	1.39
$\tau_1 + \tau_2$	0.032	0.041	0.081
s.e.	0.014	0.018	0.024
Observations	104,865	104,865	104,865

Notes: The dependent variable is the natural log of establishments per 1,000 ZIP-code residents separated into three groups by the number of employees at each establishment: fewer than 10 employees, between 10 and less than 50 employees, and greater than or equal to 50 employees. The coefficients are from stacked, weighted least squares estimation of 2 of τ_1 and τ_2 weighted by 1990 ZIP-code population. "BM" represents τ_1 which is the effect in non-Black ZIP codes. "BM × BZ" represents τ_2 which is the relative effect in Black ZIP codes. $\tau_1 + \tau_2$ represents the total effect Black mayors have on majority Black communities. Sample includes 17 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 eight 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 clustered at the city-by-breakthrough-election level are printed below.

A   Additional Tables & Figures

A.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 [A.11](#) 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 percent which contains even landslide victories.

A.2   Power Calculations

Following Mello ([2019](#)) and Schochet ([2009](#)), the minimum detectable effect (MDE) for significance level α and power κ under the assumption that the change in the outcome of interest $\Delta(y)$ is a linear function absent the discontinuity is

$$MDE = (t_{\alpha/2} + t_{1-\kappa}) \times \sqrt{\frac{1}{D(1-D)} \frac{\sigma^2_{\Delta y}}{N} \frac{1}{(1-\rho^2)}} \tag{A.1}$$

where D is the fraction of cities assigned to treatment and ρ is the correlation between the score and treatment status. Outcome variability is $\sigma^2_{\Delta(y)}$, 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 [A.5](#) for the aggregate effect of each outcome and compare those results to the difference-in-differences estimates from Table [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.

A.3   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, I estimate

$$y_{zct} = \alpha_z + \delta_t^r + \tau_1 BM_{ct} + \tau_2 BM_{ct} \times BZ_{zc} + \varepsilon_{zct} \tag{A.2}$$

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

$$y_{zct} = \alpha_z + \delta_t^r + \tau_1 BM_{ct-1} + \tau_2 BM_{ct-1} \times BZ_{zc} + y_{zct-1} + \varepsilon_{zct} \tag{A.3}$$

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 A.7.

A.4 Do Trends Change Post-Election? Trend-Break Specification

If trends in cities that elect a Black candidate for the 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), who interact a time trend centered on each breakthrough election with a Black mayor trend break. I estimate

$$\begin{aligned} y_{ezct} = & \alpha_{ez} + \delta_{et}^r + T_1 BM_{ect} \times (t - B_{ec}) + T_2 BM_{ect} \times (t - B_{ec}) \times BZ_{ezc} \\ & + \Gamma_1(t - B_{ec}) + \Gamma_2(t - B_{ec}) \times BZ_{ezc} + \varepsilon_{ezct}, \end{aligned} \tag{A.4}$$

where Γ_1 tests for differential linear pre-trends in each breakthrough city and Γ_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 A.8, 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 Finance Agency (FHFA) in Table A.9.

B 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.[25] 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 on the outcome because they serve as the chief executive.

B.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 A.10, 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 since I cannot rule out small-to-medium positive or negative effects. Because "all crimes" likely captures changes to crimes committed most frequently, such as 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

---

[25]For more on the debate, see Gerber and Hopkins (2011), Ferreira and Gyourko (2009), Ferreira and Gyourko (2014), de Benedictis-Kessner and Warshaw (2016).

are likely to be reported to police,"[26] and columns 3 and 4 separate index crimes into violent and property crimes categories.[27] 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 that 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 a 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 a weaker effect on crime over the period analyzed.

B.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 on the number of buildings, both residential and commercial, that get approved for construction and the size of these buildings at after a series of abutter hearings and impact studies. The mayor, then, influences the built environment of the municipality through these appointments.[28] 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 — 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

---

[26]https://ucr.fbi.gov/crime-in-the-u.s/2011/crime-in-the-u.s.-2011/offense-definitions

[27]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.

[28]A larger discussion of the planning and construction process can be found in Einstein, Glick, and Palmer (2020).

mayor–council events that would have the authority described in the previous paragraph. Column 1 of Table A.11 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 is 21.5 per 10,000 residents in a year. At the low end of the interval would be 18.6 buildings approved per 10,000 and 35.48 per 10,000 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 that 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 that 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, a topic I explore in the next section.

B.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 who 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) (pp. 160-165) specifically describes race-specific disparities in sewer maintenance systems. She finds that 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 4, 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.

B.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 take the form of 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 five 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.

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

Because we observe establishment gains in Black communities and because Black business owners are disproportionately more likely to serve their neighborhood (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 A.15, using data from the Census Bureau's Survey of Business Owners, provides a case study suggesting that the election of a Black mayor increases Black business formation and/or tenure. 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.[29] Between 1997 and 2007, both cities had similar growth in Black-owned firms, but after the Glover election, Shreveport had gained over 3000 more

---

[29]Frank Scott became the first directly elected mayor of Little Rock in 2019.

Black-owned firms by 2012. Growth in Black-owned firms slightly decreased over the same five-year period in Little Rock.[30]

Glover was the youngest person elected to the Shreveport city council and spent 10 years in the Louisiana House of Representatives before achieving the mayoralty. His mayoral competitor, Jerry Jones,[31] 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.[32] Turnout for the runoff was 10 points higher than expectation, attributable to a 15 percent surge by Black voters, reflecting Vogl's findings.[33] 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.[34] 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.[35] 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.

---

[30]Black employer firms observe no growth in the 15 years before the election of Glover. In fact, Shreveport lost three Black employer firms from 2002 to 2007, reducing its total to 183 firms. In the five 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.

[31]Not that one, football fans.

[32]"Republican, Democrat head to runoff in Shreveport mayoral race." Associated Press State & Local Wire, October 1, 2006.

[33]Shreveport Times, Michelle Mahfoufi, November 8, 2006

[34]https://www.shreveportla.gov/DocumentCenter/View/2542/Mayor-Cedric-B-Glover---BIO?bidId=

[35]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.
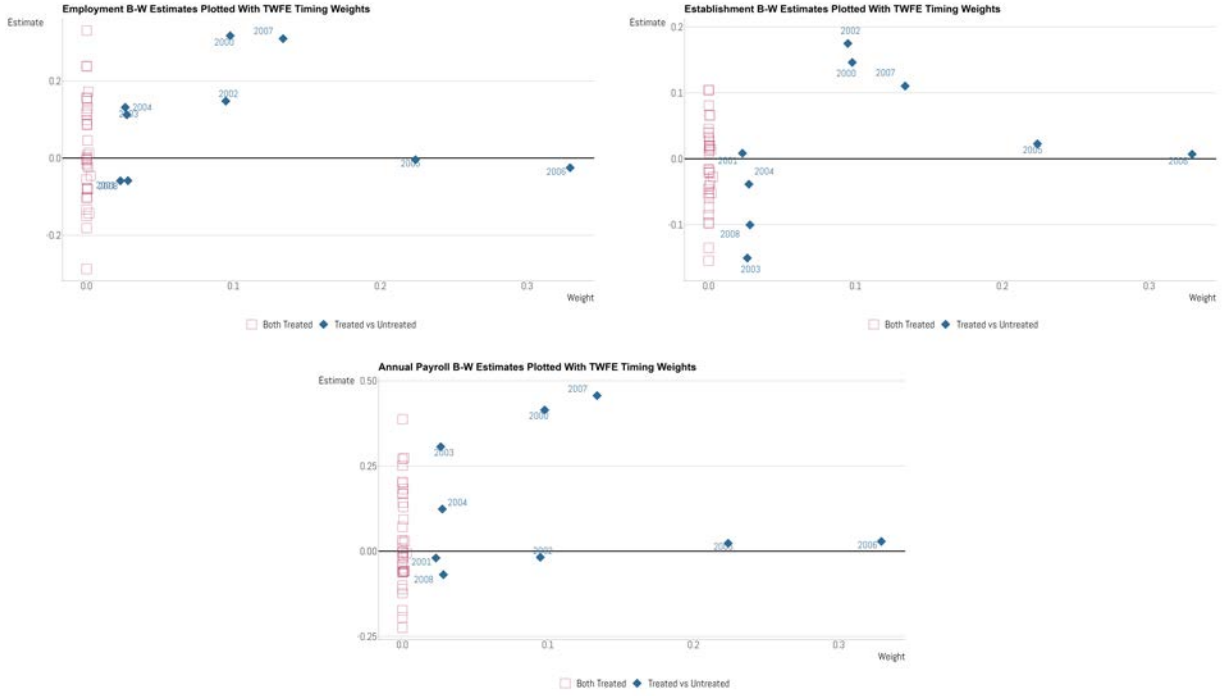
Figure A.1: Difference-in-Differences Decomposition of Estimates and Timing Weights

