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 is having an equal opportunity to advance group interests via the political process. There remains limited empirical evidence, however, whether democratic policies designed to foster political equality are connected causally to social and economic equality. In this paper, we examine whether and how the expansion of minority voting rights contributes to advances in minorities' economic interests. Specifically, we consider 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 their 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 for black workers and afforded these workers existing public-sector wage premia. 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: Labor Markets, Racial Discrimination, Public Sector Employment, Electoral Politics

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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. Leaders of this movement 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."

This paper examines whether and how the political incorporation generates individual economic benefits for disadvantaged minority citizens by examining one of the most significant episodes of minority enfranchisement worldwide: the passage of the 1965 Voting Rights Act (VRA) in the United States (U.S.). The VRA outlawed discrimination at the voting booth directed against racial minorities (and black Americans in particular). As a consequence, the size of the black American electorate increased almost overnight – particularly in the southern U.S., where voting rights had been heavily restricted during the previous century.[2]

By making politicians responsible to black voters, the statute was intended to increase the responsiveness of local, state, and federal-level representatives to racial minorities' policy interests. During the civil rights era, these interests centered on the alleviation of poverty and economic inequality (perpetuated and maintained via black political suppression). Yet, there remains relatively little evidence that political power furthered these interests. We bring evidence to bear on this question. Given that the aspect of economic status most important to blacks during this time was equal access to opportunities in the labor market (Button 1989), we examine the impact of political enfranchisement on the Southern black-white wage gap. We also seek to understand the mechanisms through which the VRA promoted racial economic equality. Understanding these downstream effects of minority voting power is particularly important given that key protections of the VRA were effectively struck down by the Supreme Court in its 2013 *Shelby County v. Holder* ruling.

To identify the causal effect of voting rights on labor market inequality, we exploit the temporal and spatial variation in minority voter protection and participation afforded by the targeted application of the VRA. While the blanket ban on voting discrimination applied nationwide, the

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

[2] 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.

statute's strictest requirements – enumerated in Section 5 – were for jurisdictions where discrimination had been particularly severe. In these places, federal authorities took active steps to register minority voters and protect against discrimination at the polls. Section 5 applied to only a subset of counties and states primarily in the South and Southwest United States (see Figure I).

The problem for causal inference using Section 5 is a familiar one. Congress did not choose which political jurisdictions would be subject to federal election oversight randomly. Several factors likely influenced this decision. While some of these factors are observable to the researcher (such as discriminatory laws or economic conditions), many are not (such as culture or racial animus). Such factors were also potentially correlated with economic outcomes. Rather than comparing outcomes across all covered counties and uncovered counties, which would largely amount to a comparison of South to North, we focus on adjacent county pairs (both across states and within states) – where one county is protected under the VRA, and the other is not.[3] The benefit of this approach is that it allows us to control for smoothly-varying unobservable conditions (such as cultural, political, or economic differences) that may confound estimates obtained using more coarse data (e.g., at the state level). This approach better approximates a quasi-experimental setting where the researcher compares "like" treated counties with "like" control counties. This design and the VRA coverage variation require more geographically-detailed location data than is typically available in public-use data samples. We thus use confidential administrative data from the U.S. Census Bureau that includes detailed information on workers' residence and mobility.

The central contribution of our paper is to test whether there exists a link between minority political empowerment and economic well-being – in other words: does the franchise have instrumental value for excluded minorities? We find that it does. The VRA reduced the conditional wage gap between black and white workers by around 5.5 percentage points between 1950 and 1980 (statistically significant at the 5 percent level), a result that is robust to several alternative specifications. This effect is driven primarily by rising black wages within VRA-covered counties, with no statistically significant cost to overall employment. Moreover, these effects are consistent with the VRA making government more responsive to black voters' interests in socioeconomic inequality. To show this discrete shift in political power in favor of black Americans, we also show that the stronger voter protections of the VRA led to a sustained increase in both political participation, as well as legislative actions favorable to minority interests.

As numerous scholars have noted, a concern for isolating the effect of any single American civil rights reform on economic outcomes is separating the effects of a single policy, such as the VRA, from the other major civil rights reforms from this period (Donohue and Heckman 1991; Smith and Welch 1977). The 1960s ushered in several major federal reforms designed to ameliorate economic inequality – including the Equal Opportunity Act (1963), the Civil Rights Act (1964), the Fair Labor Standards Act Amendments (1966), and the Fair Housing Act (1968).[4] To alleviate concerns about concurrent civil rights laws, unobserved state institutional differences, or differential

[3] Figure I presents the nationwide variation in VRA coverage, discussed at length in Section 2.

