The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Rights Act [*][†]

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Abstract

A central concern for racial and ethnic minorities living in democratic societies is having access to opportunities for economic advancement equal to their majority counterparts. In this paper, we examine whether and how democratic institutions facilitate such advancement. We explore how the political re-enfranchisement of black Americans in the U.S. South, stemming from the passage of the 1965 Voting Rights Act (VRA), contributed to improvements in relative economic status during the 1960s and 1970s. Using spatial and temporal variation arising from the federal enforcement provision of the VRA, we document that counties where voting rights were more strongly protected experienced larger reductions in the black-white wage gap between 1950 and 1980. We then show how the VRA's effect on the relative wages of black Americans operates through two demand-side channels. First, the VRA contributed to the expansion of public employment opportunities. Second, in line with previous work on the importance of civil rights laws, the VRA contributed to and complemented the enforcement of labor market policies such as affirmative action and anti-discrimination laws.

Keywords: Racial Discrimination, Electoral Politics

1   Introduction

A half-century ago, U.S. Supreme Court Chief Justice Earl Warren hailed the right to vote as one that is "preservative of [all] other basic rights," social, civic, and economic.[1] The Chief Justice's view reflects a widely-shared belief in the franchise's power to protect vulnerable citizens and help them achieve a better standard of living. Guided by this belief, black Americans during the 1960s made voting rights a centerpiece of the Civil Rights Movement for social equality. Civil rights leaders viewed political representation as necessary to adequately address economic problems related to poverty, labor market disparities, and other aspects of minority disadvantage that plagued black communities during the first half of the 20th century (Button 1989). Reverend Martin Luther King, Jr., for example, called access to the ballot box "the foundation stone for political action...[w]ith it the Negro can eventually vote out of office public officials who bar the doorway to decent housing, public safety, jobs and decent integrated education."

The view that political rights have economic effects finds substantial support in the economics literature. Positive theories put forth in Romer (1975), Roberts (1977), and Meltzer and Richard (1981) suggest that that extending the franchise to marginalized groups should, by shifting the median voter toward poorer segments of society, increase pro-poor redistribution, and in turn reduce inequality. This redistribution can come in different forms, as decisions about how to deploy the resources of the state are shaped by political pressure from various constituent groups. Governments control what to distribute (be it public assistance, legal enforcement, government contracts, or public-sector employment), how much to redistribute, and to whom. As such, the political voice of minorities is a potentially important determinant of majority-minority inequality.

Despite positive predictions regarding the relationship between political rights and economic inequality, much remains unknown about the value of political voice for the marginalized. Studies have documented how minority voting rights shape government redistribution (Cascio and Washington 2014; Husted and Kenny 1997), as well as how voting can benefit the children of the newly-enfranchised by changing the composition of spending (Fujiwara 2015; Miller 2008). There is little evidence, however, on whether democratic participation concretely improves the material circumstances of minority voters themselves by making government accountable for their interests, as theorized by Meltzer and Richard (1981).

In this paper, we document that the political incorporation of a historically disadvantaged minority group can indeed generate socioeconomic benefits. We do so by examining one of the largest enfranchisement episodes of the modern era: the passage of the 1965 Voting Rights Act (VRA). The VRA outlawed discrimination at the voting booth directed against racial minorities (black Americans in particular).[2] As a consequence of the VRA, the size of the black American electorate increased almost overnight – particularly in the southern United States, where voting

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[1] *Reynolds v. Sims*, 377 U.S. 533 (1964).

[2] As we describe in greater detail in Section 2, black Americans were, in effect, banned from voting throughout the South via a variety of legal tactics–including the discriminatory implementation of literacy tests and poll taxes.

rights had been heavily restricted during the previous century.[3]

One of the stated goals of the VRA was to increase the responsiveness of local, state, and federal-level representatives to racial minorities' policy interests. These interests most commonly related to minorities' position of socioeconomic disadvantage. As previous research documents, the exclusion of black Americans from politics is best understood as a legal system that preserved white Americans' economic dominance in the South – by fostering economic segregation in places of employment and public accommodation, as well as other official and unofficial forms of economic repression (Roback 1984).[4] As such, the major policy concern of racial minorities throughout the South was equal access to economic opportunities in the public and private sectors of the labor market (Button 1989). We thus focus on identifying whether minority political empowerment achieved the substantive goal of producing opportunities for economic advancement within the labor market. Specifically, we examine the impact of re-enfranchisement on the Southern black-white wage gap between 1950 and 1980.

To identify the causal effect of voting rights on economic inequality, we exploit the temporal and spatial variation in voting rights protection afforded by the targeted application of the VRA. In addition to the law's blanket ban on voting discrimination (which applied nationwide), Congress reserved its strictest requirements—enumerated in VRA Section 5—for the southern counties and states where voting discrimination had been most severe. In these places, the federal government took active steps to register minority voters and protect against racial discrimination in voting. Section 5 applied to only a subset of counties and states, primarily in the South and Southwest United States(see Figure 2).

As with many geographically-targeted laws, there were likely factors that led Congress to designate those jurisdictions as "VRA-covered" but which also were correlated with minority labor market outcomes. Rather than comparing labor market outcomes across all covered counties to uncovered counties, which would largely amount to a cross-state analysis, we refine the strategy by focusing on cross-state and within-state adjacent county pairs, where one county is protected under the VRA, and the other not.[5] Our approach better approximates a quasi-experimental setting where the researcher compares "like" treated counties with "like" control counties. It mitigates concerns that smoothly-varying unobservable conditions (such as cultural, political, or economic differences) may confound traditional causal estimates obtained using standard state-level analysis.[6]

To preview our results, we first confirm that the VRA accomplished its most direct goal—facilitating the voting participation of black Americans in covered jurisdictions. We show that the

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[3] In the remainder of the paper, we will refer to the southern region of the United States as "the South." For our purposes, we define this region to include Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. For ease of the exposition, we also include Arizona, as it was one of the major areas affected by the treatment variation.

[4] While we do not discuss these in depth, forms of labor repression included vagrancy laws, and anti-enticement laws, and unaddressed racial violence ((Naidu 2012; Roback 1984; Christian 2017)).

[5] Figure 2 provides an overview of the nationwide variation in VRA coverage, discussed in greater detail in Section 2.

[6] Later in the analysis, we document that on a range of observable factors, counties that lie on state borders separating VRA and non-VRA covered jurisdictions are quite similar.

VRA did in fact lead to a sustained increase in political participation (turnout increased by 10% in VRA-protected counties), and also that it increased government responsiveness to minority interests as measured by legislator behavior. Proceeding to our main results, we then show that this episode of political empowerment was causally linked to the improved economic status of black Americans in the labor market. The VRA reduced the conditional wage gap between black and white workers by around 5.5 percentage points between 1950 and 1980 – a result that is robust to several alternative specifications. To mitigate concerns about either unobserved concurrent state institutional changes or differential trends, we leverage both the 1975 expansion of the VRA across over 200 counties in the Southwest, as well as treatment variation within one state (North Carolina) where just over 40 of the total 100 counties were covered. Across these subsamples, we document results regarding the impact of minority political power that are similar in direction and magnitude. Collectively, these results provide strong evidence of an internally valid reduced-form effect of minority political empowerment on the racial wage gap.

We next turn to a discussion of economic channels through which black Americans' political incorporation reduced economic inequality. We document a form of direct redistribution through which political power improved the labor market status of black Americans: government employment. We find that in VRA-covered counties, black Americans were between 2 and 4% more likely to receive a government job relative to white workers. By providing a causal mechanism relating to minority public sector employment, our analysis complements research showing how (particularly during the second half of the 20th century) public bureaucracies provided black Americans with greater opportunities for white-collar employment and upward occupational mobility that were less prevalent in the private sector (Blank 1994; Pitts 2011). We also show that in addition to the direct benefits for those workers who become employed by government, the VRA's impact on black public sector employment also contributed to the rise in black relative incomes observed in the *private* sector.[7] To demonstrate this, we leverage cross-occupational changes in nationwide public sector growth (a proxy for increased public sector demand within a given occupation). Using this variation, we show that in occupations where private firms face greater competition with the public sector for relatively inexpensive black American labor, minority workers experience sharper improvements in wages within the private sector. By improving the bargaining power of the black labor force, the changing composition of the public sector labor force (influenced by minority political power) contributed to wage equality in markets where discrimination existed previously.

The remainder of the paper continues as follows. In Section 2, we describe the institutional setting, including a discussion of the Voting Rights Act and the variation we exploit to generate credible empirical estimates. We discuss conceptually the expected effects of minority voting power in Section 3. In Sections 4 and 5, we discuss our empirical strategy and present our primary results. We empirically analyze different labor market mechanisms in Sections 6 and 7, and political

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[7] The increased public sector employment of black Americans put upward pressure on previously low black wages by improving their bargaining power–both by dampening private labor supply and improving the outside-option wage.

mechanisms in Section 8. Finally, we offer brief concluding remarks in Section 9.

2 Context & Historical Background

To fully understand the purpose and goals of the VRA, including its intended socioeconomic benefits, it is useful to understand the background of the American South, where its core provisions were targeted. In this section, we briefly discuss the political and social exclusion that the VRA was designed to address, as well as the economic disaffection left behind by the lack of voting rights. We then describe the passage of the statute, and how its passage created a "natural experiment" that we use to study the economic impact of minority political empowerment.

2.1 Pre-1965 American South: *De Facto* Disenfranchisement of Black Americans

After the Civil War and the end of American slavery, U.S. states ratified the Fifteenth Amendment to the Constitution, which guaranteed the right to vote to all men regardless of "race, color, or previous condition of servitude." Armed with the vote and its concomitant political power, black Americans in the South prospered for the first time in history, during the period known as "Reconstruction" (Logan 2018). This period of political and economic progress was short-lived. To reimpose the hierarchy that allowed for white Americans' social, political, and economic dominance, the former Confederate states responded to the expansion in black Americans' political rights by passing laws between 1870 and 1910 that, while facially neutral, completely restricted their political participation in practice. These *de facto* franchise restrictions were commonly referred to as "Jim Crow" laws).[8] As a result of Jim Crow-era political restrictions, most eligible black adults could not register to vote during the first half of the 20th century.[9]

The impact of Jim Crow political repression was not cabined to the electoral arena. The lack of influence and representation stunted the socioeconomic development of minority communities throughout the South. Black disenfranchisement changed the composition of local public goods, often leading to the outmigration of black families from the South (Naidu 2012). Research by Margo (1982), Kousser (1980), and Pritchett (1989) show that black disenfranchisement also significantly reduced the quantity and quality of schools attended by black children. Conversely, white Americans often prospered at the expense of dis-empowered black American laborers and families. White owners of capital and property in the postbellum South benefited substantially from black franchise

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[8] These restrictive laws included whites-only primaries, poll taxes, literacy tests, and ballot box laws. Poll taxes are straight-forward to understand. Whites-only primaries prohibited eligible black American voters from participating in primaries rather than elections, since primaries were semi-private organizations that fell outside government regulation. Literacy test requirements usually mandated that individuals read and explain a portion of a state's constitution in order to vote, with performance on such tests being left to the discretion of a local (white) election official. By 1904, every Southern state except Kentucky had passed some form of Jim Crow suffrage restriction. See Perman (2001) for a history of Southern minority disenfranchisement.

[9] Florida and Tennessee were the only southern states in which as many as half of all eligible black voters were registered. Other states of the South were considerably worse-off.

restrictions. Black Americans' physical insecurity and lack of property rights allowed employers to offer to protect black Americans from exploitation at the price of equal wages. As such, property owners could retain inexpensive black labor (Alston and Ferrie 1999).

2.2 Passage of the The Voting Rights Act & the Importance of Section 5

Black Americans were at a great disadvantage economically across the South due to political exclusion during the era of Jim Crow (Sundstrom 2007). As such, political equality became a centerpiece of the Civil Rights movement of the 1950s and 1960s. Policymakers and activists believed that voting rights were a necessary step (along with the political representation that comes with it) toward improved economic wellbeing for racial minorities. In 1965, months after civil rights activists' famous march from Selma to Montgomery, President Lyndon Johnson signed the Voting Rights Act into law, restoring for black Americans (and all minorities) the right to vote in the South. The VRA remains today the key statutory tool for attacking discrimination against racial minorities in politics.

The VRA outlaws all forms of racial discrimination in politics. The key enforcement provisions of the VRA are Sections 2 and 5. Section 2(a) prohibits the use of voting qualifications that deny the right to vote on account of race or color. Section 2(b) is the main instrument to combat political discrimination *nationwide*. Enacted to give life to the Fifteenth Amendment, Section 2 forbids all electoral structures that deny racial minorities the "opportunity...to participate [equally] in the political process and to elect representatives of their choice." This provision is commonly-used by minorities today to challenge both vote-denying policies (such as voter identification requirements) and vote-diluting practices (such as gerrymandered districts) (Ho 2018; Karlan 1989).

Section 5, however, was long considered the strongest provision of the VRA.[10] This provision of the statute sought to *affirmatively* give black Americans political voice in the areas of the country (primarily in the South) where their voting rights had been most suppressed. The provision applied only to a subset of states and counties (until 2013, when the Supreme Court effectively struck the provision down in the famous *Shelby County v. Holder* decision).[11] Counties and states covered under Section 5 were required to pre-clear any change to electoral procedures with the District Court of D.C. or the Attorney General.[12] Furthermore, Section 5 also provided for the appointment of federal examiners to covered jurisdictions, and required that applicants certified by examiners be placed on voter registration lists immediately. Election law scholarship suggests that this latter part of the VRA, while less-often discussed by researchers, was crucial for ensuring that previously

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[10] Maccoon (1980), for example, argues Section 5 was "one of the most useful statutory tools for the enforcement of voting rights," and Motomura (1983) describes it "as perhaps the most important for the continuing protection of minority voting rights."

[11] 133 S. Ct. 2612 (2013). The Supreme Court technically struck down Section 4(b) which provided the formula for determining which states would be subject to Section 5.

[12] To obtain federal approval of voting changes, preclearance jurisdictions were required to demonstrate that a proposed change would not have a "discriminatory effect" or "discriminatory purpose." The election law case *Beer v. United States* defined "discriminatory effect" as "retrogression:" any change that reduced the opportunity for minority voters to elect their candidates of choice.

discriminatory jurisdictions could no longer "foot drag" to register eligible black voters (Karlan 1989). Within two years of the VRA's passage, the Johnson Administration had used civil service employees to register more black Americans than had been registered in the entire century since the Fifteenth Amendment had been ratified.[13]

Section 5's "covered jurisdictions" were originally defined in the VRA's coverage formula (Section 4(b)) to include any city, county, or state that used a test or device (e.g., a literacy test) and had less than a 50 percent turnout in the 1964 presidential election. The coverage formula thus initially applied to counties in Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia, as well as 40% of the counties contained in North Carolina and additional counties in Arizona.[14] Amendments to the VRA in 1975 extended coverage to several more counties in the South and Southwest, including counties in Florida, Oklahoma, Arizona, and New Mexico, as well as all counties in Texas.[15]

Our main analysis will compare economic outcomes for individuals residing along the county and state borders that divide VRA-covered from uncovered jurisdictions. Before proceeding to our empirical analysis, we discuss briefly why we might expect changes in the economic fortunes of blacks after the massive political shock to the South brought about by the VRA.

3 Conceptual Discussion: Economic Effects of Minority Political Power

The VRA gave black Americans in the South meaningful political voice for the first time since Reconstruction. To the extent politicians became responsive to the needs of minorities, as would be predicted by Cox and Mccubbins (1986), Dixit and Londregan (1996), and Lindbeck and Weibull (1987), the VRA should have (weakly) increased public resources flowing to black communities that had previously been unsupported by government. In these models, elected officials distribute resources to clearly identifiable constituent groups to maximize votes. As such, the VRA created strong incentives for Southern politicians to respond to the policy needs of black American communities, since this group of voters tended to be geographically clustered and have similar political preferences (Keech 1968).[16]

Whether and how minority political influence affected individual economic outcomes, however, depended on the policy preferences of the newly-enfranchised group. Without question, the central

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[13] For a discussion, see Davidson and Grofman (1992)).

[14] The original coverage formula looked at whether jurisdictions imposed discriminatory procedural devices at the time of passage, whether less tha50 percent of the voting-age population was registered to vote as of that date, or if less than 50 percent of eligible voters voted in the November 1964 presidential election.

[15] A handful of jurisdictions in California, New York and New Hampshire that had continued to administer literacy tests were also brought under Section 5 preclearance in 1970. Because our focus is on the effects of eliminating Jim Crow inequality, we do not consider these jurisdictions.

[16] As pointed out by Cascio and Washington (2014), that black Americans did not comprise a majority of the electorate would not necessarily preclude a causal relationship between their voting eligibility and the provision of various constituent services. Politicians may distribute resources such as local government employment or school improvements to several constituent groups that are able to construct a minimum winning coalition.

political concern for black Americans was equal access to employment opportunities, free of discrimination (Schwartz 1967). Figure 1 presents original survey data from a 1963 survey conducted by the National Opinion Research Center. The data indicate that by a large margin, equal opportunity for advancement within the labor market was the central policy concern of black Americans during the time in which the VRA was passed. As such, if the VRA achieved its intended effect of making government responsive, labor market inequality should have been a margin on which government was expected to act.

Figure 1: Political Attitudes of Black Americans, 1963

Survey Question Asked: "Which Political Issue is Most Important to You?"
(Source: NORC)
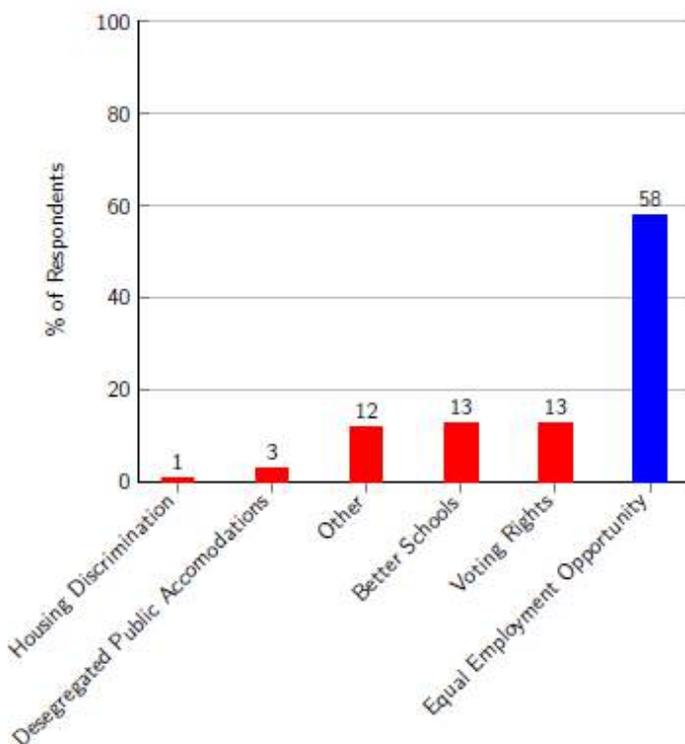
In principle, minority-preferred politicians may have improved minority workers' labor market outcomes (such as wages and employment) by altering either the demand for or supply of black American workers. On the supply side, black political empowerment may have contributed to economic equality (including within labor market) because investments in human capital-building institutions (such as schools and hospitals used by minority children) would lead to an increasingly skilled minority workforce. Qualitative and quantitative historical evidence suggests that the political representation gained under the VRA did indeed lead to such institutional improvements (Cascio and Washington 2014).