The figure plots out difference-in-differences decomposition of two-way fixed effects estimation. 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 elected a breakthrough candidate after 2010 are excluded.
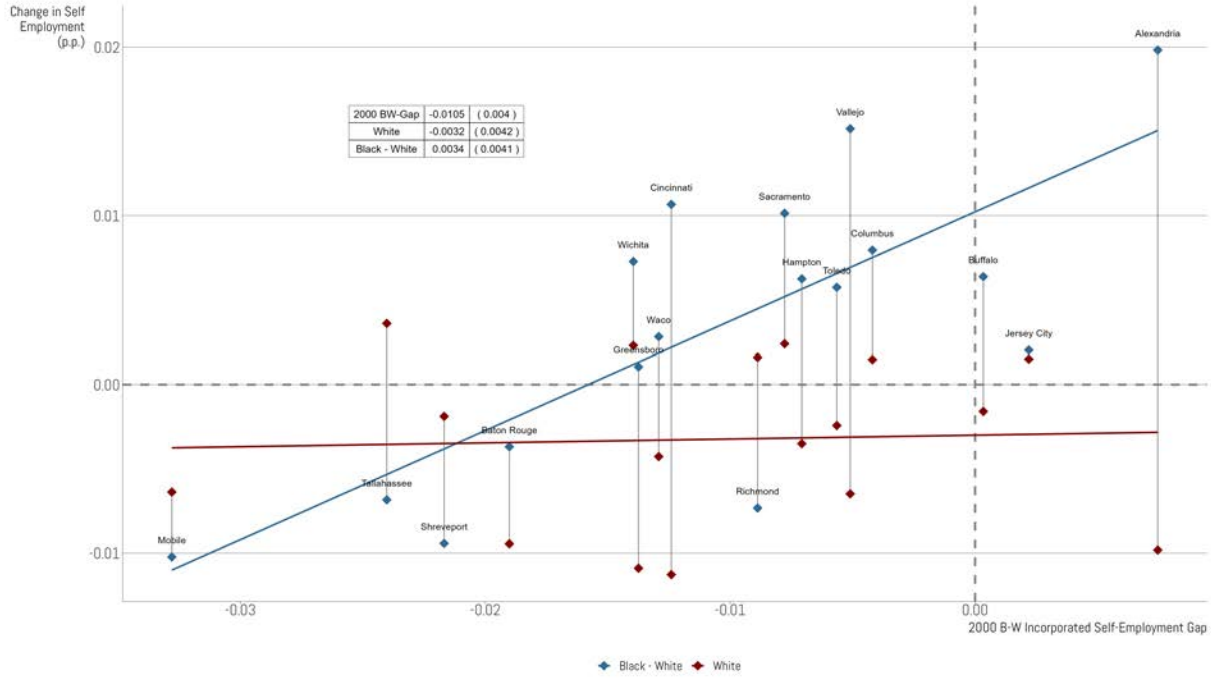
Figure A.2: Breakthrough Mayors, Incorporated-Self-Employment Outcomes, & City-Specific Changes in the B-W Gap

The figure displays results from the estimation of equation 3 using census and ACS PUMA data matched to each city 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 τ_{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, $e.g.$, $Gap = \overline{gap} = \frac{1}{17} \sum_{e=1}^{17} gap_e$.

		All	Sample
1990 City Characteristics			
	Population	337793.00	251557.00
		646548.00	146573.00
	Property Taxes	424.79	457.76
		429.80	409.07
	Sales Taxes	133.79	93.21
		161.70	131.10
	Total Revenue	2317.90	2446.61
		1507.57	1456.02
	Debt Outstanding	2828.29	3254.66
		2689.43	3961.40
	Police Spending	219.54	226.01
		86.52	90.91
	Black Pop Share	23.46	23.87
		17.99	11.89
	White Pop Share	66.97	68.70
		16.69	11.20
	Median Housing Value	93574.44	85821.74
		64511.65	56020.93
	Median Rent	376.92	359.89
		114.51	94.69
	Mayor-Council	66.67	71.11
		47.32	45.84
Zip Code Characteristics			
	Population	21209.00	21145.00
		13144.00	13060.00
	Annual Payroll	29222.08	32915.38
		107142.19	129850.41
	Wage	26.08	26.95
		7.89	9.59
	Employment	876.11	903.07
		2229.84	2279.61
	Establishments	41.91	41.95
		80.22	79.26
	HPI	84.84	84.84
		9.84	9.84
	Black Share	22.51	22.33
		24.04	23.97
	White Share	71.16	71.36
		24.33	24.33

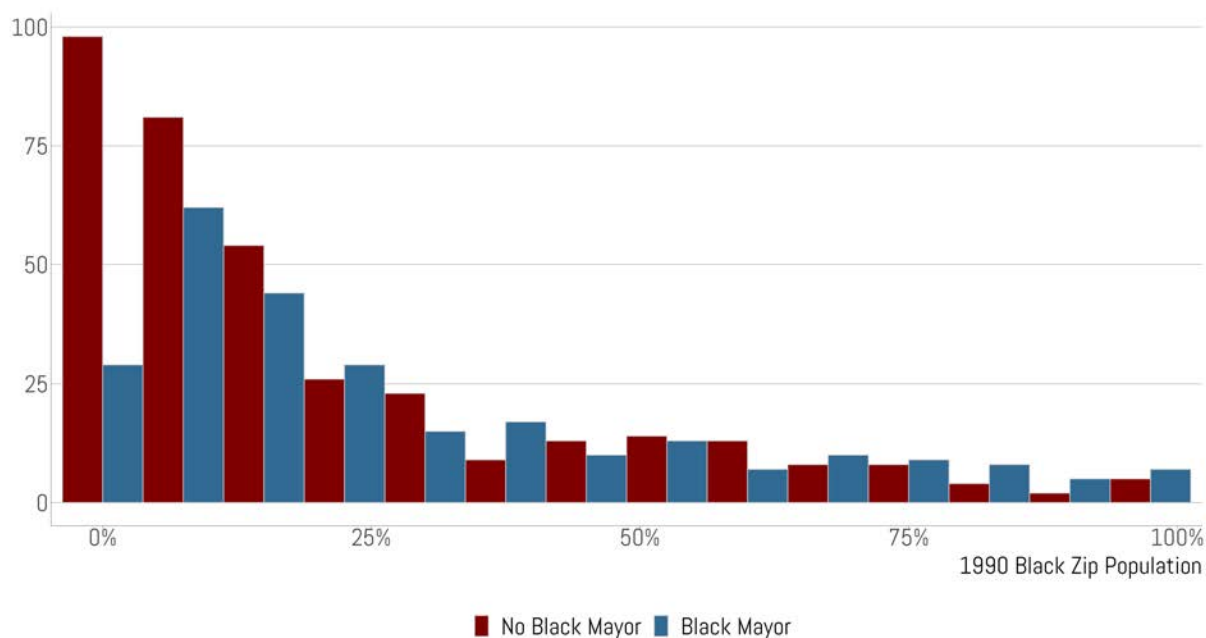Figure A.3: Summary Stats of Sample Cities Versus All Cities