[4] Note, however, that unlike the VRA, all of these laws applied with equal force nationwide.

trends, we exploit both the temporal variation arising from the expansion of voting rights protection as well as unique within-state variation. We also analyze a separate 1975 expansion of the VRA across over 200 counties in Texas, Arizona, and other counties within the Southwest. Additionally, we limit our analysis to one state (North Carolina) where approximately half of all counties were covered, and the remainder uncovered. Across these numerous subsamples and specifications, we observe effects of similar directions and magnitudes.

Having documented a causal relationship between political power and economic status, we then proceed to probe various potential economic channels through which black political incorporation reduced labor market inequality. We first focus on a form of direct redistribution that elevated the wages of black Americans: government employment. We find that in VRA-covered counties, black Americans were between 2 and 4 percent more likely to receive a government job relative to white workers. Two stylized facts highlight the plausibility of this channel. First, public-sector workers commanded a substantial premium over their private-sector counterparts – and this premium was particularly high for black workers, who earned 20 percent more in the public sector than in the private. Second, the size of the government workforce (particularly at the local and state levels) grew steadily during the second half of the twentieth century, providing new job vacancies for employment that could be affected by politics.

Both the increase in black public sector hiring as well as the public wage premium have the potential to put significant upward pressure on wages in the private sector. We demonstrate that was indeed the case; in addition to the direct benefits of the VRA 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.[5] We show that in occupations where private firms face greater competition with the government for black 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 government labor force (influenced by minority political power) contributed to wage equality in markets where discrimination existed previously. We consider these private sector wage-bargaining effects through the lens of a search model (Beaudry, Green, and Sand 2012), both to clarify conceptually the connection between the private and public sector, as well as to quantify the civil rights era's public sector effect on labor market outcomes.

Political participation and influence can also affect the labor market performance of minorities through other economic channels. We provide evidence consistent with voting rights contributing to and/or complementing existing labor market inventions that aimed to reduce black-white income disparities, such as anti-discrimination and affirmative action policies. We cast doubt, though, on channels related to improved human capital, such as improvements in the education of black workers. Finally, we consider *how* the right to vote affects political power – whether it is by changing

[5] 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.

the incentives faced by all politicians (i.e., spatial competition), or by increasing the presence of black elected officials. In our view, the weight of the evidence favors the former.

1.1 Contributions & Roadmap

Our paper lies at the intersection of research within labor economics, political economy, and economic history. First, we contribute to research on what factors affected declining labor market inequality over the twentieth century. Several studies on this area generally emphasize one of two general hypotheses regarding the causes of black-white economic convergence. On one hand, research by Smith and Welch (1989) and others show that the improvement of black economic status was driven by changes operating through supply-side mechanisms, such as the changing quantity/quality of schooling, out-migration, and the crowding-out effects of expanding social welfare programs.[6] Alternatively, on the demand side, Freeman (1973), Leonard (1990), Donohue and Heckman (1991), Chay (1998), Derenoncourt and Montialoux (2018), and others argue that legislation passed during the civil rights era contributed measurably to the improved relative economic status of black workers. These studies examine the effects of labor market regulations, such as the 1964 Civil Rights Act (CRA), the creation of affirmative action requirements, and the expansion of the minimum wage. However, while Donohue and Heckman (1991) allude to the importance of black political power to enforce the civil rights-era policy agenda to reduce economic inequality, there has been no formal test of the role played by the VRA. This paper thus provides the first empirical evidence that minority political power may also have contributed to the reductions in black-white economic inequality observed during the period.