Minority-preferred politicians may also have altered the demand side of the labor market.

Elected officials during this period had significant authority over the distribution of public contracts and policies that could discourage discriminatory practices. Minority political power may also have influenced the regulatory and legal enforcement power of the state. For example, research by Stainback, Robinson, and Tomaskovic-Devey (2005, Shulman (1984) shows that political pressures affected the enforcement of civil rights equal employment opportunity law and affirmative action mandates.

Minority-preferred politicians can also directly affect the demand for minority labor through their direct influence on government hiring. Relative to private-sector firms, government agencies may optimize over different objective functions when making employment decisions. While a private firm will be primarily concerned with profit maximization, government officials responsible for public sector hiring may be guided by non-production factors (alongside concerns about public good provision), such as equity or other political goals. For example, government actors may seek to reverse historical patterns of discrimination through the employment/promotion of minority and women workers (Blank 1994). Recent empirical research confirms that public sector employment— particularly at the local level—is shaped strongly by political considerations (Alesina, Baqir, and Easterly 2000; Enikolopov 2014; Chen, Henderson, and Cai 2017).

Numerous quantitative and qualitative accounts indicate that government jobs provided a source of economic opportunity for black Americans at mid-century. Opportunities for movement up the job ladder were much more common in the public sector (Freeman 1976; Hout 1984).[17] While the black share of public employment was lower historically in the deep South counties during the pre-Civil Rights decades, one observes a much steeper (relative) increase in the fraction of black Americans working in the public sector between 1960 and 1970 (the slope change for black public sector fraction is much larger for VRA than non-VRA states), after the year in which the VRA was passed. Moreover, the trends in share of workers in public employment follow a similar path through 1960. White public-sector employment, on the other hand, follows a similar time path both before *and* after passage of the VRA. We interpret these raw statistics as *prima facie* evidence that the VRA may have played a role in the racial composition of the government workforce.

The impact of this public sector compositional change on the minority workforce may not have been limited to the direct effect of having a less-discriminatory (and better-paying) form of employment. The state's effect on wages through public-sector hiring may also have exerted indirect pressure on private sector employers. Because government agencies often comprised a non-trivial share of the local labor market (often more around or more than 20% of local employment), both the reduction in labor supply from the positive sectoral demand shock, as well as the better outside-option wage, may have exerted upward pressure on private sector wages. To the extent that responsive officials increased the hiring of minorities in government, the improved likelihood of employment in the public sector at higher wages likely generated a substantial increase in the bar-

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[17] Freeman (1976) demonstrates that black public employees earned more than their private sector peers, and Eccles (1975) demonstrates that the ratio of black-to-white income within federal government jobs was higher than the economy-wide average for male college graduates.

gaining position of workers in the private sector through improvements on their outside options.[18] Such inter-sector general equilibrium effects have recently been formalized and estimated in the labor economics literature. Within the context of changes to local industrial composition, Beaudry, Green, and Sand (2012) document that sectoral demand shocks generate substantial cross-sectoral general equilibrium effects. When accounting for these GE effects, total wage effects are 3-4 times larger than the effects as measured by just considering the direct effects.[19] Consequently, a complete evaluation of the economic impact of a public sector channel (caused by an exogenous political shock) may need to account for such GE effects.

To summarize, in this study we provide evidence that one of the effects of political empowerment via the VRA was the redistribution of labor income to racial minorities who long suffered from private-sector employment discrimination. Empirically we show how labor market equality improved, the labor mechanisms through which these changes took place, and the political triggers that explain such improvements in black labor market status. In showing this, our study directly contributes to different lines of research—in particular, to research on political participation, on labor markets, and on racial inequality.

4   Research Design & Methodology

In this section, we explain the details of our empirical approach. Our goal is to evaluate whether black Americans' right to vote improved the economic status of black workers (relative to their white counterparts). We exploit the temporal and spatial variation in federal voting rights protection under the VRA's Section 5 (which we will refer to as simply the "VRA") to test this hypothesis in the American South. Racial economic disparities were believed to have been most acute here prior to the Civil Rights movement (Sundstrom 2007). The VRA created sharp, discontinuous changes in whether political participation by minorities would be protected by the federal government. These changes provide quasi-exogenous variation that we use to measure the role of political rights in remedying economic disparities.

4.1   Data Construction

We rely on several sources of historical data in our study, and so focus on the primary sources here, relegating secondary sources to the Data Appendix. Our primary data sources are the restricted-access United States Decennial Censuses (DEC) from 1950, 1960, 1970, and 1980. The long-form Censuses are a 20% representative sample of the U.S. population (except for 1950, for which only a 1% sample is available). We restrict our sample to working-age adult males working full-time, to remain consistent with previous research on the racial wage gap over the twentieth century

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[18] This upward pressure on private sector wages due to changes in the public sector can operate regardless of whether the outside option is exercised—in other words, regardless of whether private workers transition into the public sector.

[19] To formalize similar labor market dynamics in our setting, which incorporate political pressure and the interaction of public and private sectors wages, we provide a model in Appendix C.

(Smith and Welch 1989; Donohue and Heckman 1991; Card and Krueger 1992). The time period we analyze includes fifteen years before and after the peak of the Civil Rights movement, 1965, when the VRA was passed. The advantage of using the restricted-use Censuses is that they contain detailed information on a respondent's location of residence (including county), allowing us to compare wages for individuals that reside only in neighboring covered and uncovered *counties.* Our primary variable of interest is an individual's hourly wage.[20] The long-form DEC also contains individual information on demographic variables such as individual race, gender, and age. Moreover, DEC also has additional individual and household-level information that allows us to explore other outcomes and potential mechanisms. These include type of employer (public vs. private), occupation, county/state of work (separate from county of residence), migration status, and educational attainment

The counties covered under the VRA comes from the U.S. Department of Justice's Civil Rights Division. While we are primarily interested in the socioeconomic impact of empowerment, we also validate that the VRA had its intended impact on political participation. To this end, we also make use of county-level voting data from the Interuniversity Consortium for Political and Social Research (ICPSR) and Dave Leip's Atlas of U.S. Presidential Elections. Voting age population estimates are based on interpolation from the Census demographic data.[21] These data are used to construct county-level estimates of voter turnout (the share of votes cast to eligible voting population) in all presidential elections from 1948-1980.[22] To examine changes in politician responsiveness preferences, we use two other sources. First, we use district-level measures of political ideology and party affiliation for the 87th through the 100th Congresses from DW-NOMINATE data constructed by Poole and Rosenthal (2001). Second, we use a coding of all congressional roll-call votes (by district and year) in favor of civil rights-related issues, produced by Schuit and Rogowski (2017).[23]

We also make use of several other sources to probe robustness and mechanisms further. County-level control variables are based on public-use Decennial Census estimates. Overall levels of government hiring and expenditures at the county level are based on data from the U.S. Census of Governments. To examine political channels, such as the impact of the VRA on the election black politicians, we digitize over ten years of original data from annual reports produced the Joint Center for Economic and Political Studies (JCPES). The JCPES produces an annual listing of every black politician in the country (except for 1970), beginning in 1969.[24] We supplement this source with data from Matthews and Prothro (1966), who collected information about black elected officials in the early-1960s South.[25] Details on this data as well as other data construction can be found in the Data Appendix.

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[20] We use the log transformation of this outcome as our dependent variable.

[21] Interpolated estimates were obtained from (Gentzkow et al., 2011)

[22] We do not examine party vote shares given the concurrent period of Southern partisan realignment (Kuziemko and Washington 2018).

[23] We thank Jon Rogowski for generously providing us with this data.

[24] For 1969, the listing contains only states in the South, not the Southwest.

[25] This data in an easily-usable form was generously provided to us by Jim Alt.

4.2 Sample: Cross-border County Pairs

An obstacle to identifying the effect of the VRA is that covered jurisdictions were not randomly singled out for additional voter protections. Rather, coverage was deliberate: the VRA targeted the "worst of the worst" in terms of political discrimination against racial and ethnic minorities. One concern, then, is that the unobservable characteristics (including social, cultural, economic political conditions) that led to coverage may also be correlated with economic outcomes, creating bias. For instance, states such as Alabama, Mississippi, and Georgia had considerably more lynchings than other states (Naidu 2012). Prejudicial views about minorities are likely correlated with both political and economic outcomes.

To mitigate concerns about potentially unobservable confounders, we analyze data for the subset of adjacent county-pairs that straddle Section 5 state and county boundaries. This approach is increasingly used in observational studies of policies such as the minimum wage, tax rate changes, and health insurance expansions (Dube, Lester, and Reich 2010; Duranton, Gobillon, and Overman 2011; Feigenbaum, Hertel-Fernandez, and Williamson 2018; Clinton and Sances 2018). The intuition is straightforward: focusing on neighboring counties allows us to compare "like" jurisdictions with "like." Many cultural, political, or economic conditions – each/all of which may affect our outcomes of interest – are likely to vary smoothly rather than discrete at jurisdictional boundaries. Our approach thus mitigates concerns about smoothly-varying unobservable conditions confounding causal estimates based on data from the universe of counties (or based on state-level data, where possible).[26] Two counties separated by a border either across or within a state should appear more similar than groups of counties far away, or then entire states. The underlying assumption of our approach is that after controlling for border pair-by-race-by-year fixed effects—which together net out any time-varying, pair-specific shocks to black or white wages—any changes in outcome gaps between black and white workers are attributable to the VRA, rather than to other characteristics of the two sets of counties.

4.2.1 Matched-pair Sample: How Similar are Neighboring Counties?

We create our sample by first focusing on all states where at least one county is covered by the VRA as well as that state's neighboring states. This approach accounts for both the cross-state or within-state variation in VRA coverage.[27] We create our border county sample by limiting to all pairs of adjacent counties where one county is VRA-covered and the other county is not. The counties that are represented in our sample can be seen in Figure 3 below.

We can verify that our research strategy better approximates an "apples-to-apples" comparison using data. Table 1 presents summary statistics for our data and time period – including average

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[26] Even state-level analyses would be complicated by the fact there is much within-state heterogeneity coverage by 1975 (Ang 2018).

[27] This includes all or parts of Alabama, Georgia, Louisiana, Mississippi, South Carolina, Virginia, Texas, North Carolina, Florida, Kentucky, Arkansas, Tennessee, Oklahoma, West Virginia, Maryland, Utah, New Mexico, and California.

county characteristics based on data from the Census as well as other sources. These average county characteristics provide evidence regarding both the use of our design, and also suggest that differences between counties are attenuated when we restrict to neighboring covered and uncovered counties. Panel A of Table 1 displays average county-level characteristics for Southern states across all VRA-covered and non-covered counties in 1960—pre-dating the passage of the VRA. Columns (3) and (4) present means as well as t-test results for tests of the equality of average and treated county means, where the null hypothesis is that the means of Section 5 and non-Section 5 counties are equivalent. As Panel A demonstrates, the differences between Section 5 and non-Section 5 are often always different at the 5% level, for an array of observable characteristics. Thus, these summary statistics suggest that economic and political conditions were very different in Section 5 and non-Section 5 Southern states, and suggest that there could be fundamental unobserved differences between these states that would confound an analysis of Section 5's causal impact using state-level averages. Panel B suggests that our assumption of smoothly-varying changes across borders is valid. All difference-in-means tests produce differences that are not significantly different from zero.

4.3 Empirical Specification

Using data for the set of adjacent VRA and non-VRA counties, we employ a generalized differences-in-differences design, comparing changes in the outcomes between VRA-covered and uncovered counties, before and after the VRA took effect. In addition, because the VRA sought to affect the political participation of *black* Americans, we compare the differential in wages for black workers to white workers. In short, we estimate the causal effect of the VRA on the black-white racial wage *gap*.[28] The primary empirical specification takes the following form:

$$\log(Y_{ict}) = \beta \left[\text{VRA}_{ct} \times \mathbb{I}\left\{ r(i) = \text{Black} \right\} \right] + \mathbf{x}_{ict}\gamma + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{ict} \tag{1}$$

In this difference-in-difference-in-differences (DDD) specification, c indexes county, $r(i)$ indexes the race of person i, t indexes year, and $p(c)$ indexes a given county pair. VRA is a binary indicator variable for whether a county was was VRA-covered in a given year (post-1965 or post-1975). $\mathbb{I}\left\{ r(i) = \text{Black} \right\}$ is an indicator for whether a worker is a black American. Because we limit to black and white workers only, Black equal to 0 means that a person is white. Our primary dependent variable is the log hourly wage measured for person i in county c, year t (although we also analyze other outcomes).[29] The parameter of interest is β, the coefficient on the interaction $\left[\text{VRA}_{ct} \times \mathbb{I}\left\{ r(i) = \text{Black} \right\} \right]$ (which we will refer to as VRA × Black for ease of exposition). This interaction term thus takes the value of 1 if an individual is a black American and lives in a VRA county after the year in which the law took effect, and 0 otherwise. We include all race, county, and

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[28] The approach we use is discussed in several papers. For an example, see a recent study by Hirata and Soares (2016), who estimate the impact of trade liberalization on the minority-white wage gap in Brazil.

[29] Hourly wages are constructed from DEC data on wage income earned last year, weeks worked last year, and average hours worked per week.

year fixed effects, and two-way interactions. We also control for observable skills using experience (defined using a worker's reported age), experience-squared, and education. We allow the returns to observable skills to vary by year (γ_t) to account for changes in the wage structure during the second half of the 20th century (Katz and Autor 1999).

As is standard in a difference-in-differences (or DDD) approach, the identifying assumption is that there are no omitted factor correlated with the passage of the VRA that affects our outcome of interest. As it is difficult to completely rule out this concern in an observational setting, we also report specifications that include interactions of county-level variables measured before the enactment of the law and that are plausibly correlated with its passage. County-year fixed effects (μ_{ct}) make our estimates robust to unobserved county labor market shocks that occur over time. County-race fixed effects ($\mu_{cr(i)}$) make the estimate robust to county-specific race-specific differences that constant over time. County pair-year-race allow us to control for very local time-varying spatial heterogeneity in relative outcome trends. Variation within county pairs with different VRA protection statuses identifies the effect of the VRA on economic outcome gaps. As previously discussed, the benefit of our localized approach is that non-institutional factors, such as culture or prejudice (e.g., sources of *de facto* discrimination against black Americans), that may affect the racial wage gap are unlikely to vary discretely at jurisdictional borders. Such factors, however, may vary substantially between entire states, or across counties that are far-flung from one another (Naidu 2012).

The parameter of interest is β, which captures the impact of the VRA on the wage gap between black and white workers (conditional on education and experience). If minority political rights improve economic welfare, one would predict minority relative wages to rise (or equivalently, minority wage *disparities* to be reduced) in areas where minority voting rights are protected under the VRA. As such, we expect $\beta_1 > 0$. Voting rights protection should be associated with reductions in the wage advantage that white workers have in relation to black workers. Our identifying assumption is the standard "parallel trends" assumption: that the difference between black and white workers would have evolved similarly in VRA and non-VRA counties after 1965 (1975) in the absence of the VRA (VRA Amendments). In our approach, black and white wages can evolve differently from each other, but the wage differential between these two groups would have evolved similarly in VRA and non-VRA border counties in the absence of the law. In order for our estimates to to be biased, there must be a trend or an event at the time that the VRA takes effect in a county that affects black and white workers differently, and this pattern must not be consistent across neighboring counties. As an example, if we hypothesize that the tight economy of the 1960s was changing wages during this period of time in Southern states, with each state having different patterns of changes, then the effect of the VRA would also have to be different for black and white adults to bias our results.[30] Because VRA coverage created substantial treatment heterogeneity both across states and counties, as well as because counties that were either initially covered within

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[30] As reviewed in Section 2, we are unaware of other policies that occurred only in VRA-affected states and counties that affected black and white workers differentially at the time of adoption/expansion.

control states having "bailed out" of coverage at various points after 1965, we cluster at the county level for inference.[31]

We can indirectly assess the identifying assumption in a few ways. First, as shown in Table 1 we can demonstrate that in terms of many different economic, social, and demographic characteristics, there is no significant difference in county trends. Moreover, using an event-study framework, we can demonstrate that there was little change in the wage gap in VRA vs. non-VRA counties in the years leading up to coverage taking effect. Unfortunately, because most counties became treated in 1965, and the RDC DEC data extends only until 1950, our pre-period is short. We can provide more compelling evidence of the common trends assumption when examining the effect of the VRA on the political mobilization of black American voters, which is measured with greater frequency.

We also include other county-level characteristics that may affect labor market outcomes. We generally focus on factors that are measured prior to adoption of the VRA (typically using 1960 data – prior to the VRA passage – so measures are not affected by treatment). These factors include demographics (black population, mortality rates, and literacy rates), as well as average cultural/political measures (historical presence of lynching, or the fraction of the population that votes Republican).[32] County characteristics are interacted with linear, quadratic, and cubic time trends.

5 Results

5.1 First Stage Effects: VRA Increased Political Participation and Politicians' Responsiveness to Minority Interests

As discussed in our conceptual discussion in Section 3, the link between black Americans' political empowerment and improvements in economic equality would likely depend on the power of the VRA to mobilize black voters and change how governments serve black voters' interests. We thus begin by assessing the VRA's effects on several intermediate outcomes (in other words, to demonstrate a "first-stage" effect of political incorporation). First, did the VRA *actually* increase black citizens' registration and voting behavior? And second, did the VRA in turn improve how politicians responded to the needs of newly-empowered minority voters? Before testing our main hypothesis, we first confirm the existence of a first-state political effect of the VRA by documenting the law's impacts on both political participation and politicians' votes on minority-favored legislation.[33]

5.1.1 Minority Political Participation

To demonstrate that the VRA increased actual voter participation as intended, we examine voter turnout for presidential elections. Our main outcome is the fraction of the voting-age eligible

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[31] For robustness, we also cluster our standard errors at the state level. Standard error magnitudes are similar, and will be included in an appendix.