Figure A.4: Distribution of 1990 Black ZIP Code Population by Treatment Status

The figure displays the 1990 distribution of ZIP codes according to their Black population share. Blue bars show the distribution of treated cities that elected a black candidate from 2000 to 2010. Red bars show the distribution of control cities that observed an interracial election over the sample period or elected a Black candidate after 2010.
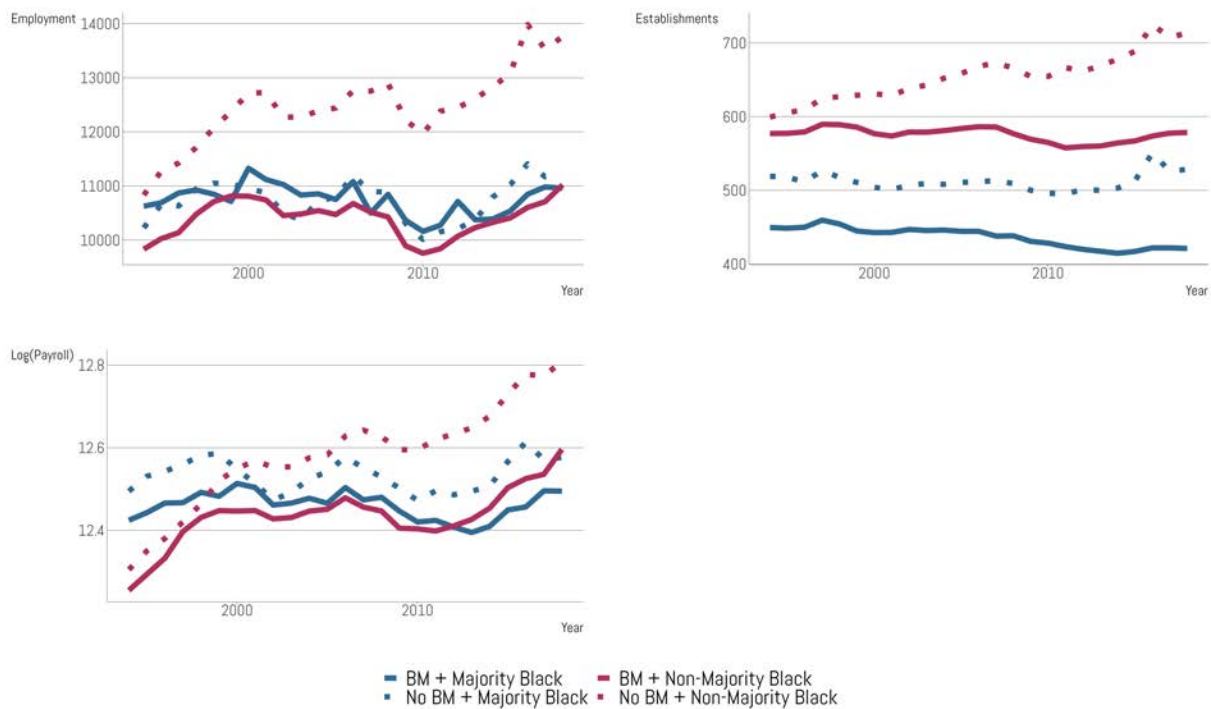
Figure A.5: Average Levels of Economic Outcomes by Treatment Status and Racial Composition

The 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 between 2000 and 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.
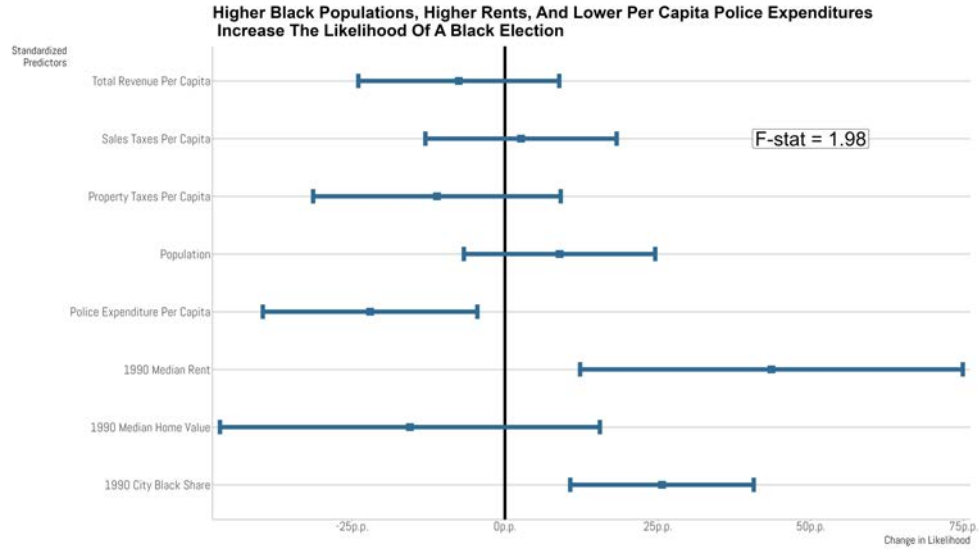
(a) Likelihood
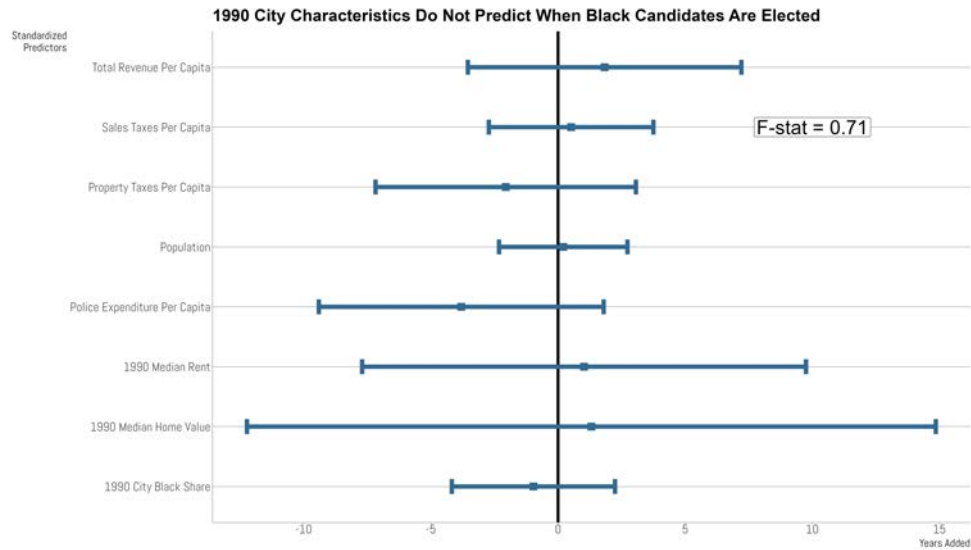
(b) Timing

Figure A.6: 1990 Characteristics Have Some Predictive Power on if but Not When a Black Candidate is Elected