Second, we contribute to work examining public sector employment in the U.S. Numerous researchers have shown that government jobs provided a source of economic mobility for black Americans beginning in the mid-20th century. Minorities in the public sector historically earned more than their private sector peers, in part due to the fact that government provided greater opportunities for white-collar employment and upward occupational mobility (Eccles 1975; Freeman 1976; Hout 1984; Pitts 2011). Raw employment statistics suggest that the public sector became an important occupational niche for black workers beginning in the 1960s, as suggested by labor historians Katz, Stern, and Fader (2005). We highlight this trend in Figure III, which shows the drastic movement of black workers into the public sector beginning in the mid-1960s, particularly within the South. We provide evidence of one causal factor – political influence – related to the increasing importance of the public sector for black employment after 1960.[7] The closest paper to

[6] Smith and Welch (1989), for example, show that increasing quantities of schooling can explain about 20-25 percent of the black-white wage gap narrowing in the late 1960s. Card and Krueger (1992) document similar findings, but also argue for a substantial role of anti-discrimination laws. Finally, President Lyndon Johnson's Great Society expansion of social welfare programs may have put upward pressure on wages. For example, marginal workers have responded to increased public assistance by reducing their participation in the labor force; this was particularly plausible for black workers with low levels of education. Donohue and Heckman (1991), however, show that this factor can explain only about 10-20 percent of wage convergence.

[7] The black share of public employment grew particularly sharply in the Deep South post-VRA (Figure IV). The time path of white public-sector employment, on the other hand, is unchanged. These raw statistics provide *prima*

ours in this regard comes from Henderson (2017), who documents that restricting immigrant voting rights at the turn of 19th century decreased the public-sector employment of such immigrants.

Third, we contribute to literature on the effects of minority political incorporation and representation. Theoretical work such as Romer (1975), Roberts (1977), and Meltzer and Richard (1981) suggest that that extending the franchise should, by shifting the median voter toward the poor, increase pro-poor redistribution and in turn reduce inequality. Despite such predictions about the relationship between political and economic inequality, there is relatively little empirical evidence on the economic value of political voice for disadvantaged minorities. Existing studies focus on how minority voting rights shape government spending and redistribution (Cascio and Washington 2014; Husted and Kenny 1997). Research also documents individual-level benefits for the children of voters that stem from enabling poor and women voters – in particular, improvements in child health and education (Miller 2008; Naidu 2012; Carruthers and Wanamaker 2017; Fujiwara 2015; Kose, Kuka, and Shenav 2019). To date, however, little evidence has been offered on the topic of whether democratic participation and accountability concretely improve the material circumstances of minority voters themselves. We demonstrate that enfranchisement and political influence can in fact confer direct benefits to the marginalized group receiving political rights.

Our study also relates to work on the employment effects of the changing supply of minority politicians (i.e., "descriptive representation"). Eisinger (1982) and Nye, Rainer, and Strat (2015), for example, document how increases in minority city council members and mayors improve minority employment outcomes in both the private and public sectors (i.e., better jobs and better pay). Our study differs from these studies by examining the effect of citizen political empowerment more generally, rather than on the effect of descriptive representation.

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 Section 6, and political mechanisms in Section 7. Finally, we offer brief concluding remarks in Section 8.

2 Context & Historical Background

In this section, we briefly discuss the context for our empirical study, including the history of black voter suppression and the structure of the VRA. The VRA sought to both facilitate the inclusion of black Americans in politics, as well as to address racial disparities in economic status left by decades of civic exclusion concentrated primarily in the American South.

facie evidence that the VRA was associated with the changing racial composition of the government workforce.

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

After the Civil War and the end of slavery, American states ratified the Fifteenth Amendment to the U.S. Constitution. The Fifteenth Amendment guaranteed the right to vote to all men regardless of "race, color, or previous condition of servitude." With the vote and its concomitant political power, black Americans in the South prospered for the first time in the country's history, during the period known as the Reconstruction (Logan 2018). This period of political and economic progress was short-lived, though, as formerly Confederate political jurisdictions sought to reimpose the racial hierarchy that allowed for white Americans' social, political, and economic dominance. To this end, Confederate states and counties responded to the expansion in black Americans' political rights by imposing policies between 1870 and 1910 that, while ostensibly 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 denial of voting rights contributed in several ways to economic inequality along racial lines. With a monopoly on political power, whites excluded blacks from receiving local public goods or forms of redistribution. Research by Margo (1982), Kousser (1980), and Pritchett (1989), for example, shows that Jim Crow political restrictions significantly reduced the quantity and quality of schools attended by black children. Southern governments also enacted regulations that segregated blacks and whites on most dimensions of social and economic life. Legal segregation reduced black citizens' access to public transportation and reduced access to many services. As a consequence, Jim Crow segregation reduced the competitiveness of black labor in at least two ways: (1) by lowering the returns available to black workers from participating in the labor market and (2) by raising the costs borne by establishments that employed black workers (Anderson and Halcoussis 1996). More generally, the system of segregation maintained by political suppression served as a reminder to blacks that they were second-class citizens in all dimensions of civil, economic, and political life. Jim Crow disenfranchisement and segregation also led to the outmigration of black families from the South (Naidu 2012).