[32] Data on county characteristics comes from the Decennial Census as well as the City and County Data Books, which are themselves typically based on official Census statistics.

[33] These effects are consistent with results in political science, including from Fresh (2018).

population that votes within a given county and election year. We estimate the following differences-in-differences (DD) analogue of the main specification:

$$\text{Political Outcome}_{ct} = \alpha \, \text{VRA}_{ct} + \mu_c + \mu_{p(c)t} + \epsilon_{cp(c)t} \qquad (2)$$

where c indexes county, t indexes year, $p(c)$ indexes county pairs.

α can be interpreted as the causal effect of coverage on voter turnout, and provides unbiased estimates of the VRA's political effect under the assumptions that there are no time-varying differences between covered and un-covered counties that affected both coverage status and post-VRA political outcomes and, that there are no geographic spillovers between counties. Spillovers may be a concern if black voters chose to move to counties covered by the VRA, or whites chose to move out of covered counties to avoid the threat of a newly enfranchised minority (likely leading to over-estimating the effect of coverage).[34]

Table 2 presents results based on Equation 2 above. Consistent with the VRA creating a new block of eligible voters, we find large and persistent increases in eligible voter turnout resulting from the extension and protection of minority voting rights under the VRA. In Columns (1) through (4), we estimate different specifications — starting with sparsest model, and sequentially adding more demanding control variables (first county characteristics, then state-specific linear trends). We find statistically significant increases in voter turnout in presidential elections across all specifications, ranging from 6 to 12 percentage points (p.p.) between 1950 and 1980. All estimates are significant at the 1% level. We also find that the similar results are obtained when we expand the sample of counties to include not just the neighboring pairs of counties, but entire states that either have a county covered by the VRA, or border such a state. These results are presented in Columns (4) and (5) of Table 2.

While increased voter turnout provides evidence of direct political participation, one limitation of the data is that it is not race-differentiated. One could imagine this is a problem, for example, because the VRA may also mobilize white voters whose sympathies lie with pre-VRA southern segregationists (we calll this effect, "white backlash"). To show that this is not a concern, we test whether the effects of the VRA on overall turnout are larger in counties that are, in essence, "more affected" by the VRA due to larger pre-VRA black population shares. To this end, we follow Cascio and Washington (2014) in estimating a DDD-style regression in which the VRA treatment indicator is interacted with the share of a given county's population that is black in 1960.[35] Consistent with the observed mobilization effect being the consequence of increased *minority* participation, the coefficient estimate for VRA × Black is positive and significant (columns (6) and (7) of Table 2).

We present the same results graphically in an event-study framework in Figure 5. The graph provides estimates using event-time dummies interacted with a dichotomous indicator for counties that ever became treated, with county and county pair-year fixed effects (standard errors clustered by county). The graph provides visual evidence of "first-stage" political effect, and also support

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[34] We rule out the likelihood of migration later in the analysis (see Section **??**).

[35] Cascio and Washington (2014) use this specification to show how the removal of literacy tests from Southern states affected participation and redistribution.

the parallel trends assumption — the estimates are relatively stable in the years before the VRA takes effect (although admittedly turnout is slightly worse in VRA counties—although consistently so). Treated counties experience a large and persistent increase in voter turnout that rises up to 15 years after a county becomes covered.[36]

Voter turnout in our sample of Southern/Southwestern counties was approximately 50% of eligible voters in 1980, the impact of the VRA accounts for about one-fifth of overall turnout. In terms of how realistic the magnitude of our estimates are, it is useful to consider two very recent papers that also examine strictly the voter turnout effects of the VRA. Limiting analysis to only counties within North Carolina, Fresh (2018) finds finds that VRA coverage increased overall voter turnout by 10-19 percentage points. Similarly, focusing on primarily the set of counties that were affected by the 1975 VRA Amendments, Ang (2018) finds that Section 5 coverage increased voter turnout by 4-8 percentage points, due to persistent improvements in minority political participation.[37] Finally, while the focus in this section is on turnout in presidential elections, the impact of the VRA is evident across all elections. In Appendix Table B1, we observe similar positive and statistically-significant effects on congressional election turnout.

5.1.2 Political Accountability and Policy Outcomes

The power of the franchise to improve the socioeconomic condition of minorities also implicitly relies on government actors who respond to the minority electorate. We thus also examine whether minority political mobilization in turn led politicians in the post-VRA South to actually *represent* the policy interests of newly-empowered black voters. There is (to our knowledge) no comprehensive data on voting records within either state legislatures, city councils, or other sub-federal legislative bodies that extends as far back as the pre-Civil Rights era. Historical data does exist, however, for the roll-call voting behavior of Congressional representatives. We use this data to test the prediction of changes in government responsiveness.

First, we examine whether the VRA impacted the voting record of Congressional representatives on the widely-used DW-Nominate score for political ideology. This measure collapses a representative's legislative roll-call voting into a time-varying, individual-level measure of conservatism, scaled from -1 to 1 (the score is increasing in conservativeness). It has been employed by numerous political scientists and economists to examine changes in political ideology over time.[38] We focus primarily on the second-dimension of the DW-Nominate score, since it captures legislator

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[36] We note, though, that there does seem to also be an increase in turnout in the one period before the VRA takes effect. We believe that this is consistent with increased social activism during the peak period of the Civil Rights movement, when organizations such as the NAACP, CORE, and Southern Christian Leadership Conference (SCLC) were actively engaged in voter registration drives during the early 1960s.

[37] The estimated treatment effects on turnout are also in line with older work by Filer, Kenny, and Morton (1991) and Highton (2004), who document that banning literacy tests increased turnout by 2 to 9 p.p., and banning poll taxes by 13 p.p. to 15 p.p.

[38] See, for example, Poole and Rosenthal (2001) or Gentzkow, Shapiro, and Taddy (2016) for descriptions of the measures' usefulness.

ideology on issues related to race and civil rights.[39] We use these data to estimate a district-level analogue to Equation 2 on district ideology with respect to racial political views. We define "VRA districts" as any district that contains at least one VRA-covered county, consistent with similar studies within this literature (Ang 2018). Our hypothesis is that the preferences of representatives from VRA districts should have become more aligned with the interests of black American voters – which presumably would be captured in their Congressional voting record.

Results are presented in Table 3, again using several different specifications and recalling that a negative score indicates a more liberal Congressional voting record. The results indicate that legislators from districts where voting rights were strongly protected became more ideologically-liberal (on race-based issues), by 6-8 percentage points (Columns 1 and 2). The coefficient for VRA is negative and statistically significant at the 10% level. These results compare favorably to recent estimates produced by Schuit and Rogowski (2017), who find when using a different measure of Congressional district responsiveness that VRA coverage increased support for civil rights legislation by twelve p.p. Column (3) shows that the effects of the VRA were stronger where black voters composed larger share a of the electorate. This result is robust to a wide range of model specifications. Finally for robustness, in Columns (4), (5), and (6) of Table 3, we repeat this exercise for the 1st Dimension of the DW-Nominate score, which measures a representative's legislative overall liberal-to-conservative ideology. We find little evidence that the VRA made representatives more liberal overall, although we do find that Congressional representatives become more liberal in general within places where the eligible minority vote share was higher (Column (6)).

We can also provide evidence that the VRA affected policy outcomes at the state or local levels. Table 4 presents evidence of how the VRA changed the allocation government spending in a manner that benefited black communities. Panel A examines how VRA coverage affected the distribution of public assistance benefits (such as welfare or UI benefits).[40] Prior to the VRA, black Americans were often shut out of access to social safety net resources. Historical accounts of the early 1960s, for example, suggest that during the era in which President John F. Kennedy started to expand anti-poverty programs, the provision of services to black American families was limited. Indeed, during Congressional debates during the buildup to the VRA's passage, advocates for a strong voter protection bill believed that minority political power would ensure that President Johnson's newly-initiated War on Poverty through social spending would not become a war waged "for white people only." As our estimates suggest, VRA coverage is positively associated with the per capita public assistance recipients.

In Columns (2)-(4) we estimate a triple-differences framework by further interacting the VRA

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[39] For robustness, we also use a measure constructed by Schuit and Rogowski (2017), which is the natural log of all total votes cast in favor of civil rights legislation in the Congress. The data for this exercise was generously provided to us by Jon Rogowski. Our results essentially serve only to confirm that their previous results hold for the subsample of districts that are contained in our analysis. Results available upon request.

[40] We use data from the Census County Data Books, which has data in 1964 and 1980 on the number of public assistance recipients in a given county.

indicator with the pre-VRA black population share. This regressions provides even stronger evidence that the VRA increased access to social spending. We observe differentially higher levels of public assistance support in counties with higher black population shares in 1960 (Columns (2)-(4) of Table 4). Each percentage point increase in black population share increased the percentage of county residents receiving public assistance by between 0.07 and 0.1 percentage point. Given that four-in-ten (41.8%) of black Americans were poor in the mid-1960s (DeSilver 2014), facilitating access to welfare support was likely a key role for government actors who cared about the social wellbeing of black Americans. These estimates are consistent with a shift in the distribution of state transfers toward areas with higher minority population shares—which we would expect to matter after black Americans could exercise their voting rights. In Columns (5)-(7), we re-estimate the core specification of Cascio and Washington (2014), to show that the VRA also increased the within-state distribution of state transfers.

5.2 Main Results: How did the VRA affect Economic Inequality?

The previous subsection confirms that the VRA achieved its initial purpose of minority political incorporation. We now turn to the main hypothesis— testing whether the minority political empowerment produced tangible economic gains. As Figure 1 above demonstrates, eliminating labor market discrimination was perhaps the most salient political demand in the minds of black Americans during the 1960s, and so we think it would be reasonable to hypothesize that minority power would be deployed to remedy this persistent problem.

We begin the discussion of our main findings by examining the effect of the VRA on labor market performance using a simple DD framework. Figure 6 presents visual estimates of the impact of the VRA on wages for both black and white American workers (separately) as dependent variables. We note a few observations based on this graph. First, wage trends in the decade before a county becomes covered are quite similar for both black and white workers. We view this as additional evidence in support of the identifying assumption that outcome trends between treatment and control groups would have evolved similarly in the absence of treatment. Second, after the VRA takes effect, we observe a mean increase in the wages of black Americans (close to 5 p.p., significant at the 5% level), as well as a modest *reduction* in white wages of approximately 1 p.p. Viewed together (in conjunction with the fact that black full-time workers earned about 60% of what similarly-situated white workers earned in terms of wages), Figure 6 suggests that the VRA did indeed improve racial equality within the labor market. Table 5 shows that these results are stable to multiple potential specifications.

For the remainder of the paper, we focus on the main estimating equation, Equation 1. Table 6 presents our main results on black Americans' relative wages under several model specifications. Recall that the coefficient β on VRA $\times$ Black indicates to us the impact of the VRA on black wages relative to white wages – so an increase in black wages in this model is also indicative of a reduction in the racial wage *gap*. Across all specifications, the results suggest that the VRA caused a statistically significant improvement in black Americans' relative labor market status. Column 1 presents our baseline estimates – using only individual worker characteristics (with returns to

human capital varying by year), and the full set of fixed effects. The regression estimate indicates that the VRA caused a 5.5 p.p. increase in black Americans' wages between 1950 and 1980, relative to white workers with the same characteristics and within the same geographic labor market. This impact is significant at the 5% level. Columns (2) through (5) show that this effect is robust across different specifications. Column (2) re-estimates the baseline model with the inclusion of several county-level controls. Because some of these controls could themselves be outcomes of the VRA (e.g., share of county population population that is non-white), we fix all controls at their pre-VRA (1960) levels, and interact the variables with linear, quadratic, and cubic time trends. The results are similar in size and significance – the VRA increased black wages by 5.8 p.p., relative to white wages (significant at the 5% level). Columns (3) and (4) add state and county trends respectively – and the results again confirm a statistically significant increase in relative black wages of about 5.6 p.p. Overall, these results provide strong evidence that the expansion and protection of black political participation rights had a strong effect on socioeconomic equality.

The magnitude of our main effect appears reasonably when situated in the existing literature. Conditional racial wage ratios within our sample increased from around 55% to just above 80% between 1960 and 1980. As such, our estimates account for around 1/5 of the decline in the adjusted wage gap — but only within the South (this estimate would be small if we expanded to consider the whole country). By contrast, Card and Krueger (1992) find that about 15-20% of the nationwide reduction in the racial wage gap owes to improvements in school quality for black American schoolchildren. Donohue and Heckman (1991) find that declining labor force participation due to President Johnson's War on Poverty about 10%-20% of black-white wage convergence during the Civil Rights movement. Finally, an outstanding recent study by Derenoncourt and Montialoux (2018) find that the 1966 extension of the minimum wage via amendments to the Fair Labor Standards Act can explain more than 20% of the reduction in the racial earnings gap.

Examining effects year by year using an event-study design also provides information regarding the linearity of treatment effects, useful for unpacking mechanisms (a point we will return to in later sections) (Kose, Kuka, and Shenav 2018).[41]

Figure 7 presents the visual display of these estimates. A limitation of using RDC micro data is that we are limited to the long form censuses only beginning in 1950. We thus have just one pre-period difference in our main outcome. Accepting this limitation, we nevertheless observe little change fin the black relative wages in the ten-year window prior to a county becoming VRA covered – providing evidence consistent with our political effects that outcomes in covered and uncovered

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[41] In particular, we estimate:

$$\log(Y_{ict}) = \sum_{t=-2}^{2} \mu_t \times [\text{VRA}_c \times \mathbb{I}\{r(i) = \text{Black}\}] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \qquad (3)$$

where s and t reference state and Census year as before. τ denotes the event year (1965). The dependent variables are the same as before. The parameters of interest are the four π's that we estimate. They separately test for mean shifts in individual economic outcomes post-VRA, after adjusting for pre-existing trends. Five years before the VRA takes effect in a county is our reference year in this regression.

were not following different trends prior to VRA coverage. Note, however, that the effects of the VRA on the black-white wage gap appear relatively soon after the VRA is enacted, with covered counties experiencing a 5% reduction in the wage gap by five years post-coverage. The speed with which the wage effects occur are consistent with work by Donohue and Heckman (1991), who suggest that civil rights legislation (including the VRA) produced relatively immediate benefits for black Americans. This observation may also tell us about mechanisms through which political power may (or may not) matter. For example, if improvements in either the quantity or quality of schools for black American children is the primary channel affecting minority labor market performance, one might expect to observe this effect with a significant time lag—only after cohorts affected by better schools entered the labor market would we expect to observe improvements in labor market performance. That the observed effect appears relatively soon after VRA coverage suggests that our results are not due to investments by government in human capital-building institutions, such as schools (or other changes to labor supply observed in future labor market participants).

To the extent one is concerned that the increase in black relative wages is part of an overall *decline* in wages within VRA counties, Figure 6 should eliminate this concern (for regression tables, see Appendix Table 5). The effects are driven primarily by an *increase* in black wages. For American workers, VRA coverage led to 4.6% increase in wages (p<0.05), which is statistically significantly larger than the more modest negative effect on whites (p<0.03).

In Appendix Table B3, we estimate several other modifications to the core specification that demonstrate the robustness of the core finding. First, to address the possibility that returns to education differed dramatically between southern states, as suggested by Card and Krueger (1992), we allow for different different returns to human capital (education and experience) by state, as well as by race. In the same vein, we also allow the returns to human capital to vary by both race and geography. We also estimate the specification applying a different functional form of the control variables. Finally, we estimate our 1 controlling for county-by-race fixed effects. Across all of these specifications, our results indicate that the VRA caused a statistically significant increase in the relative labor market performance of black men.

5.3 Robustness of the Main Finding

5.3.1 Ruling Out Confounders: 1975 VRA Amendments & Within-NC Variation

One threat to our identification strategy is the existence of other institutional/policy changes that vary discretely at county or state borders, and that coincide with the time and geography of VRA coverage. For example, in 1964, Congress passed the Civil Rights Act (CRA), which today remains one of the major laws outlawing discrimination in hiring or pay.[42] Unlike Section 5 of the VRA, which applied to only a targeted set of jurisdictions, the CRA applied nationwide – so our empirical strategy should, in principle, account for nationwide policy shocks. Nevertheless, it is possible that

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[42] Two provisions of the VRA may have pertinent in our case. Title II was the provision that outlawed discrimination within places of public accommodation. Title VII of the CRA was the provision outlawing discrimination.

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the CRA affected black Americans relatively more in covered counties, given that the VRA targeted the "worst-of-the-worst" in terms of existing discrimination.

We alleviate this concern by analyzing different subsamples of the data according to the timing of VRA coverage. Amendments to the VRA in 1975 extended Section 5 coverage protections to 283 additional counties. These counties were primarily in Texas and Arizona, but also extended to counties in New Mexico and Oklahoma, among other states.[43] We use the VRA Amendments to split the sample into DEC respondents within 1965 and 1975 VRA-affected counties (and the respective neighbor counties).[44].

In Table 7, we estimate our preferred specification separately for the 1965 and 1975 coverage rounds. Column (1) provides our benchmark estimate from 6. Columns (2) and (3) present estimates for the effect of VRA coverage limiting to subsamples affected by VRA coverage in 1965 and 1975, respectively. The evidence from both panels suggest that the impact of the VRA on black relative wages is likely not a heterogeneous effect of Title VII, nor is the effect purely an artifact of different trends in the outcome of interest. The measured effect of the VRA on black relative wages for the 1965-covered counties is 7 p.p. ($p<0.05$), and for the 1965-covered counties is 4.5 p.p. ($p<0.1$) for the 1975-covered counties.

Finally, we probe the robustness of our main finding by limiting analysis to the subsample of DEC respondents who resided in one state in which there was substantial VRA coverage heterogeneity: North Carolina. To this point, much of our main sample consists of workers within matched county-pairs that span state boundaries. As such, state-level policy changes in the 1960s or 1970s that affected the relative employment outcomes of black Americans would confound our estimates of the VRA's effect. To allay concerns about unobserved institutional changes, we leverage the uniquely even variation in VRA coverage that existed within one particular state. North Carolina is the only state nationwide for which VRA coverage was roughly even — 41 of its 100 counties were covered.[45]

In what we consider perhaps our most stringent test, we focus on North Carolina as a single-state case study of the impact of the VRA on black Americans' labor market performance.[46] With all county-pairs within one state, all DEC respondents in the sample will have been subject to all of the same state-level laws and regulations governing both politics and the labor market. Column (4) of Table 7 presents the VRA estimate for this subsample. The estimates for the within-North Carolina subsample are similar to the overall results, and in fact, are even larger in magnitude.