Panel A displays results from the regression of $Black\ Mayor_c = \Gamma X_c + \epsilon$ where X_c is a vector of city-level characteristics and a constant. The vector of characteristics is standardized; therefore, the interpretation for each characteristic is a one standard deviation change 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 $Breakthrough\ Year_c = \Lambda X_c + \nu$ where X_c is a vector of city-level characteristics and a constant. The vector of characteristics is 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.
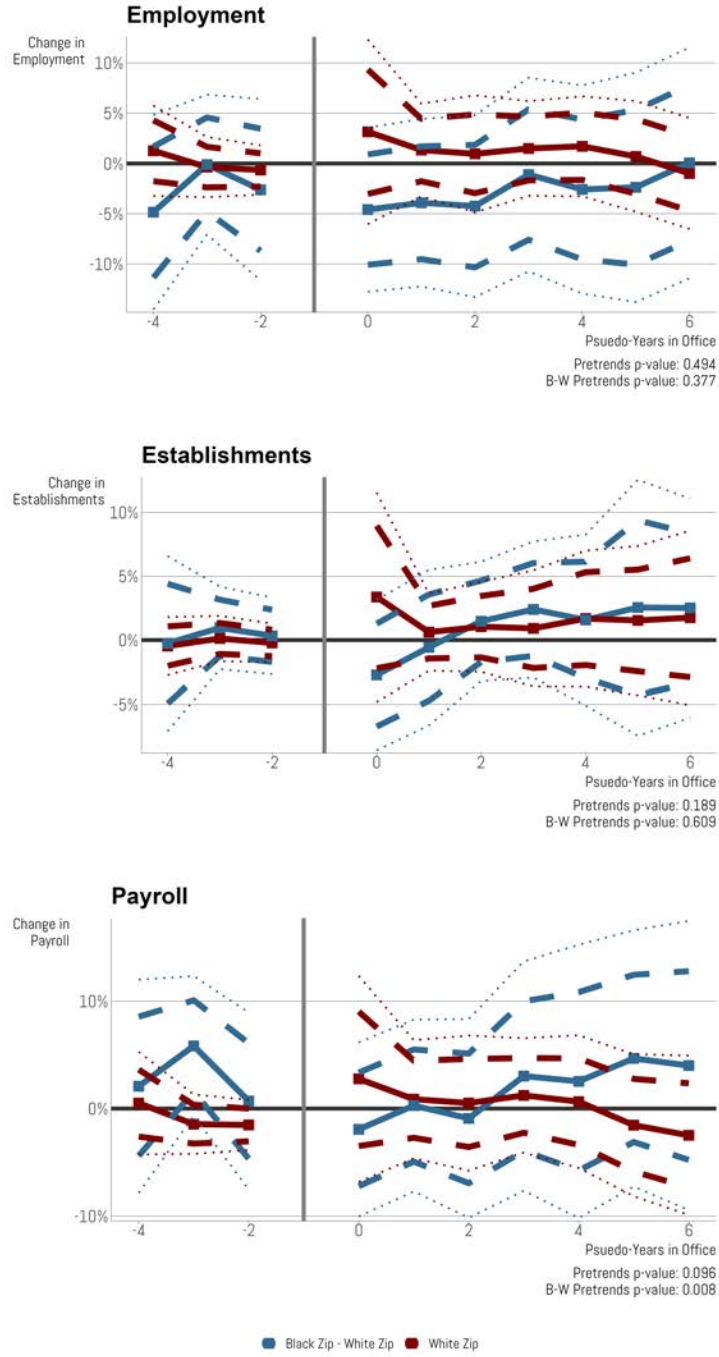
Figure A.7: Event-Study Estimates of Falsification Exercise

Results display the event-study analog of Table A.2. The sample is the set of cities in 1990 that 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 between 1998 and 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.

(a) Employment

(b) Establishments

(c) Payroll

Figure A.8: 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 ar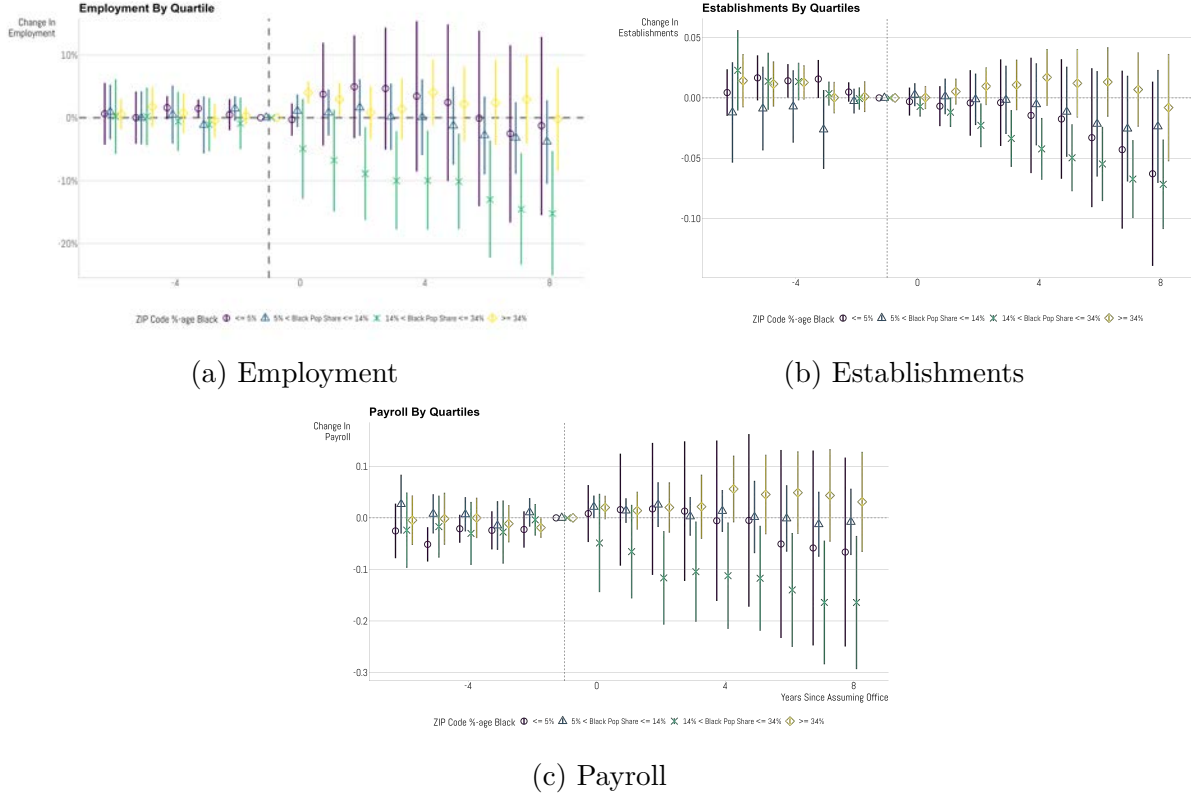e the natural log of employment, establishments, and payroll per ZIP-code resident. The first quartile includes those whose 1990 Black populations are less than or equal to 5 percent; the second quartile includes ZIP codes greater than 5 percent and less than or equal to 14 percent; third quartile includes ZIP codes greater than 14 percent and less than or equal to 36 percent; the fourth quartile includes the ZIP codes greater than 36 percent. Sample includes 17 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 eight 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. 95 percent confidence intervals clustered at the city-by-breakthrough-election level are shown.

Pre- And Post-Changes In Black Population Share

Pre- And Post-Changes In Population
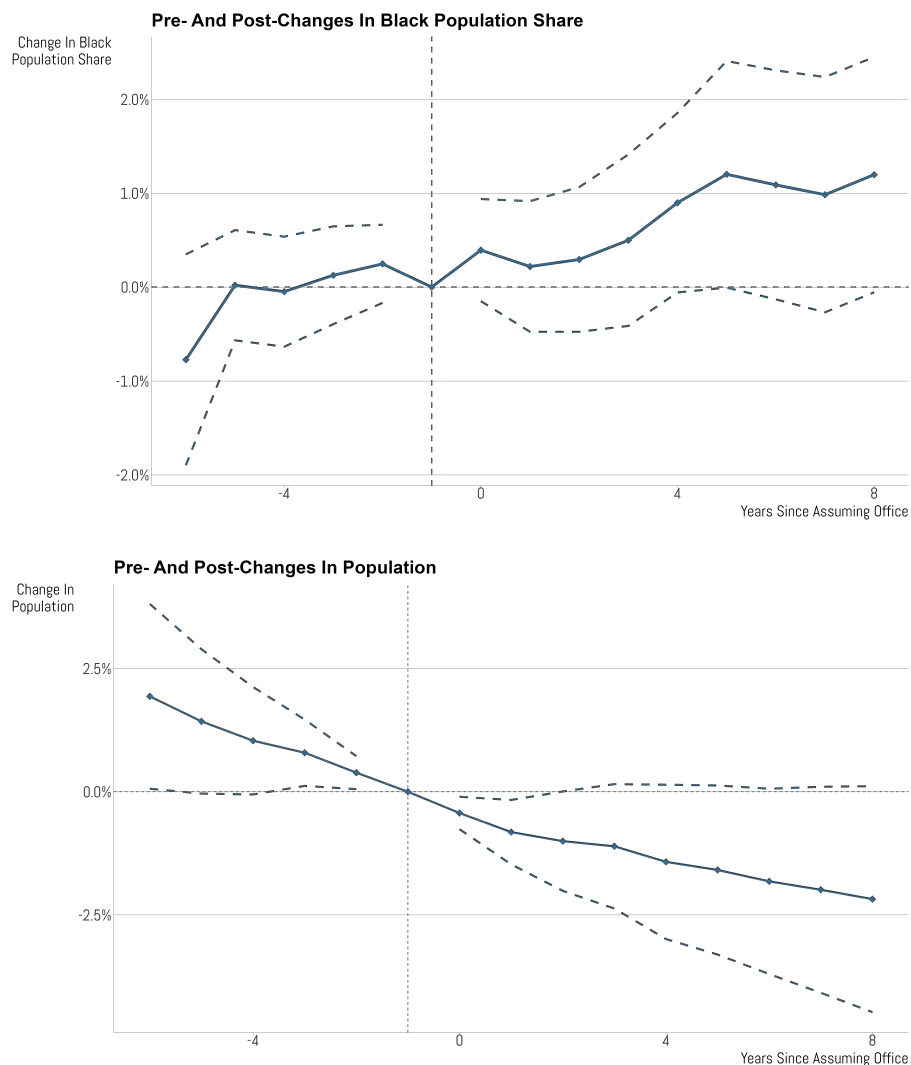
Figure A.9: Population is Declining in the Pre-Period & Continues for Four Years but the Black Population Share is Not Increasing Until After the Breakthrough Election

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 17 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. 95 percent confidence intervals clustered at the city-by-breakthrough-election level are shown.