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

It is now widely acknowledged that the widespread political exclusion of racial minorities between Reconstruction and the civil rights era was a major driver of Southern black-white economic dispari-

[8] Examples included whites-only primaries, poll taxes, and literacy tests. Poll taxes are straightforward to understand. Whites-only primaries prohibited eligible black voters from participating in primaries rather than elections, since primaries were administered by semi-private party organizations that fell outside government regulation, but controlled who would contest many elections. 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.

ties between 1890 and the early 1960s (Roback 1984; Sundstrom 2007; Wanamaker 2017). The right to participate in elections and shape policy thus became a centerpiece of the Civil Rights movement for socioeconomic equality during the 1950s and 1960s. Policymakers and activists viewed voting rights (and the political representation and influence that would come with an equally-weighted vote) as a necessary step toward improving minority socioeconomic status. In 1965, months after civil rights activists' famous march from Selma to Montgomery, President Lyndon Johnson signed the VRA into law, restoring for black Americans (and all racial/ethnic minorities) the right to vote in the South. The sections of the VRA that are still intact remain today the key federal statutory tools for attacking discrimination against racial minorities 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 to challenge vote-denying practices (such as voter identification requirements), as well as 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 preclear any change to electoral procedures with the U.S. Attorney General or the U.S. District Court for D.C.[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 discussed less often by researchers, was crucial for ensuring that previously discriminatory jurisdictions could no longer "foot drag" to register eligible black voters (Karlan 1989). Within two years of the VRA's passage, the Lyndon 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 (Sec-

[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.

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

tion 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, nearly half of North Carolina's 100 counties, and one county in Arizona.[14] Amendments to the VRA in 1975 (henceforth, the "VRA Amendments" or "Amendments") 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

Theoretical work such as Cox and Mccubbins (1986), Dixit and Londregan (1996), and Lindbeck and Weibull (1987) hypothesizes that elected officials distribute resources to clearly identifiable constituent groups in order to maximize votes. The VRA created such incentives for Southern politicians to respond to the needs of black American communities, since these constituents tended to be geographically compact and relatively homogeneous in their 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. The central political concern for black Americans was equal access to employment opportunities, free of discrimination (Schwartz 1967). Figure 1 presents data from a nationwide survey conducted in 1963 by the National Opinion Research Center (NORC). The data indicate that equal opportunity for advancement within the labor market was by far the central policy concern for black Americans at the time the VRA was passed. As such, to the extent that newly-enfranchised black voters expected government to respond to particular policy interests, dedicating state resources to ensure equal labor market opportunities was a plausible policy area for government action.

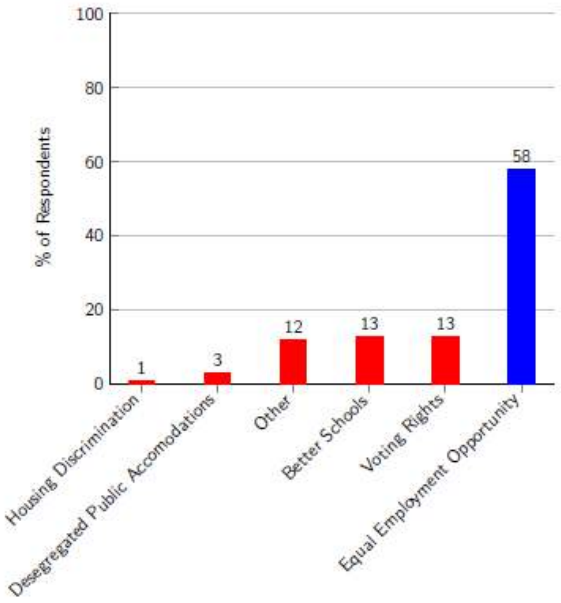
[14] The original coverage formula looked at whether jurisdictions imposed discriminatory procedural devices at the time of passage, whether less than 50 percent of the voting-age population was registered to vote as of that date, or if less than 50 percent of eligible voters voted in the November 1964 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] That blacks were not a majority of a locality's electorate would not necessarily preclude a relationship between their voting strength and policy outcomes, since black voters could form *de facto* political coalitions with sympathetic whites.