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[43] The expansion covered three of the four Census regions. Because we examine black-white economic inequality in this study, we focus on VRA coverage in the South and Southwest. This focus means that we exclude from analysis Alaska and South Dakota, where the VRA was targeting voting discrimination toward Native Americans (these states had very small black populations). Arizona and Texas alone account for over 50% of the additional coverage.

[44] The set of states that *border* states with covered counties beginning in 1975 are Oklahoma, New Mexico, Nevada, and Utah.

[45] Covered NC counties were selected for protection based on their 1964 voter turnout rates. Those with turnout below 50% were covered by Section 5, while those with turnout above were not. We view this threshold as unlikely to have been chosen with the coverage of any particular county in mind.

[46] An excellent recent study by Fresh (2017) similar examines the turnout impact of the VRA within just NC. This study documents turnout effects that are similar to those we observe in our full border county sample.

We observe that the VRA increased black relative wages by around 11 p.p. within North Carolina (p<0.01). In conjunction with the tests using the VRA Amendments, these results provide strong corroboration that the effects were not the effect of state policy changes.

5.3.2 Ruling Out Other Confounders: Compositional Changes & Cross-border Spillovers

We further document the robustness of our main results using several other tests and specifications. We highlight some of the main results here, and relegate additional analyses to Appendix B.

One important concern for our study is that voting rights protection may change the composition of the population in covered counties, leading to changes in measured labor market performance. Similarly, to the extent that the ability to participate in local politics is a locational amenity, black families may have moved differentially into counties with protected voting rights (in turn changing the composition of public goods and targeted redistribution from which black households would benefit). Indeed, the *out*-migration of black Americans followed political *disenfranchisement* during the era of Jim Crow, as documented by Naidu (2012) and Margo (1980).[47]

If changes in wages reflect that higher socio-economic status families are migrating to protected counties (e.g., Banzhaf and Walsh (2008)), then we may observe changes in the underlying population characteristics of covered counties post-VRA. Changes such as these would imply that positive earnings impacts may be in part driven by changes in the types of individuals working in covered counties, rather than direct action of government due to improved political influence.

Table 8 presents results from our test of whether the VRA led to a compositional shift in the underlying population characteristics of VRA-covered counties, either due to migration or some other channel (Isen, Rossin-Slatar, and Walker 2015). Each column presents an estimate from a regression relating an indicator for county VRA status to a different dependent variable. We test for the VRA's effect on average levels of the following characteristics between 1960 and 1980: (1) education, (2) years of work experience, (3) black fraction of population, and (4) a summary earnings index that uses the predicted values from a standard Mincerian regression. The effects of the VRA on various measures of county composition are small in magnitude and statistically insignificant, suggesting that the VRA's effect on wage equality is not the product of compositional changes.

A related concern with our empirical strategy is the possibility the VRA created cross-border spillover effects, which would bias our estimate — plausibly in either a positive or negative direction. If labor markets within a given pair of counties are linked across the border, an increase in wages and (and any employment effects) in VRA counties might affect wages in non-VRA counties (either attenuating or amplifying effect of VRA). In a perfectly competitive labor market, a positive impact

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[47] The direction of any migration-induced effect of the VRA is theoretically ambiguous, in our view. To the extent that in-migrants were substitutable with native black workers, the increase in supply would *dampen* our estimated effect of the VRA. On the contrary, to the extent there was positive selection into migration (Boustan 2009), we may overestimate the effect of the VRA because we would no longer be comparing similar black workers pre and post-VRA. Another possible migration response could have been "white flight" away from VRA-affected places (Boustan 2010). Positive *outmigration* may exaggerate the magnitude of our finding.

on black workers' wages may lead to unemployment in VRA-covered counties (due to increasing costs for marginal employers). To the extent that these workers travel to neighboring (uncovered) counties for work, the resulting increase in local labor supply may reduce black workers' earnings in these counties. Wage and employment effects would then be larger at the border—amplifying the causal estimate of the VRA's impact. On the other hand, the possibility of higher wages in treated counties across the border may pressure employers in un-treated counties to partly match the earnings increase. In this case, our causal estimate would be attenuated.

To test for possible cross-border spillovers, we implement a test for bias proposed by Dube, Lester, and Reich (2010). We test whether the effects of the VRA are similar across state borders as well as for counties interior to state borders, using the following spatial-differenced specification:

$$\log(Y_{ict}) - \log(\overline{Y_{ct}}) = \alpha + \beta(\text{VRA}_{ct} \times \text{Black}_{ict}) + (\mathbf{x}_{ict} - \overline{\mathbf{x}_{ct}})\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{r(i)t} + \epsilon_{icr(i)t} \quad (4)$$

In essence, β in tells us whether effects for border and interior counties are statistically different. Estimates based on Equation 4 are presented in Table 9. The coefficient of interest, VRA × Black, is presented in Column (1) and is small in magnitude (roughly 1 p.p., statistically significant at 5% level). This suggests that to the extent there is amplification of our primary estimate of interest, it is relatively small. For reference, columns (2) and (3) provide estimates for the VRA wage gap effect, which is β from an analogue of Equation 1 without pair fixed effects; a casual glance suggests that the effects are similar for both the interior and border county samples. Tests for robustness of these results are presented in Table B4.

Finally, in Appendix Table B5, we also test explicitly whether there is differential migration within our sample (across the matched-pair sample) directly using Census data on a person's place of residence five years earlier. The regression is similar to estimating Equation 2, but using as an outcome an indicator variable for whether a person moved from a VRA to non-VRA county (or vice versa). As Column (1) shows, net out-migration is actually declining in VRA counties (meaning the labor supply would be higher in VRA counties – likely biasing any VRA affect toward zero). In Column (2), we estimate the same specification, but include flexible controls for education and experience, in case migration is positively selected; the results are unchanged. In Columns (3) and (4), we examine whether there are heterogeneous effects by race, and find that if anything, there are *more* workers moving from uncovered to covered counties. Given that such movement is not driven by positive selection (Column 4), we believe it most likely that immigration of black workers would lead us to underestimate our effects.

6 Mechanisms: Political Power and Black Public Sector Employment

We interpret the estimates in Section 5 as the reduced-form effect of black Americans' increased influence and representation on their economic status – reducing wage inequality by nearly 6 p.p. In this section, we turn to an analysis of mechanisms through which minority political power operates.

As discussed in Section 3, there are myriad ways in which minority-responsive government actors may contribute to improvements in black Americans' labor market status—directly and indirectly. To summarize our results briefly, we first focus on a direct mechanism that has been given relatively less attention within economics and is consistent with evidence in empirical political economy: the hiring of black workers within government. As we will show, the VRA is positively associated with public sector employment of minorities. Public sector demand contributed to improvements in black Americans' labor market outcomes by offering those employees better-paying jobs, and by extension, improving the outside-option wage for *other* black workers–both in the public and private sectors.[48]

By no means, however, is this the only way that minority political power may have contributed to economic equality. For example, a large body of scholarship documents the importance of rising demand for black labor as labor market discrimination abated. Most research in this vein focuses on the important roles of anti-discrimination policy and minority-preferential affirmative action programs (Donohue and Heckman 1991; Chay 1998). On the supply-side, Smith and Welch (1989) as well as Card and Krueger (1992) demonstrate the role of improvements in the quantity and quality of black schooling in the South. To be sure, the VRA likely had complementary relationships with some of these mechanisms (Cascio and Washington 2014; Donohue and Heckman 1991). As such, we will consider these channels carefully in Section 7.

6.1 The Importance of the Public Sector

Numerous studies (primarily within sociology, political science, and history) have documented how government agencies have historically serve as equalizing institutions by offering high-quality job opportunities for minority workers (Hout 1984; Laird 2017). The public sector is thought to have been especially influential during the 1960s and 1970s as a gateway to the middle class for black Americans (Katz, Stern, and Fader 2005). To give readers a rough sense of magnitudes: during the 1960s and 1970s, the proportion of black manager-level workers within government increased roughly sixty-seven percent, compared to an increase of only fifteen percent in the proportion of white managers (Collins 1983).[49]

To illustrate the value of public sector jobs to black Americans in the South, we first estimate the public sector premium within our sample – separately for black and white workers. The results are presented in Table 10, based on estimating a simple OLS regression of log wages on a public sector indicator variable, controlling for education, experience, and geography. These estimations demonstrate that black Americans working in the public sector were paid *substantially* more. Black government workers in the South earned 20% more than their private sector counterparts (p-value

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[48] In Appendix C, we formalize the process through which a positive change in minority political power leads to a redistribution of jobs within the bureaucracy toward minority constituents, as well how this change in public sector labor demand affects overall black wages, including within the private sector.

[49] This labor market advancement was due to both state intervention that increased recruitment of black workers as well as more opportunities in higher-ranking professional and managerial positions than existed for minorities in the private sector.

< 0.01). The premium nearly disappears, however, for white workers. The benefits to working in government for black Americans should not be surprising. Discrimination was less acceptable in government and pay schedules were more likely to be heavily regulated. Historical accounts also suggest that the public sector provided minorities who were fortunate enough to be hired access to clerical and semi-professional jobs (Frazier 1957).

The passage of the VRA coincided with the rapid expansion of government nationwide. State and local government employment in particular expanded rapidly during the second half of the twentieth century within both VRA and non-VRA jurisdictions (see Figure 8). This expansion in state capacity opened up new job vacancies for minorities without the need to displace current white workers. Previous work shows that racially integrating the public sector was easier during periods of government growth, rather than during periods of contraction (Krislov 1967).

Given the relative economic benefits of working in the public rather than private sector, it is sensible to suspect that one form of direct state responsiveness to black economic disadvantage was through public sector hiring. Qualitative and quantitative historical evidence suggests that political pressure under the VRA led public sector agencies in federal, state, and local government to become employers committed to equal employment opportunity. Maynard Jackson (Atlanta's first black mayor), for example, came to power in the early 1970s on the promise of hiring and promoting minority workers to positions of importance within local government (as well as with the promise of government contracts based on minority hiring).[50] State agencies became enforcers of non-discrimination in the public sector – unsurprisingly, given the ability of voters to punish discriminatory government agencies (King 2012).

6.2 Impact of the VRA on Public Sector Hiring

To examine whether minority political power achieved under the VRA increased the number of black Americans employed by government, we use the DEC "Class of Worker" variable. This variable categorizes people according to the type of ownership of the employing organization, and thus identifies workers who are employed by government.[51] To analyze whether the VRA increased the relative likelihood of a black worker being employed within the public sector, we slightly modify our primary specification and estimate the following linear probability model:

$$\mathbb{I}(\text{Public Employee} = 1)_{ict} = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icr(i)p(c)t} \quad (5)$$

The dependent variable is an indicator for whether an individual is employed in the public sector, and the coefficient of interest is the same as for Equation 1 above. Results are presented in

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[50] However, the value of public sector employment to black Americans in the Jim Crow South extends back even further than this. In fact, one of Martin Luther King's central policy goals during the Alabama-based Birmingham Campaign of 1963 was to pressure local governments to hire black Americans into local government jobs (Jackson, 2007).

[51] Unfortunately, we cannot distinguish between federal, state, and local public workers prior to 1970. We thus group together all workers employed by a government agency at any level.

Table 11, and suggest that a greater reallocation of public sector jobs from whites to blacks took place in VRA-protected areas. In our preferred specification (Column (1)), we find that the VRA increased the likelihood of a black worker being employed by government by 3.8 p.p. (significant at the 1% level). Columns (2)-(5) demonstrate that this effect is stable to the inclusion of state trends and additional human capital controls, as well as for the cross-state and within-state (NC) samples. Overall, our analysis suggests that the VRA increased the likelihood that blacks would receive a public sector job by between 2 and 4%.[52] We also rule out that black workers' increased public sector presence is due to differential growth of government across VRA and non-VRA counties. Appendix Table B8 uses data on total public-sector labor force (at the county level) from the U.S. Census of Governments, and shows that public sector growth by VRA coverage status does not explain our results. An OLS regression shows only a small, statistically insignificant correlation between VRA coverage and public sector size.

Building political pressure to enforce equal opportunity in the public sector may have also led to better pay in these jobs–either through promotions or reductions in discrimination within jobs. As such, the impact of minority hiring in the public sector may come not only by improving the likelihood of employment but also by improving minority public workers' wages. We test for the impact of political empowerment directly by modifying our wage regression to account for heterogenous effects of the VRA on public and private sector workers.[53]

Results for this test of heterogeneity are presented in Table 12. Summing up the coefficients on VRA × Black is 0.02, suggesting that VRA coverage reduced the wage gap by around two percentage points, less than the private sector wage gap. These estimates are consistent with anecdotal evidence of declining discrimination even *within* the public sector. Only after the Civil Rights revolution began did black Americans working for government agencies begin to receive better pay, as well as other benefits – including a steady pension, long-term job security, and regularly scheduled opportunities for upward mobility to professional and managerial jobs.

6.3 The Impact of Minority Public Sector Hiring on Overall Minority Wages

To this point, we have shown that the VRA (1) improved the labor market status of black workers, (2) improved the likelihood that a black worker will be employed in the public sector which commands a higher wage than working in the private sector, and (3) improved wages within the

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[52] In the Appendix, we provide a number of robustness checks. For instance, Table B7, shows that in absolute terms, black workers were also more likely to be employed in government.

[53] Specifically, we estimate the following specification:

$$\log(W_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \text{Public}_{ict}) +$$
$$\beta_3(\text{VRA}_{ct} \times \text{Public}_{ict}) + \beta_4(\text{VRA}_{ct} \times \text{Public}_{ict}) + \beta_4\text{Public}_{ict} +$$
$$+ \mathbf{X}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icr(i)p(c)t} \tag{6}$$

where $Public_{ict}$ denotes public sector worker status. We are interested in understanding the overall effect of VRA on the public wage gap which we obtain by adding the overall reduction in the wage gap plus the differential effect on public workers, i.e., $\beta_1 + \beta_2$. The sum of these two coefficients tells us how much the black-white wage gap went down (black relative wages increased) for public employees, in VRA-covered counties relative to uncovered counties.

public sector for black workers. We now proceed to establish the connection between improved performance of black workers in the public sector and improved economy-wide wages. We establish this connection in two ways. First, we provide *prima facie* evidence of a relationship between the public sector and the private sector by focusing on occupations that experienced higher and lower rates of national growth from 1960 to 1980. Second, we measure the general equilibrium effects that arise from an increased bargaining position of black workers in the private sector because of improvement in the outside option of working for the government.

6.3.1 Reduced-form Evidence of Public Sector Effects on Overall and Private Sector Wages

We begin by providing reduced-form evidence that the VRA's effect on minority public sector employment did put upward pressure on wages in the private sector (as one might expect when a local economy is hit with a positive sector-specific labor demand shock). To demonstrate this point, we exploit the heterogeneity of workers across occupations, and the fact that certain private-sector workers were more-affected by public sector demand under the VRA. Specifically, we test whether black workers in occupations that experience greater public sector growth (a proxy for labor demand) over our time period also observe differentially greater wage gains. The intuition for this test is that minority voting power will be most beneficial in occupations where there are more vacancies to reallocate public employment to minority workers. These occupations are the ones where private firms face the largest increase in competition for blacks workers. In other words, if public sector job availability and wage increases improve black private sector wages, the impact of the minority political pressure on private wages should be felt most strongly in occupations where the demand for workers is increasing.

We proxy for occupational demand by sorting occupations into quartiles by national public sector employment growth by occupation from 1960 to 1980. We begin by first defining public sector growth by occupation as:

$$\Delta PubSec_{o,-i} = \%PublicSector_{o80,-i} - \%PublicSector_{o60,-i} \tag{7}$$

which denotes the change in relative change in demand for public sector workers within occupation o. We construct these measures at the national level. We then split all occupations within our sample into quartiles, giving us $\Delta \text{PubSec}_{cq,-i}$, which we relabel as $\Delta \text{PubEmp}_{cqi}$ for simplicity. We then use these measures in a modification to our primary specification (Equation 1) in which we examine heterogeneous effects by exposure to increased public sector demand.[54]

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[54] Specifically, we estimate the following specification for heterogeneous treatment effects by quartile of occupation-specific public sector growth:

$$\log(\text{Y}_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \Delta \text{PubEmp}_{cq1})$$
$$+ \beta_3(\text{VRA}_{ct} \times \text{Black}_{ict} \times \Delta \text{PubEmp}_{cq4}) + \beta_4(\text{VRA}_{ct} \times \Delta \text{PubEmp}_{cq1}) + \beta_5(\text{VRA}_{ct} \times \Delta \text{PubEmp}_{cq4})$$
$$+ \beta_6(\text{Black}_{ict} \times \Delta \text{PubEmp}_{cq1}) + \beta_7(\text{Black}_{ct} \times \Delta \text{PubEmp}_{cq4})$$
$$+ \beta_8 \Delta \text{PubEmp}_{cq1} + \beta_9 \Delta \text{PubEmp}_{cq4} + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \tag{8}$$

If the public sector is a channel that contributes to racial wage convergence for black workers, we predict observing the greatest convergence in the top quartile of public sector growth. As Table 13 shows, that is indeed the case. β_3 (the coefficient on $VRA_{ct} \times Black_{ict} \times \Delta PubEmp_{cq4}$) is positive, indicating that black workers in the private sector experience the greater wage gains in jobs that face the most competition from public sector agencies due to increasing demand. While we do not interpret that magnitude of coefficient, it is significant at the 5% level, and we interpret the sign to suggest substantial spillovers from public sector hiring gains to the private sector in those occupations that are most heavily affected. For robustness, we repeat our estimation of Equation 8, but interacting VRA × Black with a continuous measure of sector sector growth by occupation, rather than using quantiles. The effects are similar.

6.3.2 Decomposing the VRA's Public Sector Effect on Private Sector Wage Improvement

This subsection thus far highlights how public sector employment both directly and indirectly effects on overall black wages. We conclude this subsection by quantifying the extent to which the VRA's effect on the overall wage gap is explained by a public sector channel. In other words, what is the contribution of greater labor demand and better compensation for minority workers in the public sector on the private sector wage gap reduction? As we previewed in Section 3, the impact of the VRA on labor market outcomes will consist of more than the mechanical effect of having a higher-paying government job. As work by Beaudry, Green, and Sand (2012) shows, accounting just for these direct estimates may underestimate the true effect of changes in the public sector due to the VRA – due to the existence of spillover (GE) effects to the private sector. To account for these effects in more formal detail, we consider a labor market model with public employment à la Mortensen-Pissarides, which we describe in some detail in Appendix C.