Black Population Changes By Quartiles

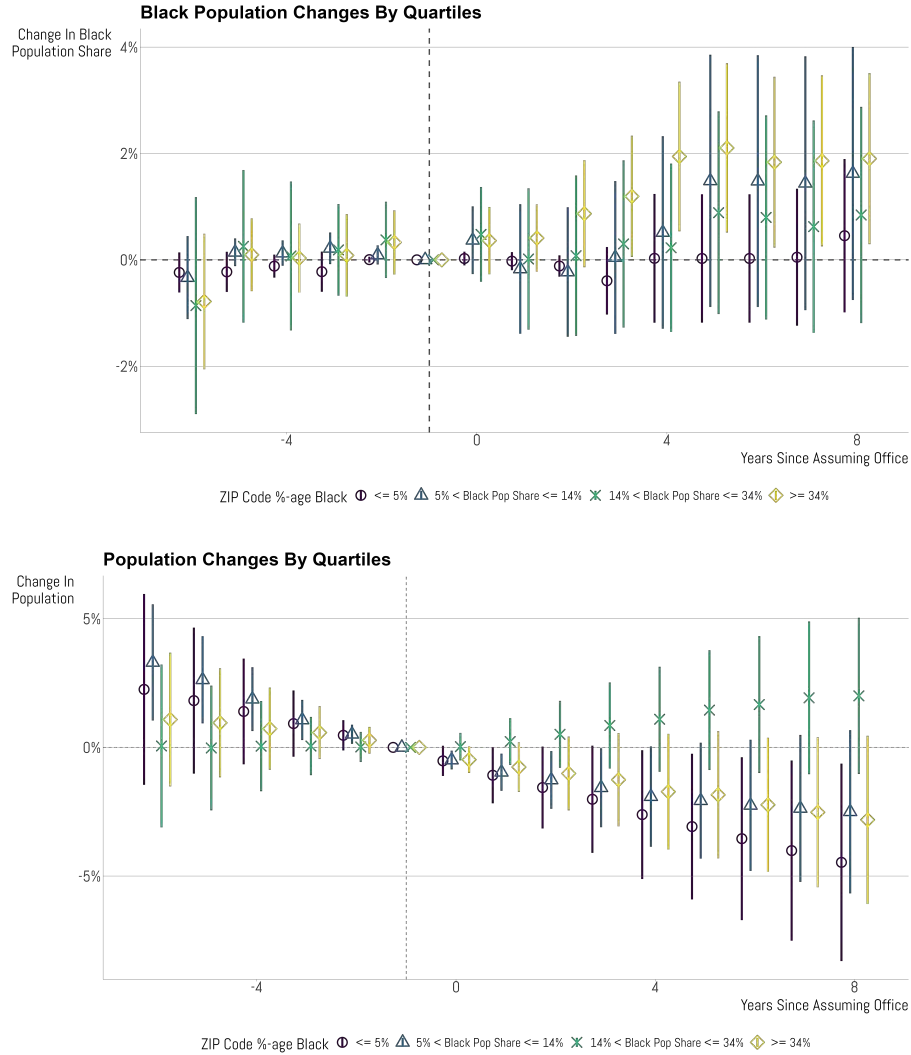**Population Changes By Quartiles**

Figure A.10: 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 includes those whose 1990 Black populations are less than or equal to 5 percent; the second quartile includes ZIP codes greater than 5 percent and less than or equal to 14 percent; the third quartile includes ZIP codes greater than 14 percent and less than or equal to 36 percent; the fourth quartile includes 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 17 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 eight 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. 95 percent confidence intervals clustered at the city-by-breakthrough-election level are shown.
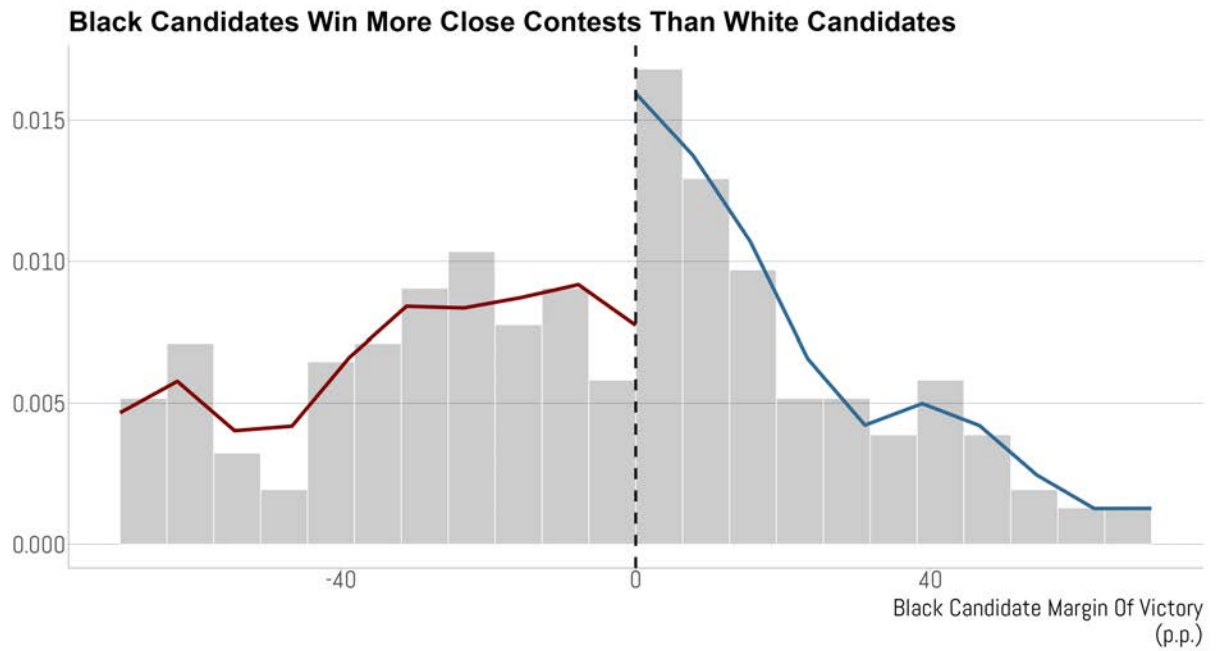
Figure A.11: Histogram of Margin of Victory of Black Candidates