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. Black political empowerment may have contributed to economic equality, including within the 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 increased the demand for black workers. Politicians exercise significant authority over the distribution of contracts, which could be used to discourage discriminatory practices and increase minority hiring. 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. Politicians can also directly affect the demand for minority labor through government hiring. Several studies have demonstrated how political factors affect the size and composition of the government workforce in the U.S. (Alesina, Baqir, and Easterly 2000; Glaeser and Shleifer 2005; Enikolopov 2014; Chen, Henderson, and Cai 2017). Relative to private firms, which are typically understood as profit-maximizers, government agencies may be guided by factors beside efficient production. For example, politicians use the employment/promotion of minorities and women as

a tactic to reverse historical patterns of discrimination (Blank 1994).

While new opportunities for government employment offered black workers opportunities to earn a higher income, the compositional change of the minority workforce (toward greater public-sector employment) may also have exerted indirect pressure on private-sector employers. Because government agencies often comprised a meaningful share of the local labor market (often around 20 percent 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.[17] Such inter-sector general equilibrium effects have recently been formalized and estimated in the labor economics literature. In the context of changes to local industrial composition, Beaudry, Green, and Sand (2012) document that sectoral demand shocks have substantial cross-sectoral general equilibrium effects on worker wages. When accounting for such effects, total wage effects are 3-4 times larger than the effects as measured by just considering the direct effects.[18] Consequently, a complete evaluation of the economic impact of a public sector channel (caused by an exogenous political shock) may require accounting for such equilibrium 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 blacks' right to vote improved the economic status of black workers (both in absolute terms and 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.

[17] 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.

[18] 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 B.

4.1 Data Construction

We rely on several sources of data in this paper. Our main data are the restricted-access United States Decennial Censuses (DEC) from 1950, 1960, 1970, and 1980.[19] The long-form Censuses are a 20 percent representative sample of the U.S. population (except for 1950, for which only a 1 percent 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.(Smith and Welch 1989; Donohue and Heckman 1991; Card and Krueger 1992).[20] The time period we analyze includes fifteen years before and after the peak of the civil rights movement, 1965, when the VRA was passed. This period of time covers the primary period during which black progress in the labor market was observed (Bound and Freeman 1992). The advantage of using the restricted-use Censuses is that they contain detailed information on a respondent's county of residence, 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.[21] 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 set of 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 black political empowerment. 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 Census demographic data.[22] 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.[23] To examine changes in politician responsiveness preferences, 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). DW-NOMINATE is a multidimensional scaling technique which collapses legislative roll-call voting into a twodimensional ideal point. While the first dimension is commonly considered to be the contemporary liberal-conservative measure (scaled from -1 to 1), the second dimension of DW-NOMINATE, which we examine here, has historically tracked policy issues that cut across party lines and relate to support for civil rights and other race-related issues.[24]

[19] We access these data within the Berkeley Research Data Center (RDC).

[20] Future work will consider the impact of political empowerment on black women.

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

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

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

[24] Research suggests that after 1980, there is 2nd dimension is no longer useful due to Southern Realignment. This is not a problem for our purposes, though. Moreover, for corroboration, we also compare these results to a coding of all congressional roll-call votes (by district and year) in favor of civil rights-related issues, produced by Schuit and

Finally, 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 (COG). To examine political mechanisms, such as the impact of the VRA on the election of black politicians, we digitize over ten years of original data from annual reports produced by the Joint Center for Political and Economic Studies (JCPES). The JCPES produces an annual listing of every black politician in the country (except for 1970), beginning in 1969.[25] We supplement this source with data from Matthews and Prothro (1966), who collected information about black elected officials in the early-1960s South.[26]

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 – any or all of which may affect our outcomes of interest – are likely to change smoothly rather than discretely 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).[27] 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.

Rogowski (2017). Results are available upon request.