Using this framework, we estimate the public sector channel in two steps: (i) we estimate the component of the private sector wage that in equilibrium arises from changes in public sector hiring practices; and (ii) we estimate the effect on wages that is due to the VRA, just like we have done in previous sections. For our first step, the estimating equation we use is given by Equation (9) which is a rewritten version of Equation (15), for blacks and whites independently, after approximating the weights in the government-driven component of the wage using second-order Taylor expansions:

$$\log(Y_{ict})^{Private} = \alpha_0 + \alpha_1 Black_{ict} + \alpha_2 \overline{PubEmp}_{ct,black} + \alpha_3 \overline{PubEmp}_{ct,white} \tag{9}$$

The identifying assumption underlying this test for heterogeneous effects by public sector growth is that factors contributing to the decrease in the wage gap in VRA counties at the border are orthogonal to growing public sector demand for certain occupations. This is, there is no factor that simultaneously: (i) differentially affects blacks relative to whites, and (ii) differentially affects VRA counties at the border, and (iii) has differential effects over time similar to the VRA, (iv) and affects occupations with high national public-sector demand growth, and (iv) operates at a scale large enough to exert pressure globally. This means, for example, that the increased national public sector demand for clerical workers was not intertwined with decreases in the public sector wage gap in VRA counties at the border following the passage of the regulation through other channels different from the joint effects of public sector changes in occupational demand and the VRA. To the extent such factors might exist, we provide robustness estimates using different controls and fixed effects, with no significant changes in our estimates.

$$+\alpha_4\overline{PubEmp}^2_{ct,black} + \alpha_5\overline{PubEmp}^2_{ct,white} + \alpha_6\overline{PubEmp}_{ct,black} \times \overline{\log(W_{ct,black})}^{Public}$$

$$+\alpha_7\overline{PubEmp}_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_8\overline{PubEmp}^2_{ct,black} \times \overline{\log(W_{ct,black})}^{Public}$$

$$+\alpha_9\overline{PubEmp}^2_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_{cont}X_{ict} + \epsilon_{i,c,p(c),t}$$

where $\overline{PubEmp}$ is the proportion of public employees and $\overline{\log(W)}^{Public}$ is the average public sector wage net of Mincerian traits, both per county, year, and race. We will refer to our fitted values $\widehat{\log(W_{i,c,t})}^{Private}$ as the general equilibrium component of private sector wages. In our second step, we estimate the causal effect of the VRA on the general equilibrium component of private sector wages:

$$\widehat{\log(W_{i,c,t})}^{Private} = \beta_0' + \beta_1'(VRA_{ct} \times Black_{ict}) + \boldsymbol{\gamma'}\mathbf{X}_{ict} + (\delta_c' \times \delta_t') + (\delta_r' \times \delta_c') + (\delta_{p(c)}' \times \delta_r' \times \delta_t') + \epsilon_{i,c,p(c),t}'$$

The contribution of a change in public sector labor practices on the *private* wage gap is given by the following variance decomposition:

$$\frac{Var\big(\beta_1'(VRA_{ct} \times Black_{ict})\big)}{Var\big(\beta_1(VRA_{ct} \times Black_{ict})\big)} \tag{10}$$

Our preliminary results suggest that changes in public sector hiring practices explain between 29 and 35% of the reduction in the wage gap following the VRA.

7 Testing Other Labor Market Mechanisms

In Section 3, we suggested that minority political influence may have contributed to labor market equality through other effects on the private labor market. For example, there were also potential direct ways that political power at the local, state, and federal levels may have improved the labor market outcomes of black Americans. At the federal level, existing research suggests that the effectiveness of Title VII in eliminating racial disparities in wages and employment was a "politically mediated" process (Stainback, Robinson, and Tomaskovic-Devey 2005). We provide suggestive evidence either in support or against some of these mechanisms here.

7.1 Using Heterogeneity to Test Anti-Discrimination and Affirmative Action Regulations

We begin this section by providing suggestive evidence that minority political power was complementary to federal anti-discrimination and affirmative action laws, presumably due to improved likelihood of enforcement. As described above, Title VII of the CRA outlawed employer discrimination in pay, hiring, and promotions on the basis of race. The agency responsible for ensuring workplace equality is the Equal Employment Opportunity Commission (EEOC), which by the mid-1960s received the authority to investigate and negotiate complaints of discrimination.

Unfortunately, there exists little granular and systematic data on either affirmative action policies or Title VII enforcement.[55] We thus conduct a series of indirect tests in this section to suggest that government sympathy for the interests of minorities in equal employment opportunity may have been enhanced by minority political power. Our tests for heterogeneous effects are in the spirit of Smith and Welch (1977), who argue that the "implied threat" of government anti-discrimination activity contributed to reduced discrimination within private firms. Moreover, historical accounts of the 1960s-era South provide good reasons to believe that the force of the federal anti-discrimination effort was augmented by local political actors. From 1966 through the early 1970s (when, as we've shown, significant improvements in black wages took place), the EEOC investigated nearly 80,000 complaints of employment discrimination, filed largely in the South by grassroots political activist groups such as the National Association for the Advancement of Colored People (NAACP).[56]

To test for the existence of a private-sector enforcement channel of the VRA, we construct a county-level measure of exposure of the local workforce to Title VII of the CRA, using geographical data on the fraction of a county's manufacturing workforce employed in small (less than 20 employees) vs. non-small establishments. Research by Carrington, McCue, and Pierce (2000) shows that anti-discrimination and affirmative action policies were less-enforced for small employers, using county-by-industry-by-establishment size variation. We adopt a variant of this approach, modifying it slightly to account for correlated unobservable factors (i.e., factors that are correlated with the presence of large establishments). Specifically, we exploit the change in the establishment size threshold for Title VII coverage from 25 to 15 employees. We use data on establishments both above and under 20 employees drawn from the 1962 County Business Patterns to estimate the probability of workforce exposure to CRA enforcement within a given county (more details about our construction of county civil rights law exposure will soon be provided in an Appendix).

Table 14 presents the results of a test for heterogeneous effects by county-level exposure to federal anti-discrimination laws. The hypothesis we are testing with this regression is whether the change in minority political power (as measured by VRA coverage) augmented the effectiveness of Title VII. If not, we would expect to see no meaningful result for the interaction between VRA × Black and Title VII Exposure (the final row of the table). This is not the case, though. Both with and without baseline controls (Columns (1) and (2)), we find that the effects of the VRA on relative black wages are larger for manufacturing workers in counties that are more likely to affected by the CRA and federal affirmative action requirements, consistent with the findings of Carrington, McCue, and Pierce (2000). Both estimates are significant at the 5% level. We do not interpret the magnitude of the estimates, as it is only meant to be a proxy for the presence

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[55] Although we were preliminarily granted access to the EEOC's establishment-level data that would have allowed us to investigate in more detail the possibility of legal enforcement within the private sector as a mechanism, the Commission's external researcher program was temporarily halted in early 2018 due to concerns about data protection.

[56] We do not take a strong stand on the precise way through which black voting rights and improved political representation improved legal enforcement. Rather, we take at face value work in political science and sociology suggesting that even bureaucratic enforcement of the CRA depended on political factors (Wood 1990; Dávila and Bohara 1994). We readily admit, however, that this evidence is weaker than our other tests in terms of internal validity, and so urge readers to interpret the analysis in this subsection as merely suggestive.

of anti-discrimination law in a county. However, the results do suggest that the minority electoral power may have contributed to black Americans' improved labor market standing through legal enforcement and breaking down labor market segregation.[57] In Column (3) of Table 14, we conduct the same test for heterogeneous effects of the VRA, but we now also control for a worker's occupation. Interestingly, the magnitude of the interaction coefficient is measurably reduced, and is no longer statistically significant at any conventional level. One way to interpret Column (3) relative to Columns (1) and (2), then, is that occupational upgrading may account for some part of the improved wages of Black Americans. We discuss this possibility in greater depth in the Appendix.[58]

At the state and local levels, minority political pressure also led governments to enact their own affirmative action programs. While many of these programs dealt with hiring goals within public agencies (effects subsumed in our public sector results in Section 6), a more common form of local affirmative action required private employers to take specific steps to increase the employment of women and minorities (Nay and Jones 1989; Santoro and McGuire 1997). In particular, affirmative action in contracting (AAC) policies typically conditioned state contracts on minority workforce requirements. Southern states and counties also enacted other policies that likely had indirect effects on minority economic status. For example, the Georgia legislature in the 1980s gave tax breaks to government contractors who employed black workers and subcontracted with black-owned businesses.[59]

Unfortunately, we do not know if any comprehensive data source on local and state AAC or AAE programs – particularly one that extends back to the immediate post-Civil Rights era. However, existing contemporary research suggests that local-level AAC programs are more likely to be enacted in cities where the government chief executive is an elected mayor rather than an appointed city manager. Researchers believe that this relationship exists because elected mayors are more vulnerable to electoral pressures, and so mayors in cities with sizable minority population shares use AAC policies as a form of redistribution. Using geographic variation in a city's form of government (mayor vs. city manager) as a source of treatment effect heterogeneity, Table 17 will demonstrate that the effects of the VRA on black wages are stronger in exactly the localities where one would expect minority electoral power to influence how politicians govern.[60]

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[57] In ongoing work, we are collecting detailed county-level data on government contracts. It is well-known that government contractors were more likely to hire blacks than were non-contractors, due in part to new federal affirmative action regulations beginning in 1966 (Leonard 1990). Moreover, a large fraction of government contracting relates to military spending. As such, we plan construct an instrument for exposure to contracting (which in turn implies greater exposure to labor market regulations related to minority hiring) using the total value of government contracts within a county in 1960. The intuition for this test is that it provides pre-VRA geographic heterogeneity in the likelihood that firms would be more or less subject to government oversight of minority hiring, which would presumably be enhanced by the effects of minority political power.

[58] See Appendix A for details.

[59] Additionally, local political lobbying led to minority business incentive programs designed to increase city contracting with minority businesses, which in turn created new employment opportunities for both entrepreneurs and employees (Nay and Jones 1989)).

[60] We are still awaiting Census disclosure for these results.

7.2 Labor Supply: Changes in Human Capital Are Not the Primary Channel

We close this subsection of the analysis by analyzing whether improvements in black workers' skills are a main channel through which black political empowerment caused reductions in the black-white wage gap. Recent studies document how, consistent with models of distributive politics, the VRA changed the size and composition of government spending in a manner benefited black constituents (Cascio and Washington 2014; Husted and Kenny 1997). Newly-enfranchised black voters' policy preferences often included spending on local institutions with the potential to improve black workers' human capital, and in turn raises their wages.

Cascio and Washington (2014), for example, show that the VRA led to reallocation of public expenditures in areas that with higher black population shares (this would occur if local, state, or federal politicians target newly enfranchised blacks to maximize vote share). This reallocation of state spending was particularly targeted toward improving schools attended by minority children. Similarly, research also shows that the elimination of political discrimination against black Americans led to improvements in black Americans' health in the post-Jim Crow era (Krieger et al., 2014). These studies suggest that the relative growth in black wages may be attributable also to a rise in the supply of skills offered by an increasingly well-educated, healthier, or otherwise more productive black workforce.

Direct and indirect evidence, however, suggests that improvements in human capital caused by the VRA are not the main channel that explain our main findings. First, the timing of the effects discussed in Section 5.2 above is not consistent with the main channel being improved education for black workers. As Figure 7 shows, the effects of the VRA are apparent within five years of the coverage taking effect. That the observed effect appears so soon after passage of the VRA suggests that our results are not due to differential changes in human capital that are due to solely to improved investments in educational attainment (or other changes to labor supply observed in future labor market participants).

We directly test the plausibility of this channel using the micro data on workers' educational attainment, with our results presented in Table B9. First, we show directly that the VRA has no differential effect on the educational attainment gap. We detect no statistically significant effect on black-white worker education gap (as measured by the highest level of education a worker achieves). Moreover, we reestimate our primary specification while accounting for the VRA's impact on education and experience. We add control variables for the interaction terms between the VRA and education/experience to Equation 1. If the VRA was affecting wages through its effect on human capital, than the interactions of VRA and the Mincerian characteristics should drive down/away our primary effect. We do not observe this to be the case, though: there is virtually no change in the parameter of interest, $VRA \times Black$. Albeit an imperfect test given that we are potentially controlling for an outcome, the results are nevertheless suggestive that the VRA did not affect black wages (at least exclusively) by improving the supply of skills provided by black American workers. Again, if anything, controlling for human capital as a mechanism *strengthens* our main results – raising our preferred estimates by 0.2 to 0.3 p.p. (significant at the 5% level).

Finally, in Columns (7) and (8), we find no discernible effect of the VRA on either the rates of high school or college completion of black workers.

8 How Does Minority Political Power Operate?

The previous discussion of channels focused on the labor market mechanisms (supply and demand sides) that gave rise to improved black labor market status. We have provided relatively little discussion, however, of exactly how political incorporation and improved representation functioned in response to the VRA, such that downstream economic benefits were realized by black communities. Existing research in political economy highlights two distinct political channels. Models of spatial competition suggest that policy choices reflect the preferences of the electorate – and in particular, changes in the "median voter" (Downs 1957; Meltzer and Richard 1981). These models of distributive politics suggest that politicians will target resources to identifiable and politically persuadable interest groups to earn their electoral support. Citizen-candidate models, however, suggest that politicians embody the preferences of their constituents—suggesting the electing minorities could lead to distinct policy outcomes that benefit these communities (Besley and Coate 1997)

8.1 Spatial Competition

Theories of spatial competition and distributive politics suggests that black enfranchisement following the VRA should have (weakly) increased public resources flowing to black communities (Lindbeck and Weibull 1987; Cox and Mccubbins 1986; Dixit and Londregan 1996). To demonstrate that these models of distributive politics may be at work in our setting, as suggested by Cascio and Washington (2014), we estimate a similar specification to this paper. Namely, we exploit heterogeneity in the pre-VRA black population share. To the extent that distributive politics is a political channel through which the VRA improved black wellbeing, we would expect that larger post-VRA shifts in in economic status should occur counties with higher black population shares in treatment states than in a group of comparison states. The intuition is that black counties where 40% of the voting population is black will exert more influence over the distribution of resources, and hence demand more government support in terms of employment, contracts, and other policies, than counties where only 5% of the electorate is black. If black constituents vote cohesively, then local politicians would be under greater electoral pressure in the former case, leading them to be more attentive to their policy needs.

Table 15 provides evidence suggestive of this mechanism: that the $VRA \times Black \times \%PopBlack$ is significant and has the same sign as our primary variable of interest suggests that black workers benefited more economically from voting rights in jurisdictions where there they exerted more pressure as a voting block. Standard political economy models also provide sharper predictions about the relationship between the size of the local black electorate and policy responsiveness that generates benefits minority constituents. Assuming black and white Americans do not crossover in their preferences, black constituents' ability to affect policy should jump discretely at or around 50% (when it becomes a majority within a given city). We thus test for nonlinearities in the

heterogeneous strength of minority voting power by dividing counties into 10 % bins by black population shares. We estimate the following regression for heterogeneous effects of the VRA on black relative wages:

$$\log(Y_{ict}) = \sum_{j=1}^{5} \alpha_j \left[\% \text{ Pop. Black}_c = j\right] \times [\text{VRA}_{ct} \times \text{Black}_{ict}] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t}$$

(11)

where j indicates one of five different bins for county black population share. $j = 1$ indicates a county where 1960 black population share is between 10 and 20%, $j = 2$ indicates a county where 1960 black population share is between 20 and 30%, and so on; $j = 5$ indicates a county where 1960 black population is over 50%.

The results from this estimation are presented in Figure 9. They convey a few suggestive, but nevertheless important, findings about how minority political power serves the interest of minority voters. First, as demonstrated by coefficients α_2, α_2, and β_4, it was not necessary that blacks comprise a majority of the electorate for the VRA to improve socioeconomic conditions. However, the magnitude of the coefficient on α_5 suggests that when black constituents comprise a majority of the electorate, the economic benefits are substantially larger. Although merely suggestive (due to potential correlated unobservables), this finding is consistent with the the Median Voter hypothesis. We find qualitatively similar findings for public sector employment (i.e., the effects are considerably stronger in counties where black voters constitute over 50% of the local population). To the extent these results are suggestive of political forces, they also suggest that *local* government matters.

8.2   Descriptive Representation

Theoretical and empirical research suggest identity politics as one reason that the VRA may have translated into material benefits for black communities. Under a model of identity politics, the VRA might have enabled black voters to elect black representatives, who would in turn redistribute benefits such as government jobs and government contracts, as well as enact polices such as AAC policies, due to a common ideology. For example, in the Indian context, Pande (2003) as well as Chattopadhyay and Duflo (2004), demonstrate empirically that representatives' personal ideology, proxied by gender and ethnicity, affect the distribution of public goods in a manner that benefits historically disadvantaged voters. Anecdotal evidence lends some credence to this possibility. After 1965, black Americans ascended to political office nationwide at a pace never before seen. The number of black elected officials in local, state, and federal government rose more than six-fold from 1970 to 2000 (JCPES, 2000).

However, there is very limited *causal* evidence on whether the increase in black office-holding was due to the passage of the VRA, and perhaps more importantly, whether any increases in descriptive representation lead to substantive improvements in minority economic wellbeing. We test the plausibility of this channel by first demonstrating that the VRA increased the presence of black officials in elected office differentially in covered areas. To do this, we digitized data from

various volumes published by the Joint Center for Political and Economic Studies. The JCPES published its annual "Black Elected Officials: A National Roster" each year beginning in 1969.[61] Because our constructed data begin only in 1969 (after the VRA was passed), we also supplemented our data construction using data from Alt (1984), which contains the number of black elected officials in the South in 1960.[62] Although we still cannot establish a pre-trend in minority elected officials prior to 1960, our qualitative search suggested that prior to the Civil Right era, there was virtually no black representation in the South at any level of government (JCPES, 2000).[63]

In Table 16, we provide results from estimating the impact of VRA coverage on the presence of black elected officials within a county. The results clearly indicate an increase in the number of minority elected officials, as might be expected.

9   Conclusion

Understanding the politics-economics nexus is important for understanding the effect of the VRA, as political representation is intimately related to distributional issues. Moreover, the VRA and black economic progress are intertwined historically, since racial minorities' demand for equal economic opportunity was a central feature of the Civil Rights social movement that led to passage of signature laws such as the VRA and the Civil Rights Act. As such, a complete understanding of the effects of the fVRA requires understanding the accuracy of economic historian Gavin Wright's claim that, "black political power has played an important role in improving racial economic equity" (Wright 2013). If the policy demands of now-enfranchised voters include policies that improve their economic lives (i.e., desegregated labor markets, elimination of workplace discrimination, improved schools, etc.), one might expect to observe improved economic outcomes in the short-term following this large-scale enfranchisement event.