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

Figure A.12: 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 for each event. The bottom left figure captures τ_1, the total effect in non-Black ZIP codes, and the bottom right panel captures τ_2, the relative effect between Black and non-Black ZIP codes. I report 95 percent heteroskedastic robust confidence intervals.
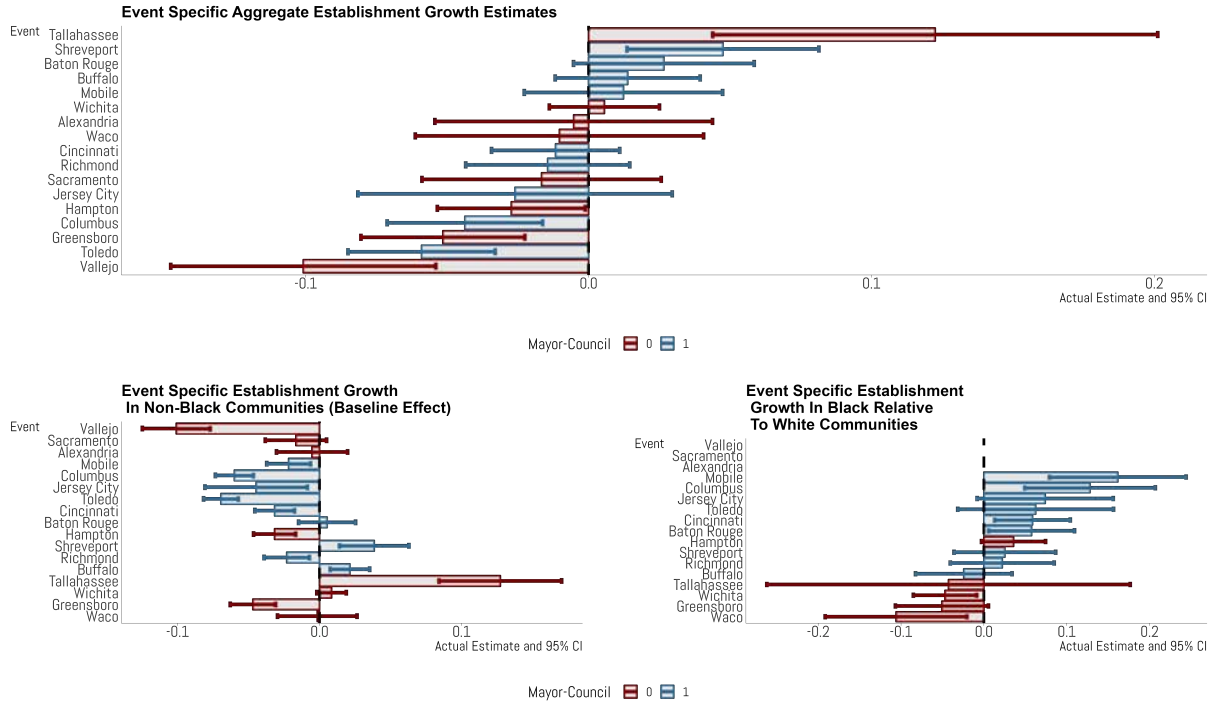
Figure A.13: Event-Specific Estimates of Establishments

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 for each event. The bottom left figure captures τ_1, the total effect in non-Black ZIP codes, and the bottom right panel captures τ_2, the relative effect between Black and non-Black ZIP codes. I report 95 percent heteroskedastic robust confidence intervals.
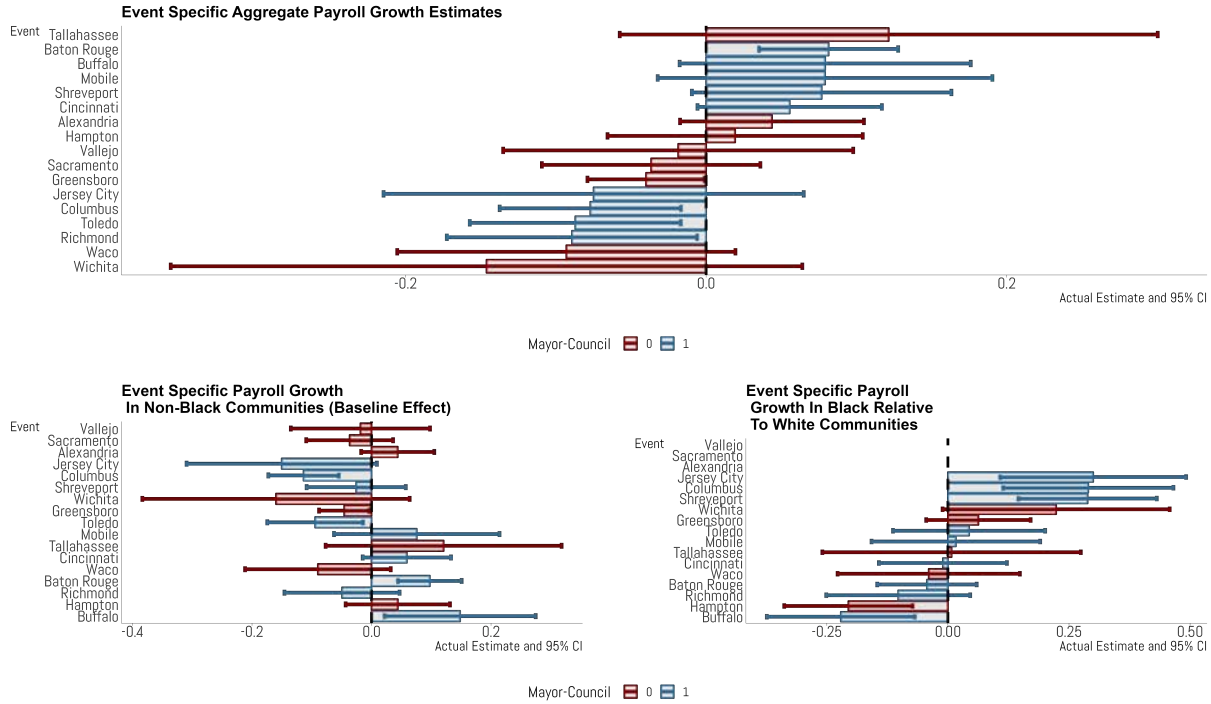
Figure A.14: Event-Specific Estimates of Payroll