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

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

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

4.2.1 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 and the within-state variation in VRA coverage.[28] Our sample includes all pairs of adjacent counties where one county is VRA-covered and the other county is not. The counties that are in our sample can be seen in Figure II below.

We can provide corroborating evidence that our research strategy better approximates an "apples-to-apples" comparison. Table A.1 presents summary statistics for our sample – including average county characteristics based on data from the Census as well as other sources in 1960, just before the VRA was passed. 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 the data to neighboring covered and uncovered counties. Panel A of Table A.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 VRA and non-VRA counties are equivalent. As Panel A demonstrates, the differences between VRA and non-VRA are often always different at the 5 percent level, for an array of observable characteristics. Thus, these summary statistics suggest that economic and political conditions were different in VRA and non-VRA Southern states, and suggest that there could be fundamental unobserved differences between these states that would confound an analysis of the VRA's causal impact using all counties. Panel B corroborates our assumption of smoothly-varying changes across borders. 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 government targeted the political empowerment of *black* Americans, we analyze the labor market outcomes of black workers relative to white workers. In short, we estimate the causal effect of the VRA on the black-white racial wage *gap*.[29] The primary empirical specification takes the following form:

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

In this difference-in-difference-in-differences (DDD) specification, c indexes county, $r(i)$ indexes

[28] 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.

[29] For an example of this approach, see Hirata and Soares (2016), who estimate the impact of trade liberalization on the minority-white wage gap in Brazil.

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 VRA-covered in a given year (post-1965 or post-1975). $\mathbb{I}\{r(i) = \text{Black}\}$ is an indicator for whether a worker is black. Because we limit our data 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).[30] The parameter of interest is β, the coefficient on the interaction $[\text{VRA}_{ct} \times \mathbb{I}\{r(i) = \text{Black}\}]$ (which we will refer to as VRA $\times$ 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 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).

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 that blacks' relative labor market performance would have evolved similarly in VRA and non-VRA counties after 1965 (1975) in the absence of the VRA (VRA Amendments). A potential threat to identification is any 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 remain constant over time. County pair-year-race fixed effects allow us to control for localized, 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 regression framework is that it controls for non-institutional factors that are unlikely to vary discretely at jurisdictional borders, and which may affect the racial wage gap – such as culture or prejudicial attitudes (e.g., sources of *de facto* discrimination against black Americans). Such factors, however, may vary substantially between entire states, or across counties that are far-flung from one another (Naidu 2012). 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

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

pattern must not be consistent across neighboring counties.[31] VRA coverage applied to counties within part or all of 21 states. We thus cluster at the county level for inference.[32]

We can indirectly assess the identifying assumption in a few ways. First, following Hornbeck and Naidu (2014) we examine predifference between VRA and non-VRA counties. We estimate how trends in various economic, demographic, and social characteristics before the VRA relate to future coverage. The regression is as follows:

$$\mathrm{Y}_{ct} - \mathrm{Y}_{c(t-a)} = \beta \mathrm{VRA}_c + \epsilon_c \tag{2}$$

Table I demonstrates that, in terms of many different economic, social, and demographic characteristics, there is no significant difference in county trends. This is true both with and without state fixed effects (Columns 1 and 2, respectively). When we examine the interior and border counties pooled, we find that the trend differences often *are* significant. Additionally, in Section 5 below, we use an event-study to suggest 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.

As controls, we include several 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).[33] County characteristics are interacted with linear and quadratic time trends.

5 Results

5.1 First-stage Effects: Did the VRA Change Politics?

The link between the VRA and downstream improvements in minority socioeconomic outcomes likely depended on how it made government accountable to black voters' interests in economic opportunity. We thus begin by briefly assessing the impact of political incorporation on black political participation (a plausible first-stage effect the VRA).[34] To demonstrate that the VRA mobilized black voters as intended, we use voter turnout for presidential elections as our out-

[31] 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.

[32] For robustness, we also cluster our standard errors at the state level.

[33] 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.