In this study, we confirm that this hypothesis is indeed true. We show that minority political empowerment has important labor market benefits for previously disenfranchised minorities. Our estimates demonstrate that counties where voting was protected experienced larger reductions in the black-white wage gap. We also thoroughly probe mechanisms, finding strong evidence that the VRA altered labor demand. We document that the VRA increased (relatively) the likelihood of blacks being employed in the public sector, as well potential complementarities between political power and the enforcement of private sector labor market policy.

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[61] In the process of writing this paper, we were pointed to an excellent new working paper by Bernini, Facchini, and Testa (2018), who examine the impact of the VRA on the composition of elected officials. While the data on minority elected officials that we compiled was from the same source, we use the data for a different (albeit complementary) purpose.

[62] We are tremendously grateful to Jim Alt, who provided us with his data on minority political behavior during the pre-VRA era.

[63] See PBS (2000).

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10 Figures

Figure 2: U.S. Counties by VRA Section 5 Coverage
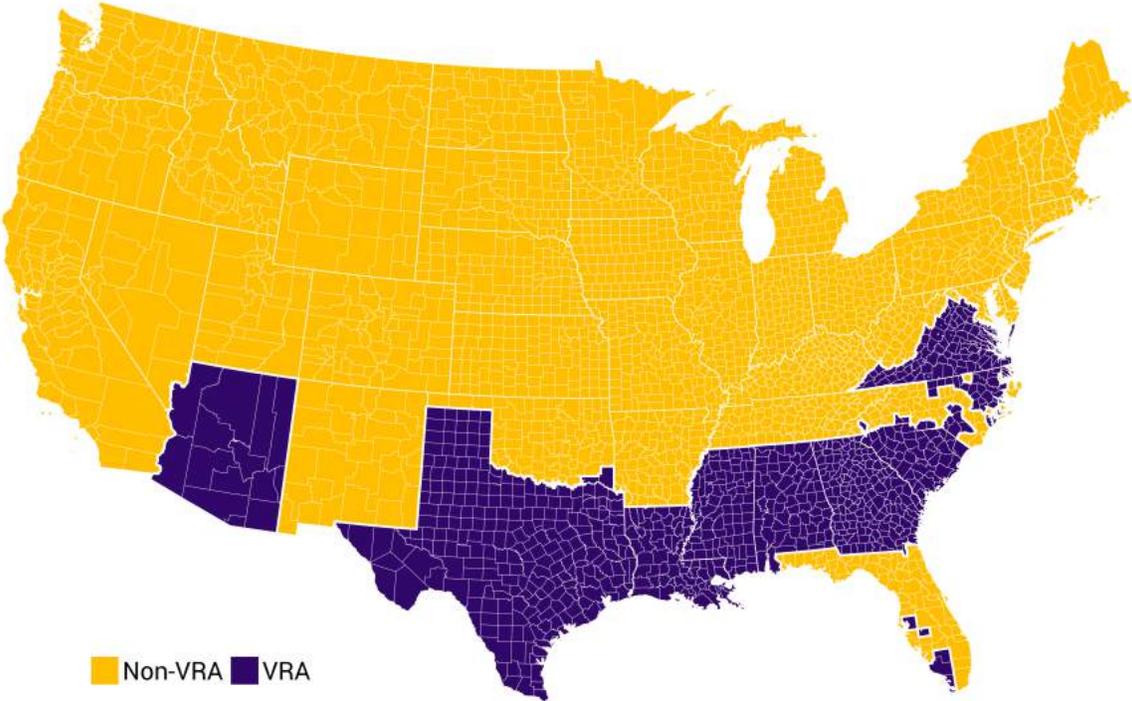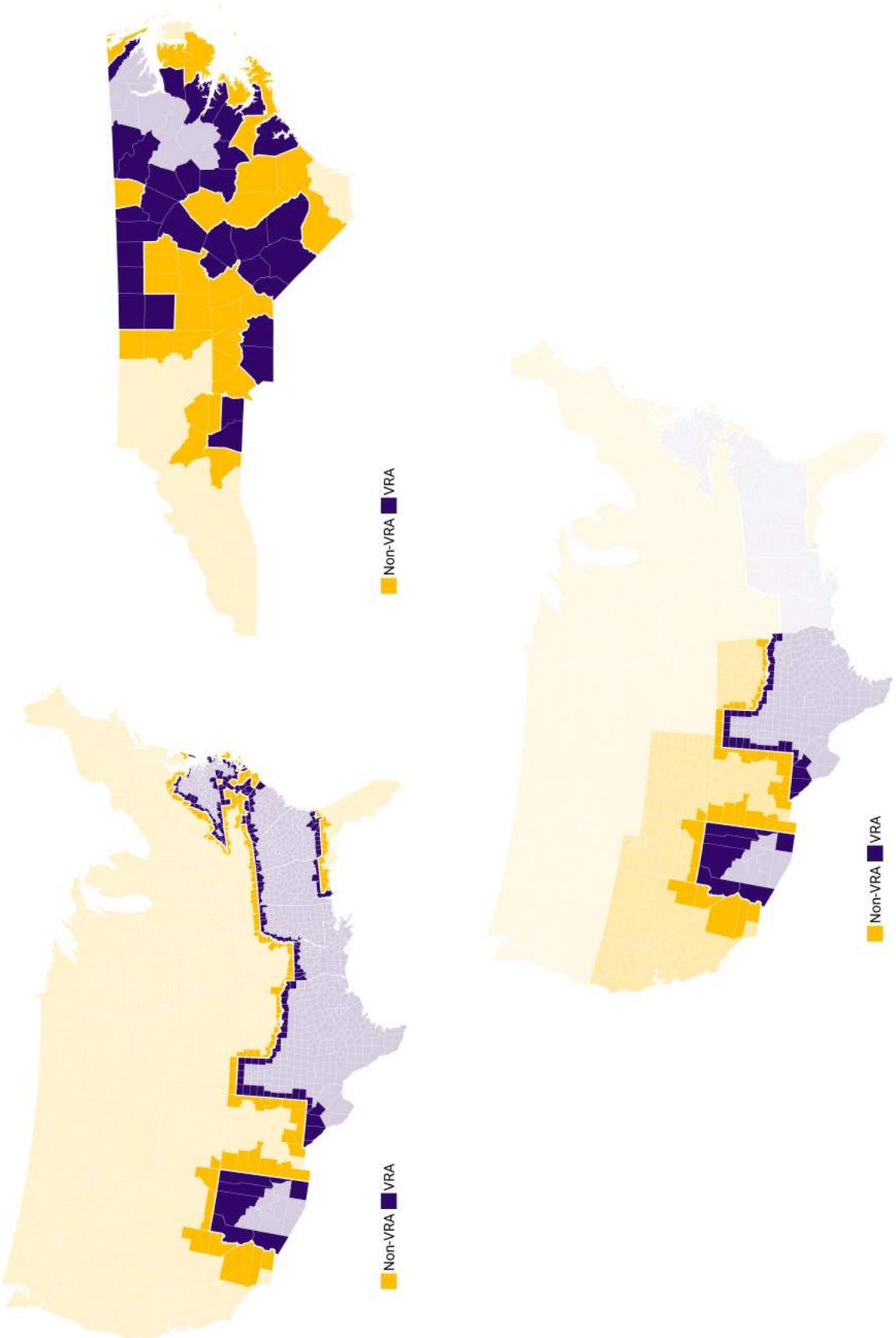
Figure 3: VRA Covered Jurisdictions

Figure 4: Public Sector Employment by Race
(Source: Decennial Censuses)
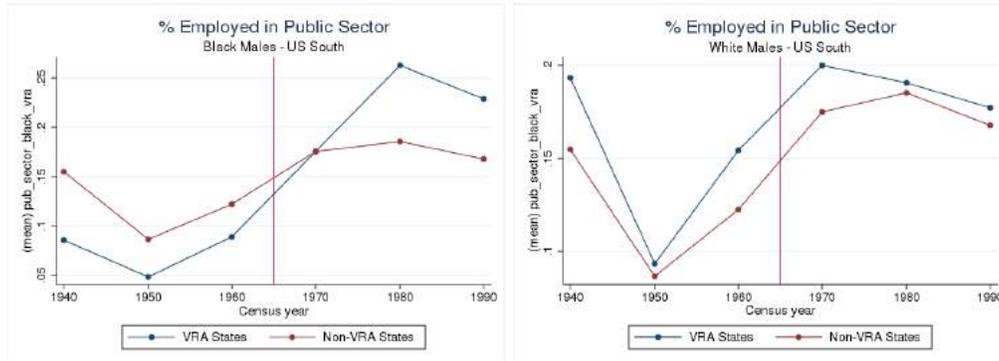
Figure 5: Impact of the VRA on Presidential Turnout
(Heterogeneous Effects by % County Black)
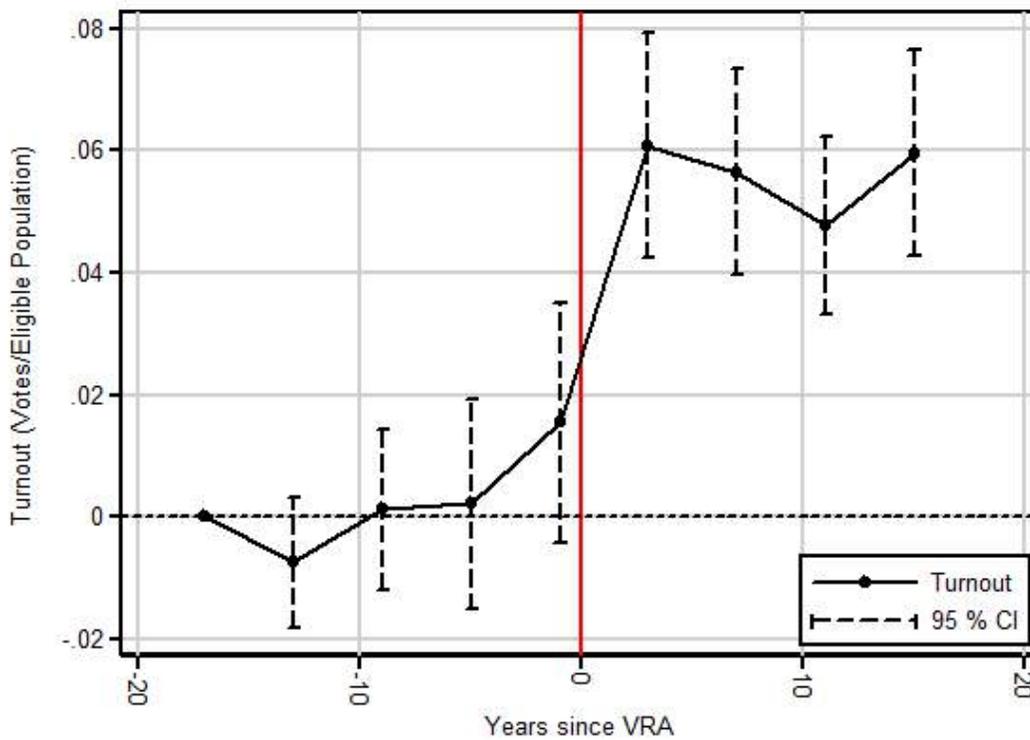
NOTES: Figure presents event-time estimates of how VRA coverage interacted with pre-VRA black population relates to voter turnout. The dependent variable (y-axis) is the voter turnout in presidential elections, and the independent variable (plotted) is the interaction between between the ever-VRA-covered indicator, a year indicator, and the pre-VRA percentage of the population that is black within a county (standardized, mean 0). All specifications include county and year fixed effects, as well as state-specific linear time trends. The model also includes the controls for unemployment, population density, high school graduation rate, and farm population rate, fixed at 1960 levels and interacted with linear and quadratic trends.

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Figure 6: Impact of the VRA on Wages
(by race)
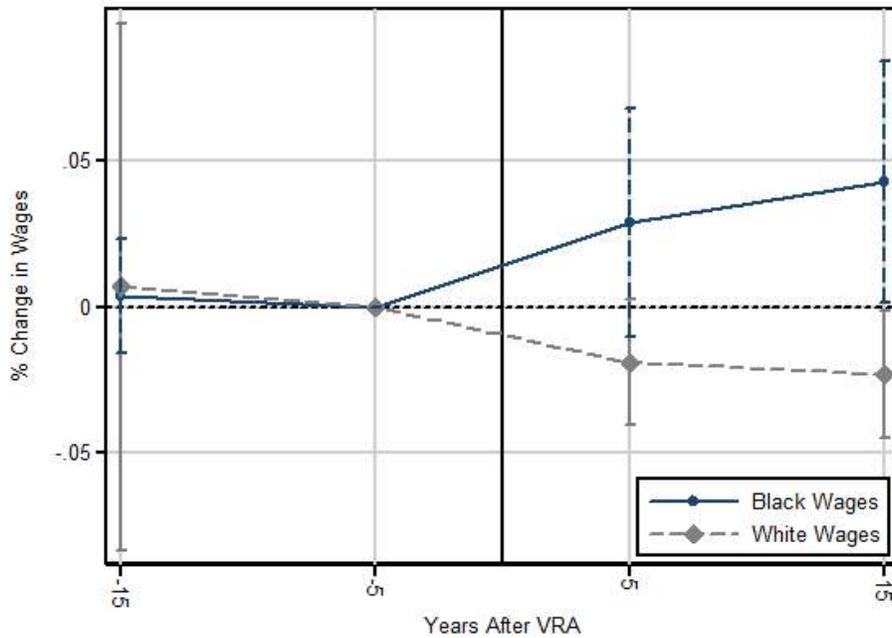
NOTES: Figure presents event-time estimates of how VRA coverage affects wages for black and white workers, separately. Estimates are normalized to five years prior to VRA coverage taking effect. Source: Decennial Census.

Figure 7: Impact of the VRA on the Black-White Wage Gap: Event Study Estimates
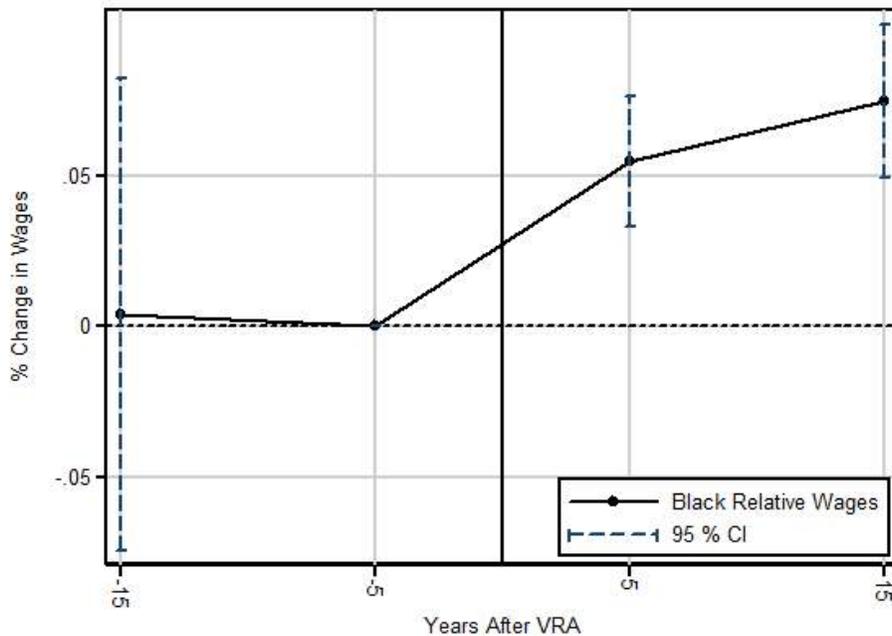
NOTES: Figure presents event-time estimates of how VRA coverage affects black relative wages . Estimates are normalized to five years prior to VRA coverage taking effect. Source: U.S. Decennial Census.

Figure 8: Local Government Growth, 1957-2007
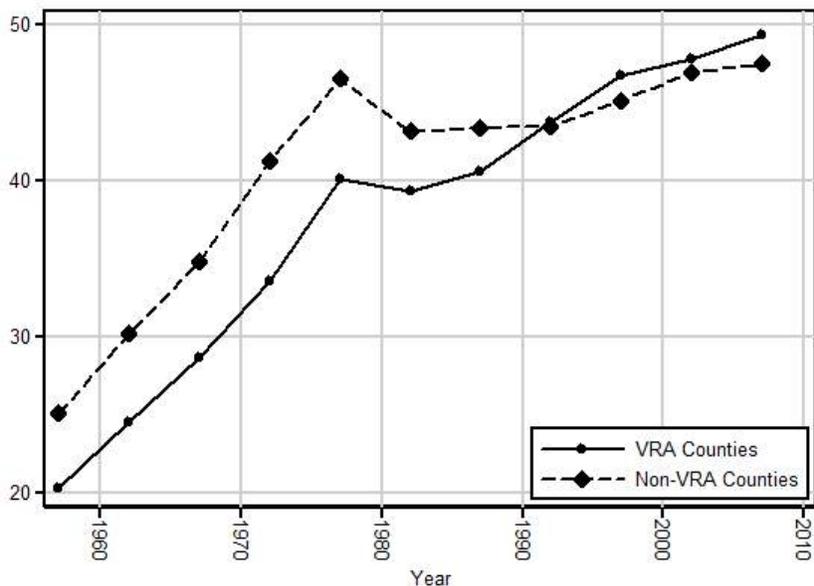
Notes: Figure presents the number of employees per 1000 people in for the sample of neighboring VRA and non-VRA counties. We restrict to counties with populations of larger than 10,000. Source: U.S. Census of Government.

Figure 9: Heterogeneous Effects of the VRA:
Wage Results by County Black Population Share
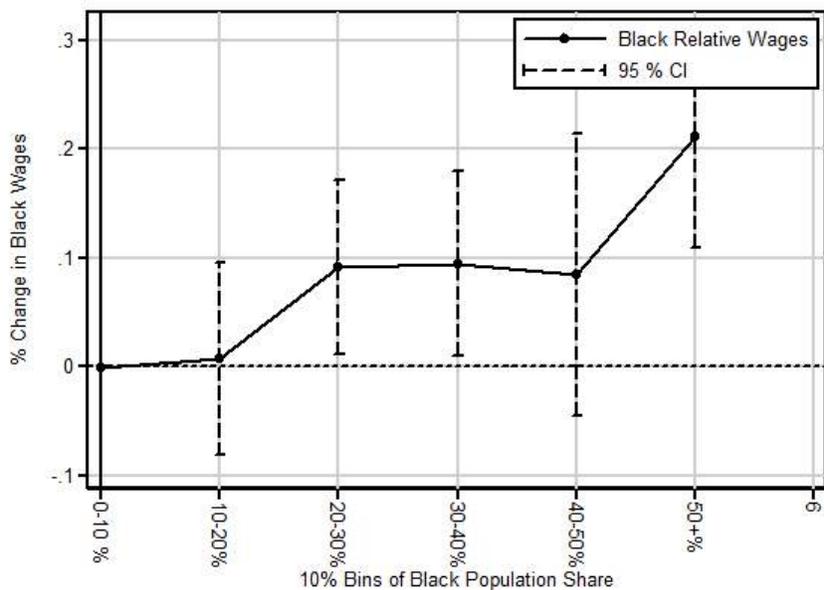
Notes: Figure examines the heterogeneous effects of VRA coverage on black relative wages, by black population share within a county. Each point indicates coefficient of interaction between main explanatory variable (VRA × Black) and a dummy variable for whether a respondent resides in a county with a given level of black population share along the X-axis. Source: U.S. Decennial Census.