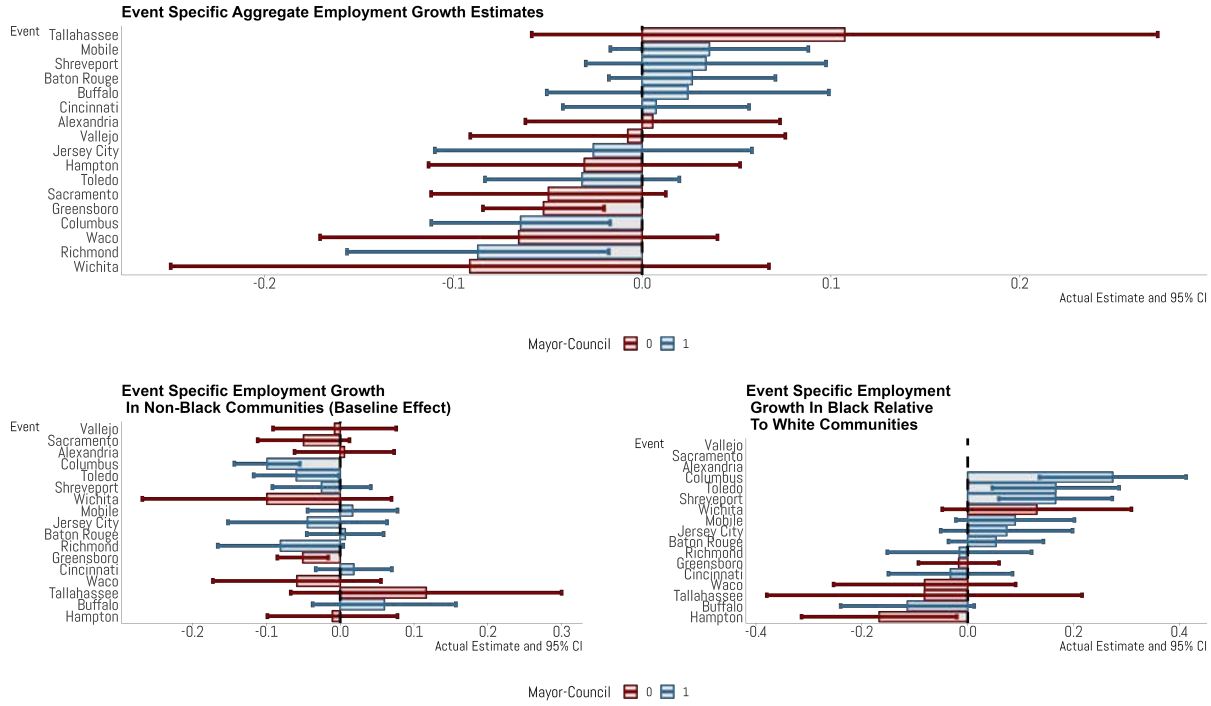Results display event-specific estimates for the natural log of payroll per ZIP-code resident. For each event, sample is constructed similarly to my main specification. The top panel captures the aggregate effect for each treatment city. The bottom panel estimates an event-specific version of 2 for each event. The bottom left figure captures τ_1, the total effect in non-Black ZIP codes, and the bottom right panel captures τ_2, the relative effect between Black and non-Black ZIP codes. I report 95 percent heteroskedastic robust confidence intervals.
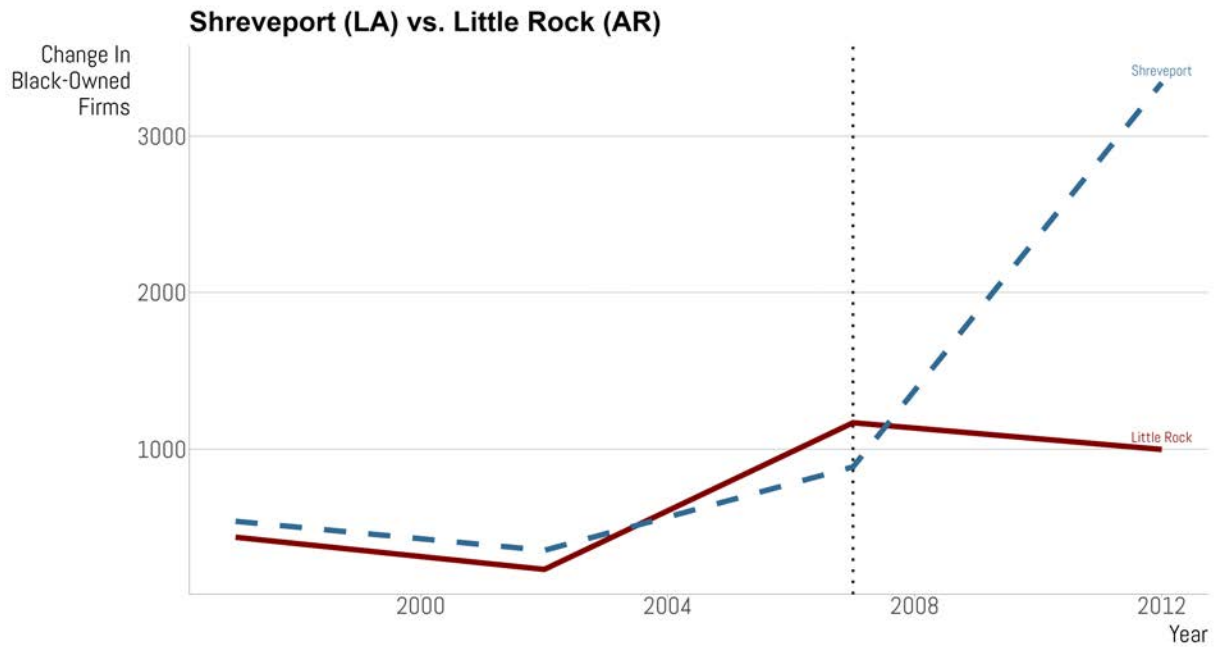
Figure A.15: Changes in Black-Owned Firms - Shreveport vs. Little Rock

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

Table A.1: Breakthrough Mayor and Runner-Up Age and Experience

CITY	MAYOR COUNCIL	WINNER	RUNNERUP	AGE-WIN	AGE-RUN	EXP-WIN	EXP-RUN	COUNCIL EXP-WIN	COUNCIL EXP-RUN
MOBILE	1	SAM JONES	JOHN PEAVY	58		18	14	0	14
SACRAMENTO	0	KEVIN JOHNSON	HEATHER FARGO	42	55	0	19	0	11
WICHITA	0	CARL BREWER	CARLOS MAYANS	50	53	6	14	6	4
BATON ROUGE	1	MELVIN HOLDEN	BOBBY SIMPSON	52	51	20	12	4	12
SHREVEPORT	1	CEDRIC GLOVER	JERRY JONES	50		16	0	6	0
JERSEY CITY	1	GLENN CUNNINGHAM	TOM DEGISE	57	51	14	8	8	8
BUFFALO	1	BYRON BROWN	KEVIN HELFER	47		10	11	5	6
GREENSBORO	0	YVONNE JOHNSON	MILTON KERN	65		14	0	14	0
CINCINNATI	1	MARK MALLORY	DAVID PEPPER	43	34	10	4	0	4
COLUMBUS	1	MICHAEL COLEMAN	DOROTHY TEATER	45		8	17	8	2
TOLEDO	1	JACK FORD	RAY KEST	54	52	27		20	0
WACO	0	MAE JACKSON	4 CANDIDATES	63		4		4	
ALEXANDRIA	0	WILLIAM EUILLE	WILLIAM CLEVELAND	53	55	9	15	9	15
HAMPTON	0	MAMIE LOCKE	PAT MINETTI	46	67	4	0	4	0
RICHMOND	1	DOUGLAS WILDER	RUDY MCOLLUM	73	49	24	9	0	9
VALLEJO	0	OSBY DAVIS	GARY CLOUTIER	62	45	14	8	0	8
TALLAHASSEE	0	JOHN MARKS	JOHN PAUL BAILEY	55	56	0	8	0	8
				54.2	51.6	12.4	8.94	4.89	6.18
				8.35	7.65	8.31	6.1	5.53	4.99

Table A.2: Do Black Communities Improve from Interracial Elections Alone? Falsification Exercise

	Log Emp Per Capita	Log Est Per Capita	Log Payroll Per Capita
Black Mayor	0.021	0.014	0.013
	(0.015)	(0.015)	(0.018)
Black Mayor x BZ	-0.038	0.008	0.024
	(0.03)	(0.014)	(0.031)
Pre-Election Trend	-0.004	0.001	-0.001
	(0.005)	(0.002)	(0.005)
Pre-Election Trend x BZ	0.012	-1.559e-05	-0.012
	(0.009)	(0.008)	(0.01)
Total Effect	-0.016	0.022	0.037
	(0.024)	(0.015)	(0.024)
N	47324	47324	47324

Notes:

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 1998 and 2012 and 22 cities whose 1990 populations were greater than 100,000 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 taken 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 A.3: Breakthrough Mayor Effect on Employment Outcomes and Wages - Manual Averaging Using the ACS and Decennial Census

	Employment Rate	Labor Force Participation	Employed - Local Government	Employed - State Government	Log Wage
BM × Black	0.001	0.003	0.003	−0.013	−0.002
	(0.013)	(0.010)	(0.009)	(0.010)	(0.035)
BM	−0.009	−0.010	−0.003	0.008	0.026
	(0.010)	(0.008)	(0.007)	(0.009)	(0.029)

Notes: Table conatins manually averaged estimates of equation 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 who identify as only Black or white alone and are between the ages of 25 and 54 inclusive who 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 A.4: Effect of Breakthrough Black Mayor on ZIP Code Income Using IRS Statistics on Income

	Share Filing	Log(AGI)	Log(Salary)	Log(Business Income)	Log(AGI Per Filer)	Log(Salary Per Filer)	Log(Business Income Per Filer)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black Mayor	−0.011	−0.028	−0.035	−0.051	−0.002	−0.008	−0.024
	(0.004)	(0.031)	(0.030)	(0.036)	(0.016)	(0.016)	(0.037)
$BM \times BZ$	0.001	−0.051	−0.031	0.151	−0.014	0.006	0.170
	(0.009)	(0.032)	(0.032)	(0.067)	(0.017)	(0.020)	(0.064)
$\tau_1 + \tau_2$	−0.01	−0.079	−0.066	0.099	−0.016	−0.003	0.146
	(0.011)	(0.035)	(0.035)	(0.067)	(0.016)	(0.019)	(0.061)
Observations	81,794	81,794	81,794	65,521	81,794	81,794	65,521

Notes: Table A.4 displays estimation of stacked weighted least squares of equation efeq: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.