11 Tables

Table 1: Summary Statistics - County Characteristics in 1960

	(1)	(2)	(3)	(4)
Variable	**Non-VRA Counties**	**VRA Counties**	**Mean Difference**	**P-value**
Interior Counties				
Median Income	3799.23	3429.09	370.14	0.02
% Pop. Black	.08	0.26	-0.18	0.01
% Ag. Workers	0.230	0.199	0.03	0.19
% FT Employed	0.71	0.68	0.03	0.01
% 25 y.o.-HS Educated	0.31	0.28	0.03	0.00
Rep. Party Voteshare	0.62	0.67	0.05	0.00
Border Counties				
Median Income	3818.27	3649.84	168.34	0.39
% Pop. Black	.18	0.17	-0.01	0.61
% Pop. Urban	.29	0.32	-0.03	0.38
Farm Share	0.21	0.21	0.00	0.82
% FT Employed	0.71	0.68	0.02	0.17
% 25 y.o.-HS Educated	0.29	0.29	0.00	0.97
Rep. Party Voteshare	0.61	0.63	0.02	0.08

NOTES: This table reports average characteristics across both Section 5 and non-Section 5 counties, for both the border county sample as well as the interior county sample.

Table 2: The Effect of the VRA on Political Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	.115***	.079***	.063***	.148***	.087***	.061***	.042***
	(.010)	(.007)	(.007)	(.005)	(.004)	(.012)	(.010)
VRA × Black Pop. Share						.003***	.002***
						(.000)	(.000)
N	2651	2651	2651	12848	12848	2651	2651
Controls		X	X	X	X	X	X
State Trends			X		X		X

Notes: This table presents regression coefficients from 7 separate regressions, one per column. An observation is a county-year. The dependent variable is county-level turnout in presidential elections. The independent variable is a dichotomous variable indicating whether a given county is protected under VRA-Section 5 (and where relevant, the interaction between the VRA indicator and the county population share that is black). Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table 3: Impact of the VRA on Legislative Responsiveness (Ideology)

	(1)	(2)	(3)	(4)	(5)	(6)
VRA	-0.08*	-0.06*	-0.04	0.05*	-.02	.02
	(0.04)	(0.03)	(0.04)	(0.03)	(0.02)	(0.03)
Black Pop. Share		-0.03***	-0.03***		-0.03***	-0.03***
		(0.01)	(0.01)		(0.01)	(0.01)
VRA × Black Pop. Share			-.12			-0.21*
			(0.15)			(0.11)
N	1699	1699	1699	1699	1699	1699
DW-Nom. Dimension	2	2	2	1	1	1
Controls		X	X		X	X

Notes: This table presents regression coefficients from 6 separate regressions, one per column. An observation is a congressional district-year. The dependent variable is one of the two dimensions of the Poole-Rosenthal DW-Nominate Score, and the independent variable is an indicator variable for whether a district is covered under Section 5 of the VRA. All regression include Congress (year) and district (geography) fixed effects. Standard errors are in parentheses and are clustered by district. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table 4: Impact of the VRA on Policy Responsiveness (Spending), 1957 - 1982

	Panel A: Per Cap. Public Assistance				Panel B: Per Cap. State-Local Transfers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	0.01***				-0.05***	-0.05***	.01
	(0.001)				(0.01)	(0.01)	(0.01)
VRA × Black Pop. Share		0.07***	0.07***	0.10***	0.12***	0.11***	0.06***
		(0.02)	(0.02)	(0.02)	(0.02)	(.02)	(0.02)
N	690	690	690	690	2176	2176	2176
County Controls	X		X	X		X	X
State Trends	X			X			X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. An observation is a county-year. The dependent variable in Columns (1)-(4) is the per capita number of public assistance recipients in a given county (measured twice - in 1964 and 1980). The dependent variable in Columns (5)-(7) is the per capita levels of state-to-local intergovernmental transfers a given county receives (measured every five years between 1957 and 1983). The independent variables are an indicator variable for whether a district is covered under the VRA, as well as (where relevant) the interaction between the VRA indicator and the 1960 county population share that is black. Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. Sources: County Data Books, 1944-1977; Census of Governments

Table 5: The Effect of the VRA Wages, by Race, 1950-1980

	(1)	(2)
	Panel A: Black Workers	
VRA	0.050**	0.054**
	(.027)	(.027)
N	115000	115000
	Panel B: White Workers	
VRA	−.014**	−.007*
	(0.006)	(0.005)
N	558000	558000
	Panel C: Black-White Outcome Gap	
VRA × Black	0.055**	0.058***
	(.027)	(.027)
N	67300	67300
County-level Controls		X

Notes: This table presents regression coefficients from 5 separate regressions, one per column. This table reports estimates of ordinary least squares regressions relating the Voting Rights Act to (relative) wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 6: The Effect of the VRA on Black Relative Wages, 1950-1980

	Outcome Variable: Log(Wage)				
	(**1**)	(**2**)	(**3**)	(**4**)	(**5**)
VRA × Black	0.055**	0.058**	0.056**	0.056**	0.048*
	(0.027)	(0.027)	(0.28)	(0.28)	(0.35)
N	673000	673000	673000	673000	673000
County-level Controls		X	X	X	X
State Trends			X		
County Trends				X	
County-by-race Trends					X

Notes: This table presents regression coefficients from 5 separate regressions, one per column. Each column reports estimates of ordinary least squares regressions relating the VRA to (relative) wages. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are county characteristics measured in 1960, interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 7: The Impact of the VRA on the Black-White Wage Gap: Subsample Analysis, 1950–1980

	Outcome Variable: Log(Wage)			
	(**1**)	(**2**)	(**3**)	(**4**))
VRA × Black	0.057**	0.071**	0.046*	0.114***
	(0.027)	(0.035)	(0.032)	(0.038)
Sample	Full Sample	1965 VRA	1975 VRA	NC
Baseline-Year Controls	X	X	X	X
N	673000	530000	150000	180000

Notes: This table presents regression coefficients from 4 separate regressions, one per column – each for a different subsample (with the main sample being in Column (1)). Each column reports estimates from ordinary least squares regressions relating the VRA to (relative) wages. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are county characteristics measured in 1960, interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 8: The Effect of the VRA on County Compositional Changes, 1960-1980

Outcome:	(1) Education	(2) Experience	(3) % Black	(4) Earnings Index
VRA	0.40	0.22	0.01	434.58
	(0.82)	(0.80)	(0.04)	(1464.2)
N	600	600	600	600
County-level Controls	X	X	X	X

Notes: This table reports estimates of OLS regressions relating the VRA to average county charcteristics. The dependent variable in each column is a characteristic in a given year. All regressions include county baseline controls, pair-year, and county fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 9: The Effect of the VRA on Relative Wages: Spillover Effects

	Outcome Variable: Log(Wage)		
	(1)	(2)	(3))
VRA × Black	0.064**	0.045***	0.011**
	(0.030)	(0.018)	(0.005)
Sample	Matched Pairs	Interior	Difference
N	670000	3741000	670000
County-level Controls	X	X	X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the VRA to (relative) black wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. Column (1) limits to the county pair sample, Column (2) limits analysis to the interior (counties in which all adjacent counties are either covered or uncovered), and Column (3) reports the difference. All regressions include county-race, county-year, and year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls are measured in 1960, and interacted with linear and quadratic time trends. Regressions control for race-specific returns to human capital. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 10: Public Sector Wage Premium Estimates (1960)

Outcome Variable: Log(Wage)		
	(**1**)	(**2**)
Public Worker	0.029***	0.196***
	(0.003)	(0.009)
Worker Sample	White	Black

Notes: This table presents regression coefficients from 2 separate regressions, one per column. Each coefficient is an estimate from an OLS regressions of log wages on an indicator that equals 1 if an individual is a government employee. Regressions control for individual education, years worked, and squared(years worked), and state fixed effects. Models are estimated using the 1960 Census, for all workers in counties eventually covered under the VRA. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. Source: IPUMS Decennial Census

Table 11: The Effect of the VRA on Public Sector Employment, 1950-1980

	Outcome Variable: Public Sector Employment				
	(1)	(2)	(3)	(4)	(5)
VRA × Black	0.038***	0.020**	0.038***	0.027**	0.027**
	(0.009)	(0.01)	(0.009)	(0.012)	(0.012)
N	673000	673000	673000	175000	175000
County-level Controls	X	X	X	X	X
State Trends			X		
Sample	Full CB	Full CB	Full CB	NC	NC

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to employment in the public sector. An observation is an individual in a given Decennial Census year. The dependent variable an indicator that equals 1 if an individual is a government employee. The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Columns (2) and (4) include additional human capital controls. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. Models are estimated on either the full cross-state border (CB) sample, or the North Carolina-only (NC) sample. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 12: Heterogeneous Wage Effects of the VRA, by Sector (Public or Private)

| | Outcome Variable: Log(Wage) | | |
	(1)	(2)	(3)
Public	0.02	0.02	0.02
	(0.022)	(0.022)	(0.022)
VRA × Black	0.139***	0.144***	0.149***
	(0.019)	(0.019)	(0.019)
Black × Public	0.052***	0.053***	0.053***
	(0.015)	(0.016)	(0.016)
VRA × Public	0.011	0.011	0.011
	(0.036)	(0.036)	(0.036)
VRA × Black × Public	-0.069**	-0.069**	-0.070**
	(0.027)	(0.027)	(0.027)
County-level Controls		X	X
Race-by-education Controls			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the Voting Rights Act to (relative) black wages. Public is an indicator variable for whether a worker is employed in the public sector. An observation is an individual worker in a given Decennial Census year. The dependent variable is log wage, and the independent variables are interactions for: VRA × Black × Public (the interaction between a worker's race, public sector status, and whether the worker's county of residence was covered by the VRA in a given year), as well as all lower-order interactions. The (adjusted) baseline black-white hourly wage gap (in 1960) was -0.43 log points. All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 13: Wage Effects of the VRA, by Public Sector Occupational Growth

	Outcome Variable: Log(Wage)	
	(1)	(2)
VRA × Black	0.081**	0.114**
	(.07)	(0.57)
VRA × Black × $\Delta\text{PubEmp}_{60-80,Q1}$	-0.078**	-0.101**
	(0.03)	(.046)
VRA × Black × $\Delta\text{PubEmp}_{60-80,Q4}$	0.088**	0.008
	(0.045)	(.043)
N	153000	54500
Worker Sample	Private	Public
County-level Controls	X	X

Notes: This table presents regression coefficients from 2 regressions, one per column. Each coefficient is an estimate from an OLS regression relating the Voting Rights Act to (relative) black wages, examining heterogeneity by public sector occupational growth. An observation is an individual in a given Decennial Census year. The dependent variable is log wage, and the independent variables are interactions between VRA, the race indicator Black, and whether a respondent works in an occuation that is in either the first or fourth quartile for public sector growth. All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table 14: Heterogeneous Effects of the VRA on Relative Black Wages: Testing Complementarity between Political Power and Civil Rights Act

	Outcome Variable: Log(Wage)		
	(1)	(2)	(3)
VRA × Black	−0.077	−0.065	−0.062
	(0.072)	(0.054)	(0.109)
TitleVIIExposure × Black	5.727**	5.302*	4.908**
	(2.34)	(3.17)	(0.2.34)
VRA × Black × TitleVIIExposure	0.361**	0.351**	0.28
	(0.142)	(0.157)	(0.365)
N	10500	10500	10500
Controls		X	X
Occupation Controls			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. An observation is an individual Census respondent in a given Census year. The dependent variable is the log wage, and the independent variable of interest is the the interaction between an indicator for a county's VRA converage status in a specific year (a dummy), an indicator for whether a worker is black, and the county-level exposure of the manufacturing workforce to federal civil rights laws (as defined in Section 7.2 and the Appendix). All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC

Table 15: Heterogeneous Effects of the VRA on Relative Black Wages:
By Black Population Share

Outcome:	Log(Wage) (1)	Pub. Emp. = 1 (2)
VRA × Black	0.036	0.036
	(0.030)	(0.030)
VRA × Black × 1960 Black Pop. Share	0.002***	0.147**
	(0.000)	(0.062)
N	673000	673000
Controls	X	X

Notes: This table presents regression coefficients from 2 separate regressions, one per column. An observation is an individual Census respondent in a given Census year. The dependent variable is the log wage, and the independent variable of interest is the the interaction between an indicator for a county's VRA converage status in a specific year (a dummy), an indicator for whether a worker is black, and the county-level exposure of the manufacturing workforce to federal civil rights laws (as defined in Section 7.2 and the Appendix). All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC

Table 16: The Impact of the VRA on Black Elected Representatives, 1960–1980

	Outcome Variable: Black Elected Officials			
	(1) Log(Count)	**(2)** County-wide =1	**(3)** Mayor=1	**(4)** Mayor=1
VRA	0.148**	0.121**	0.011	0.022***
	(0.068)	(0.044)	(0.017)	(0.007)
Sample	Border	Border	Border	Full
Baseline-Year Controls	Yes	Yes	Yes	Yes
N	810	810	810	3,750

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each coefficient is an estimate from OLS regressions relating the Voting Rights Act to the presence of black elected officials. An observation is a county-year. The independent variable is the VRA indicator (whether a county was covered by the VRA in a given year). All regressions include county and year fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: Joint Center for Economic and Political Studies

Table 17: Effects of the VRA on Relative Wages, by Local Government Structure

Outcome Variable: Log(Wage)		
	(**1**)	(**2**)
Black × VRA × Mayor-Council Govt.	+0.xxx***	
	(0.xxx)	
Black × VRA × At-Large Districts		−0.xxx***
		(0.xxx)
Black × VRA	+0.xxx***	+0.xxx***
	(0.xxx)	(0.xxx)

Notes: **RESULTS TO BE INCLUDED.** This table presents regression coefficients from 2 separate regressions, one per column. An observation is an individual Census respondent in a given Census year. The dependent variable is the log wage, and the independent variable of interest is the the interaction between an indicator for a county's VRA converage status in a specific year (a dummy), an indicator for whether a worker is black, and a dummy variable for whether the county-seat in a given county has a mayor-council executive structure, or an at-large district legislative structure. All regressions control for individual education, years worked, and squared(years worked), and include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC and ICMA

Appendices

A Occupational Upgrading

The impact of minority political empowerment on employment outcomes in both the private and public sector also likely affected the occupational redistribution of workers. Prior research on black economic progress in the North, for example, documents positive effects of government action through fair employment agencies on black workers' occupational upgrading in the 1950s (Collins, 2003; Liggett 1969).

Understanding the VRA's effect on occupational upgrading (distinct from wages and employment) is important for a few reasons. To a large extent, discrimination in labor market opportunities (within both the public and private sectors) involved barriers to entry for certain occupations. For example, most black workers within the public sector in 1960 worked as janitors. Thus, to the extent that the VRA improved black wages, one would reasonably expect this impact is at least partially understood as positive occupational upgrading.

The occupational redistribution and upgrading of black American workers likely reflect the mechanisms we test. Ample research shows that the public sector, for example, provided more opportunities for upward job mobility to managerial positions (Hout 1984). Similarly, through the desegregation of labor markets that Jim Crow politics sustained (Roback, 1986), the VRA helped break down the segregated labor markets through which wage discrimination operated.

However, the VRA may have also indirectly created opportunities for black American workers to move up the economic ladder. The movement of black Americans to the public sector likely created new opportunities for other black workers within the private sector (assuming private labor demand stayed fixed). As we discussed above, the public sector was the entryway for an emergent black middle class. The proportion of black Americans working as managerial and professional workers was *sixty-two* percent greater within the public sector than for white workers. By 1970, 27 percent of black managers and 11 percent of white managers and administrators worked in government (Collins, 1983).

We test for occupational upgrading in a similar spirit to Collins (2003). We compute a similar measure, *OccScore*, as follows: using the median income earned in 1960 by for each three-digit occupational category, we create an ordinal ranking of all occupations in our sample. This ranking we define as our *OccScore* variable. Using this variable, we reestimate Equation 1, with the natural log of the occupational score instead of income.[64]

We can also probe these results more to understand the mechanism of upgrading better. In particular, increased opportunities to reach the professional and managerial ranks within government would most directly affect highly-educated black workers. We confirm that this is indeed the case by showing that the VRA's positive effect on the likelihood of being employed within the public sector is substantially larger for black workers who are college graduates or higher (see Table X). Moreover, as we have just discussed, if government hiring was reducing the supply of college-educated blacks within the private labor force, we might expect more occupational upgrading withing the *private* sector for black workers with less education. This is indeed what we find.

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[64] The estimating equation is thus:

$$\log(OccScore_{ict}) = \beta_0 + \beta_1(VRA_{ct} \times White_{ict}) + \mathbf{x}_{ict}\boldsymbol{\gamma} + (\delta_c \times \delta_t) + (\delta_r \times \delta_c) + (\delta_{p(c)} \times \delta_r \times \delta_t) + \epsilon_{i,c,p(c),t} \quad (12)$$

The results in this subsection help paint a more complete picture of how the VRA (and the civil rights era more broadly) may have contributed to black economic advancement. Although black workers in the South occupied the lower rungs of the economic ladder prior to mid-century, the combination of increased public sector hiring as well as private sector intervention—both facilitated by the VRA—allowed black Americans to achieve success in new occupations and professions.