Table A.5: Regression Discontinuity Power Calculations

Outcome	DD estimate (Table 2, columns 1, 4, 7)	0.5	0.75	1	1.5	2
Employment	2.2%	6.6%	6.4%	5.5%	5.2%	5.0%
Establishments	2.0%	4.3%	4.0%	3.5%	3.3%	3.1%
Payroll	−0.009%	7.7%	7.4%	6.4%	6.1%	5.8%

See Appendix A.2 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.

Table A.6: Breakthrough Election Effect Using Conventional TWFE and Lagged Independent Variable

	Stacked	Conventional	Lagged	Lagged Dynamic
	(1)	(2)	(3)	(4)
Panel A: Employment				
BM	−0.025	−0.031		
	(0.012)	(0.018)		
BM$_{-1}$			−0.005	−0.003
			(0.014)	(0.004)
Log(Emp Per Capita)$_{-1}$				0.844
				(0.021)
Panel B: Establishments				
BM	−0.020	−0.030		
	(0.010)	(0.017)		
BM$_{-1}$			−0.003	−0.002
			(0.011)	(0.003)
Log(Est Per Capita)$_{-1}$				0.858
				(0.038)
Panel C: Payroll				
BM	−0.014	−0.018		
	(0.019)	(0.026)		
BM$_{-1}$			0.012	−0.001
			(0.019)	(0.005)
Log(AP Per Capita)$_{-1}$				0.844
				(0.015)
Observations	104,865	15,575	15,575	14,760

Table A.7: Breakthrough Election Effect By ZIP-Code Racial Composition Using Conventional TWFE and Lagged Independent Variable

	Stacked (1)	Conventional (2)	Lagged (3)	Lagged Dynamic (4)
Panel A: Employment				
BM	−0.038	−0.037		
	(0.016)	(0.022)		
BM × BZ	0.070	0.033		
	(0.050)	(0.058)		
BM_{-1}			−0.010	−0.004
			(0.021)	(0.005)
Log Emp Per Capita$_{-1}$				0.844
				(0.021)
$BM_{-1}×$ BZ			0.031	0.003
			(0.048)	(0.008)
Panel B: Establishments				
BM	−0.031	−0.037		
	(0.012)	(0.020)		
BM × BZ	0.062	0.039		
	(0.023)	(0.037)		
BM_{-1}			−0.009	−0.004
			(0.014)	(0.003)
Log Est Per Capita$_{-1}$				0.858
				(0.038)
$BM_{-1}×$ BZ			0.030	0.009
			(0.026)	(0.004)
Panel C: Payroll				
BM	−0.029	−0.026		
	(0.024)	(0.031)		
BM × BZ	0.084	0.040		
	(0.066)	(0.079)		
BM_{-1}			0.005	−0.004
			(0.026)	(0.006)
Log AP Per Capita$_{-1}$				0.844
				(0.015)
$BM_{-1}×$ BZ			0.042	0.014
			(0.065)	(0.012)
$\tau_1 + \tau_2^{Emp}$	0.032	-0.004	0.02	-0.001
	(0.038)	(0.047)	(0.033)	(0.022)
$\tau_1 + \tau_2^{Est}$	0.03	0.002	0.022	0.005
	(0.016)	(0.03)	(0.016)	(0.037)
$\tau_1 + \tau_2^{Pay}$	0.055	0.014	0.047	0.01
	(0.053)	(0.067)	(0.048)	(0.015)
Observations	104,865	15,575	15,575	14,760

Table A.8: Breakthrough Election Trend Breaks In Economic Activity Per 1,000 ZIP-Code Residents

	Log(Employees Per Capita)	Log(Establishments Per Capita)	Log(Payroll Per Capita)
	(1)	(2)	(3)
Pre-Breakthrough Trend	−0.001	−0.002	0.001
	(0.003)	(0.002)	(0.004)
Pre-Breakthrough Trend×BZ	0.005	0.004	0.004
	(0.009)	(0.004)	(0.012)
Post-Breakthrough Trend ×BZ	0.007	0.006	0.012
	(0.008)	(0.003)	(0.010)
Post-Breakthrough Trend	−0.008	−0.004	−0.009
	(0.003)	(0.002)	(0.005)
$\tau_1 + \tau_2$	0	0.002	0.003
	0.007	0.003	0.009
Observations	104,865	104,865	104,865

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. Model is weighted least squares estimates of equation A.4 weighted by 1990 ZIP-code population.

Table A.9: Breakthrough Election Trend Breaks In ZIP-Code Economic Activity

	Log(Employees)	Log(Establishments)	Log(Payroll)	HPI w/ 2000 Base
	(1)	(2)	(3)	(4)
Pre-Breakthrough Trend	−0.004	−0.005	−0.002	−0.031
	(0.003)	(0.003)	(0.004)	(0.016)
Pre-Breakthrough Trend×BZ	0.0005	0.0002	−0.0001	0.001
	(0.009)	(0.005)	(0.012)	(0.016)
Post-Breakthrough Trend ×BZ	0.007	0.006	0.012	0.008
	(0.007)	(0.003)	(0.009)	(0.020)
Post-Breakthrough Trend	−0.006	−0.002	−0.007	0.038
	(0.003)	(0.002)	(0.004)	(0.024)
$\tau_1 + \tau_2$	0.001	0.004	0.005	0.046
	0.006	0.003	0.009	0.029
Observations	104,865	104,865	104,865	84,452

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 A.4 weighted by 1990 ZIP-code population.

Table A.10: Relationship Between Black Mayors and Crime

	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)
Panel A: Entire Sample						
Black Mayor	−0.013	−0.013	−0.020	0.048	0.084	0.010
	(0.046)	(0.052)	(0.052)	(0.058)	(0.072)	(0.045)
Panel B: Mayor-Council Events						
Black Mayor	−0.029	−0.010	−0.018	0.047	0.089	0.018
	(0.041)	(0.049)	(0.048)	(0.078)	(0.091)	(0.062)
Observations	6,519	6,519	6,519	6,519	6,518	6,518

Notes: Coefficients are city-level, stacked least-squares estimates with the inclusion of a city-year-breakthrough-election trend. 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 17 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, are clustered at the city-by-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 A.11: Relationship Between Black Mayors and Residential Development

	Log Buildings Per Capita	Log Units Per Capita	Log Large Development Units Per Capita	Log Value Per Capita
	(1)	(2)	(3)	(4)
Panel A: Entire Sample				
Black Mayor	0.100	0.183	0.034	0.280
	(0.137)	(0.183)	(0.360)	(0.161)
Panel B: Mayor-Council Events				
Black Mayor	0.381	0.487	0.386	0.610
	(0.174)	(0.283)	(0.605)	(0.204)
Observations	6,236	6,236	5,289	6,236

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 5 units. The second panel only retains stacks where the mayor serves as chief executive. Sample includes 17 cities that directly 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, are clustered at the city-by-breakthrough-election level. Data come from the U.S. Census Bureau's Building Permits Survey at the place level from 1994 to 2018.

Table A.12: Relationship Between Black Mayors and Changes to Public Finances

	Log Debt Per Capita	Log Property Taxes Per Capita	Log Revenue Per Capita	Log Sales Per Capita	Log Total Taxes Per Capita
	(1)	(2)	(3)	(4)	(5)
Panel A: Entire Sample					
Black Mayor	0.082	−0.061	0.066	0.115	−0.048
	(0.190)	(0.041)	(0.065)	(0.336)	(0.031)
Panel B: Mayor-Council Events					
Black Mayor	−0.164	−0.134	0.019	−0.109	−0.074
	(0.090)	(0.062)	(0.061)	(0.170)	(0.052)
Observations	6,585	6,585	5,868	3,746	5,883

Notes: Coefficients are city-level, stacked least-squares estimates. The dependent variables are listed at the top of each column and are derived from the Annual Survey of Local Governments. Sample includes 17 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 1000,000 whose Black populations are above 10 percent that either observed an interracial election or elected a Black candidate more than eight years after the breakthrough election. 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 breakthrough election and the 8 years following the election. Standard errors are clustered at the city-by-breakthrough election level and are printed below the point estimates.