B   Additional Tables

Table B1: The Effect of the VRA on Political Participation
(Turnout for Congressional Races)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	.120***	.092***	.058***	.153***	.101***	.098***	.065***
	(.010)	(.009)	(.009)	(.005)	(.005)	(.012)	(.018)
VRA × Black Pop. Share						.001***	.001
						(.000)	(.001)
N	2651	2651	2651	12848	12848	2651	2651
Controls		X	X	X	X	X	X
State Trends			X		X		X

Notes: This table presents regression coefficients from 7 separate regressions, one per column. An observation is a county-year. The dependent variable is county-level turnout in congressional elections elections. The independent variable is a dichotomous variable indicating whether a given county is protected under VRA-Section 5 (and where relevant, the interaction between the VRA indicator and the county population share that is black, "Black Pop. Share"). Standard errors are in parentheses and are clustered by county. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table B2: The Effect of the VRA on Black Relative Wages, 1950-1980 - Robustness

	(1)	(2)	(3)
VRA × Black	0.56**	0.057**	0.058**
	(0.27)	(0.28)	(0.27)
N			
County-level Controls	X	X	X
State-by-human capital FE	X		
Race-by-human capital FE		X	
County-by-race-by-human capital FE			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. This table reports estimates of ordinary least squares regressions relating the VRA to (relative) wages. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are county characteristics in 1960 interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table B3: The Effect of the VRA on Black Relative Wages, 1950-1980 - Subsample Analysis

	(1)	(2)	(3)
	Panel A: 1965 Sample		
VRA × Black	0.071**	0.071**	0.073**
	(0.035)	(0.034)	(0.035)
N	524000	524000	524000
	Panel B: 1975 Sample		
VRA × Black	0.043	0.048*	0.047**
	(0.045)	(0.027)	(0.026)
N	149000	149000	149000
	Panel C: North Carolina		
VRA × Black	0.116**	0.158***	0.158***
	(0.054)	(0.048)	(0.048)
N	175000	175000	175000
Controls		X	X
County Trends			X

Notes: This table presents regression coefficients from 9 separate regressions, 3 per panel and 1 per column. Each coefficient is an estimate from OLS regressions relating the Voting Rights Act to (relative) wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). Panel A presents presents estimates using onlythe VRA border county pairs for which the VRA became active in 1965. Panel B presents presents estimates using only the VRA border county pairs for which the VRA became active in 1975. Panel C presents presents estimates using only the VRA border county pairs within North Carolina. All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table B4: Comparing Border and Interior Estimates
(Testing for Cross-border Spillovers) - Robustness

	(1)	(2)	(3)
	Panel A: Border		
VRA × Black	0.0055**	0.064**	.0.064**
	(0.027)	(0.03)	(0.028)
N	670000	670000	670000
	Panel B: Interior		
VRA × Black	0.043**	0.044**	.0.044**
	(0.017)	(0.018)	(0.018)
N	3741000	3741000	3741000
	Panel C: Difference		
VRA × Black	0.009*	0.010**	0.018*
	(.005)	(.004)	(.010)
N	670000	670000	670000
County-level Controls		X	X
Race-Education Controls			X

Notes: This table presents regression coefficients from 9 separate regressions - three panels with three columns per panel, and each panel-column cell providing results from one regression. This table reports estimates of ordinary least squares regressions relating the VRA to (relative) black wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends for column (3) (our preferred specification), while in column (2) are interacted with only linear trends to show robustness. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC.

Table B5: Effect of the VRA on Cross-Border Migration, 1960-1970

	(1)	(2)	(3)	(4)
VRA	-0.109*	-0.118*	-0.123**	-0.124**
	(0.061)	(0.062)	(0.62)	(0.62)
VRA × Black			0.077	0.082
			(0.048)	(0.052)
N	198000	198000	198000	198000
County Controls	X	X	X	X
Individual Controls		X		X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. The sample used is the "migration sample" (i.e., those individuals who changed residence from five years earlier). Each coefficient is an estimate from OLS regressions relating the Voting Rights Act to cross-border migration, using Census data on a person's place of residence five years ago. An observation is an individual in a given Decennial Census year. The dependent variable is an indicator for whether a person moved across VRA lines, and the independent variables are VRA and VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table B6: The Effect of the VRA on Public Sector Employment, 1950-1980 (Robustness)

	(1)	(2)	(3)	(4)
VRA × Black	0.028**	0.03***	0.024**	0.035***
	(0.11)	(0.01)	(0.011)	(0.009)
N	673000	673000	673000	673000
Human Capital Controls		X	X	X
County-level Controls			X	X
Returns to Ed. by Race				X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to employment in the public sector. An observation is an individual in a given Decennial Census year. The dependent variable an indicator that equals 1 if an individual is a government employee. The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table B7: The Effect of the VRA on Public Sector Employment, 1950-1980 (Absolute Levels)

	(1)	(2)
VRA	0.082***	0.055***
	(0.01)	(0.01)
N	34000	34000
County-level Controls		X

Notes: This table presents regression coefficients from 2 separate regressions, one per column. Each coefficient is an estimate from linear probability regressions relating passage of the VRA to employment in the public sector. An observation is an individual in a given Decennial Census year. The dependent variable an indicator that equals 1 if an individual is a government employee. The independent variable is the VRA indicator variable, for whether the VRA was in place in a given county and year. Standard errors are in parentheses and are clustered by county. County controls are measured at 1960 levels, and interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC.

Table B8: Impact of the VRA on County-level Public Employment, 1957-1982

	(1)	(2)	(3)
VRA	0.001	0.001	0.001
	(0.030)	(0.001)	(0.002)
N	1780	1780	1780
County-level Controls		X	X
County Trends			X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the VRA to the overall size of the public sector. The dependent variable is the size of the government workforce, normalized by total population. All regressions include county pair-year and county fixed effects. Standard errors are in parentheses and are clustered by county. Controls are measured at 1960 levels, and interacted with linear and quadratic time trends. ***,**,* denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table B9: Testing the Human Capital as a Mechanism

	Outcome: Education			Outcome: Log(Wage)			Outcome: Higher Ed. Achieved?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VRA ×Black	-0.111	-0.132	-0.105	0.061**	0.061**	0.061**	-0.01	0.012
	(0.133)	(0.136)	(0.139)	(0.03)	(0.03)	(0.03)	(0.022)	(0.15)
N	673000	673000	673000	673000	673000	673000	673000	673000
Controls - 1		X	X		X	X	X	X
Controls - 2			X			X	X	X

$^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$

Notes: This table presents regression coefficients from 8 separate regressions, one per column. Each regression result reports estimates of ordinary least squares regressions relating the VRA to either: black (relative) wages (Columns (1)-(3)), log wages (Columns (4)-(6)), or an indicator variable for whether a respondent completed high school or college (Columns (7) and (8), respectively). An observation is an individual in a given Census year. The independent variable is VRA × Black (the interaction between a worker's race and whether the worker's county of residence was covered by the VRA in a given year). All regressions include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. Controls are county characteristics in 1960 interacted with linear ("Controls-1") and quadratic time trends ("Controls-2"). $^{***}, ^{**}, ^{*}$ denotes statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

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C Model

The goal of building this model is to provide analytical clarity regarding the impact political power has on racial wage and unemployment disparities. In short, political power directly affects redistribution by changing the allocation of public employment, public wages and benefits. However, how these effects translate to the private sector is less clear. To guide our thoughts on how political empowerment can affect labor market outcomes, we develop a labor equilibrium model with search frictions in the vein of Mortensen-Pissarides. But in order to account for redistributive policies amongst groups of voters we augment our model by incorporating hiring in the public sector.

The public sector and unemployment transfers are controlled by a politician who maximizes a welfare function weighted by the relative political strength of each group (in our case, black and white voters). Our model follows a line of research exploring the role of government intervention in the labor market. For example, in one recent paper, Kline and Moretti (2013) use similar tools to explore the interaction between migration, standard spatial equilibrium models, and the impact of place-based policies on the labor market.

To incorporate racial disparities within the labor market into our setting, we allow private sector employers to allocate vacancies across different groups of workers. This is sensible as long as there are incentives for the employer to hire differently as, for example, when one group has a lower bargaining power. As hiring in one group entails increasing search costs, in equilibrium, both groups are employed.

The rationale behind our model is that by raising the demand for one group of workers in the public sector, the government impacts the labor supply of this group in the private sector, thus increasing the group's private sector wage. We show formally that this increase in wage is greater than offsetting changes in both the demand for the affected and unaffected groups of workers. Expectedly, employment rates in the private sector will decrease for the affected group relative to the unaffected one. As we will show, this will have the consequence of affecting the redistribution policies in the public sector.

C.1 Labor Market with Public Employment

We consider an economy where workers differ only along a non-productivity dimension, $i \in \{a, b\}$ under which they can be identified (e.g., race). Each dimension contains an identical continuum of infinitely lived workers of measure one. The private sector employer interviews candidates with full information of their type, or equivalently, posts vacancies (v_i) for each group. Each match generates productivity p. The matching function, $m(u, v)$ is increasing and concave in both unemployed workers (u) and vacancies (v), and has constant returns to scale. The arrival rate for workers is defined as $\frac{m(u,v)}{u} \equiv m(\theta)$, where $\theta = \frac{v}{u}$ is the labor market tightness. The hiring rate per vacancy is defined as $\frac{m(u,v)}{v} = \frac{m(\theta)}{\theta} \equiv q(\theta)$. The arrival rate of job offers for workers is increasing in labor market tightness, $m_\theta(\theta) > 0$, while the hiring rate decreases with labor market tightness, $q_\theta(\theta) < 0$. The wage for each group is determined by bargaining between the employer and each employee of all groups. While the bargaining position and labor market tightness might differ across groups, the marginal product of labor is the same for each worker. γ is the cost to the firm of posting a job. δ is the exogenous separation rate, which we take to be constant across groups and types of employers. Search on the job is not allowed. To simplify notation, we postpone the use of superscripts to next subsection.

The value of a unfilled vacancy obeys:

$$rV = -\gamma + q(\theta)(J - V)$$

while the value of a filled vacancy follows:

$$rJ = -w + \delta(V - J)$$

Competitive entry of firms to the market requires that the value of an unfilled vacancy goes to zero:

$$rV = 0$$

We depart from the standard model by adding public sector employment. The public sector wages, w_g, and the public hiring matching rate, are decided by the politician and taken exogenously by the market. The value of public sector employment follows:

$$rW_g = w_g + \delta(U - W_g)$$

The value of private sector employment, and unemployment are given by:

$$rW = w + \delta(U - W)$$

$$rU = b + m_g(W_g - U) + m(\theta)(W - U)$$

The wage for each group is determined by Nash Bargaining principles:

$$\beta J = (1 - \beta)(W - U).$$

The equilibrium dynamics of unemployment, public sector employment, and private sector employment are governed by the flows in and out of unemployment. In the steady state, flows from unemployment to employment must match separations:

$$u = \frac{\delta}{\delta + m(\theta) + m_g}$$

$$e_g = \frac{m_g}{\delta} u, \qquad e = \frac{m(\theta)}{\delta} u$$

$$e + e_g + u = 1$$

The model can be reduced to the following two relationships for each group:

$$\frac{\gamma}{q(\theta)} = \frac{p - w}{r + \delta} \tag{13}$$

$$w = \frac{\gamma\beta}{1 - \beta} \frac{r + \delta + m(\theta)}{q(\theta)} - m_g \frac{b - w_g + \theta\frac{\gamma\beta}{1-\beta}}{r + \delta + m_g} + b \tag{14}$$

Equation (13) is the familiar job creation condition. As expected, labor market tightness decreases with wage and increases with the productivity level. Equation (14) is the wage equation and can be rewritten as:

$$w = \underbrace{\frac{\gamma\beta}{1-\beta}\frac{r+\delta}{q(\theta)}}_{\text{Surplus Distribution}} + \underbrace{\frac{r+\delta}{r+\delta+m_g}\left(\overbrace{b}^{\text{Transfers}} + \overbrace{\theta\frac{\gamma\beta}{1-\beta}}^{\text{Search Incentives}}\right) + \frac{m_g}{r+\delta+m_g}\overbrace{w_g}^{\text{Public Wage}}}_{\text{Social Planner Component}} \tag{15}$$

This relationship illustrates the various components determining the wage level in the private sector. The wage is given by a surplus distribution component plus a component arising, at least partly, through the intervention of the public sector. This component can be thought of as a weighted average of the public wage on the one hand, and unemployment benefits and search incentives on the other, where the weights are determined through public hiring.

C.2 Politician

The politician's goal is to maximize a weighted average of the welfare of each group of voters, where the weights are a function of each group's political strength. The welfare of each group is given by:

$$\Phi^i = e^i(w^i - \tau) + e^i_g w_g + u^a b$$

where τ is the tax rate, e^i_g is race-group government employment, and e^i is a group's private employment. Since workers have linear utility, and public transfers as well as public wages are financed through the tax proceedings, it follows that a given race group's welfare equals the total private wages:

$$\Phi^i = e^i w^i$$

Denoting the political strength of blacks by ω, the politician's problem is to solve:

$$\max_{m^a_g, w^a_g} (1-\omega)\Phi^a + \omega\Phi^b$$

subject to the budget constraint:

$$e^a_g w^a_g + e^b_g w^b_g + u^a b^a + u^b b^b = (e^a + e^b)\tau$$

The following derivatives are useful:

$$\frac{d\Phi^i}{db^i} = e^i\left(\frac{r+\delta}{r+\delta+m^i_g}\right), \ \frac{d\Phi^i}{dw^i_g} = e^i\left(\frac{m^i_g}{r+\delta+m^i_g}\right), \ \frac{d\Phi^i}{dm^i_g} = \frac{d\Phi^i}{db^i}\left(\frac{b^i - w^i_g + \gamma\theta^i\frac{\beta^i}{1-\beta^i}}{r+\delta+m^i_g}\right).$$

The first order conditions with respect to public wages, w_g, and public hiring, m_g, provide the following equilibrium conditions:[65]

$$\begin{cases} (i : Public\ Wage) & \omega\left[1 + \frac{e^b}{e^a}\frac{u^a}{u^b}\frac{r+\delta+m^a_g}{r+\delta+m^b_g}\right] = 1 \\ (ii : Public\ Hiring) & \frac{w^b_g - b - \gamma\theta^b\frac{\beta^b}{1-\beta^b}}{r+\delta+m^b_g} = \frac{w^a_g - b - \gamma\theta^a\frac{\beta^a}{1-\beta^a}}{r+\delta+m^a_g} \end{cases}$$

_____

[65] See Appendix for details.

From conditions *(i)* if there is an increase in black workers' political strength, the politician needs to increase black public hiring, m_g^b, relative to white public hiring, m_g^a, in order to stay in equilibrium:

Remark 1 (Public Hiring): *Given an increase in political strength of group b, ω, public hiring for group b must increase relative to group a.*

Furthermore, since from condition *(i)* public hiring for blacks increases, condition *(ii)* implies that the public wage of black workers increases relative to the public wage of white workers, i.e. the public wage gap narrows:

Remark 2 (Public Wage Gap): *Given an increase in political strength of group b, ω, the public sector wage disparity between group a and group b narrows.*

From the wage equation (13), condition *(i)*, and *(ii)* it follows that the wage gap in the private sector narrows. To see this, consider the job creation conditions for each group:

$$\frac{\gamma}{q(\theta^a)} = \frac{p - w^a}{r + \delta}, \qquad \frac{\gamma}{q(\theta^b)} = \frac{p - w^b}{r + \delta}.$$

Subtracting both equations, and taking derivatives on both sides, we obtain a relationship characterizing the change in racial wage gap:

$$\frac{d(w^a - w^b)}{d\omega} = (r + \delta)\gamma \frac{d}{d\omega}\left(\frac{1}{q(\theta^b)} - \frac{1}{q(\theta^a)}\right)$$

The equilibrium unemployment for each group is:

$$u^i = \frac{\delta}{\delta + m(\theta^i) + m_g^i}$$

rearranging yields an expression for the private sector equilibrium match rate:

$$m(\theta^i) = \frac{(1 - u^i)\delta - m_g^i u^i}{u^i} = \frac{\delta}{u^i} - \delta - m_g^i$$

Since $m(\theta^i)$ is an increasing function of θ^i, an increase in the public sector match rate implies θ^i must decrease. Similarly, since the hiring rate per vacancy, $q(\theta)$, is decreasing on labor market tightness, a decrease in θ^i implies $\frac{1}{q(\theta)}$ decreases. Since following an increase in political strength for blacks, the match rate for blacks increases relative to white's, the wage gap in the private sector is also reduced:

Remark 3 (Private Wage Gap): *Given an increase in political strength of group b, ω, the private sector wage disparity between group a and group b narrows.*

Importantly, condition *(ii)* also describes the consequences of increasing unemployment transfers and decreasing overall public sector wages. Although the narrowing in the public sector wage gap is necessary, changes in unemployment transfers can be used to attenuate the extent to which the wage gap narrows. This is to say, unemployment benefits can increase after an increase in the political strength of one group vis-à-vis the other. But that change is to attenuate the gains accrued by the disadvantaged group in the private sector. To see this, recall the wage equation:

$$w = \frac{\gamma\beta}{1 - \beta}\frac{r + \delta}{q(\theta)} + \frac{r + \delta}{r + \delta + m_g}\left(b + \theta\frac{\gamma\beta}{1 - \beta}\right) + \frac{m_g}{r + \delta + m_g}w_g$$

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Since the private sector wage is affected by the weighted average of benefits and public sector wages, the group with the highest public sector matching rate will benefit the least. Public sector revenue neutrality implies:

$$\frac{d(w_g^b - w_g^a)}{d\omega}e_g^b + \frac{db}{d\omega}u + \frac{dw_g^a}{d\omega}(e_g^b + e_g^a) = 0$$

Condition *(ii)* tells us that public revenue can be reallocated (1) from benefits to reduce the wage gap; and (2) from level wages to either increase benefits or reduce the wage gap. The optimal transfer is determined by the matching rate for each group, and by the size of the unemployed sector relative to the size of black government employment. Formally:

Remark 4 (Unemployment Transfers & Public Wages): *If, $m_g^a > m_g^b$, revenue will be reallocated from unemployment transfers to reduce the public sector wage gap. If, $m_g^a < m_g^b$, revenue will be reallocated from level wages to unemployment transfers, if $\frac{m_g^b - m_g^a}{u(r+\delta+m_g^a)} > \frac{1}{e_g^b}$, and to reduce the public sector wage gap, if $\frac{m_g^b - m_g^a}{u(r+\delta+m_g^a)} < \frac{1}{e_g^b}$.*

Remark 4 provides an unexpected perspective on why transfers increase in places with more minority political participation. Transfers do not necessarily operate to optimize the welfare of black constituents, but instead could operate to compensate losses of whites in the private sector. To see this, we can again to examine the wage equation. When $m_g^a > m_g^b$, black workers benefit the most from transfers, but since these funds are used to finance the reduction in the wage gap, the reduction is partly offset. Conversely, when $m_g^a < m_g^b$, unemployment transfers benefit white constituents more, while decreasing wage levels in the public sector affects black workers more. By financing extra transfers through reductions in wage levels, transfers are used for the purpose of offsetting private wage gains generated by increased public sector hiring and reductions in the public sector wage gap.