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

come, as is standard in this literature (Ang 2018).[35] The econometric specification is the following differences-in-differences (DD) analogue of our main specification:

$$\text{Political Outcome}_{ct} = \alpha \, \text{VRA}_{ct} + \mu_c + \mu_{p(c)t} + \epsilon_{cp(c)t} \tag{3}$$

where c indexes county, t indexes year, $p(c)$ indexes county pairs. α provides an unbiased estimate of the VRA's causal effect of VRA coverage on voter turnout, under the assumptions that there are no time-varying differences across covered and uncovered counties, and that there are no geographic spillovers between counties. Spillovers are a concern if black voters chose to move to counties covered by the VRA.[36]

Table II presents results in Panel A. Consistent with the VRA mobilizing a new block of eligible voters, we find sizable, statistically significant increases in eligible voter turnout resulting from the extension and protection of minority voting rights under the VRA. The effects of the VRA on political participation are robust to several control variables (Column (2)), and the inclusion of state trends (Column 3). We find statistically significant increases in voter turnout ranging from 6 to 12 percentage points (p.p.) between 1950 and 1980; estimates are significant at the 1 percent level. We also find that the effect of the VRA is increasing in black population share (Column (4)). The coefficient on $VRA \times Black$ is positive and significant.[37] This specification suggests that the VRA increased participation at least in part through the mobilization of black voters, since these counties were more heavily affected by the VRA. In Appendix Table A.2, we demonstrate that results are unchanged when examining all counties within the states comprising our sample (not only the subset of county pairs).

In Figure V, we present the results graphically, displaying estimates of event-time dummies interacted with both the fraction of the population that is black, and a dichotomous indicator for counties that became VRA-covered. The graph provides visual support of a first-stage political effect, as well as support for the parallel trends assumption. The estimates are relatively stable in the years before the VRA takes effect (turnout is slightly worse in VRA counties – although the trend is relatively flat).[38] Post-VRA, covered counties experience a large, persistent increase in voter turnout relative to uncovered counties.

The conceptual mechanisms we discuss in Section 3 above rely on government action by those accountable to minority voters. As such, we also consider whether elected officials responded to minority political mobilization by supporting the preferred policies of black constituents. Because historical data on local/state policymaking is limited, we examine whether the VRA changed the

[35] We also return to a discussion of how the VRA improved accountability in Section 7.

[36] We demonstrate that migration is not a major concern since it is small quantitatively, and so is unlikely to account for measured effects on political and economics outcomes. See Section 5.3.2).

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

[38] 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 National Association for the Advancement of Colored People (NAACP), CORE, and Southern Christian Leadership Conference (SCLC) were actively engaged in voter registration drives during the early 1960s.

behavior of Congressional representatives, using the widely-known DW-Nominate score. This measure of Congressional behavior collapses a representative's legislative roll-call voting record into a time-varying measure of ideology, scaled from -1 to 1 (increasing in "conservativeness").[39] We focus on the second-dimension of the DW-Nominate score, which captures conservative ideology on issues related to race and civil rights. We estimate a district-level analogue of Table 3, defining VRA districts as any district that contains at least one VRA-covered county, consistent with standard practice in this literature (Ang 2018). Results are presented in Panel B Table II. Both with and without controls (Columns 1 and 2, respectively), the VRA coefficient estimates are negative and significant at the 10 percent level, indicating that the VRA made Congressional districts more racially liberal by 6-8 percentage points (Columns 1 and 2, which include and exclude controls, respectively). We also find that the effect of the VRA on racial conservativeness is decreasing in minority population, although the interaction between VRA coverage and the fraction of a district that is black is not statistically significant.[40] In Appendix Table A.5, we provide further evidence that the VRA affected policy outcomes at the state and local levels. Overall, our subanalysis provides evidence consistent with recent research by Schuit and Rogowski (2017), who show that VRA coverage increased government support for civil rights policy.

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

Knowing that the VRA achieved its initial purpose of minority political incorporation, we turn to testing the main hypothesis – did political empowerment produce tangible economic gains for poor black Southerners? As Figure 1 shows, eliminating labor market discrimination was *the* most important policy issue for black Americans in the 1960s. It is therefore reasonable to hypothesize that meaningful political power in the hands of black Americans would be used to address this 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 VI presents visual estimates of the impact of the VRA on wages for both black and white American workers (separately) as dependent variables. A limitation of using the administrative Census micro data is that we are limited to the censuses only beginning in 1950. We thus have just one pre-period difference in our main outcome. Accepting this limitation, a few features of these graphs are worth noting. 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

[39] See, for example, Poole and Rosenthal (2001) or Gentzkow, Shapiro, and Taddy (2016) for further descriptions of this data.

[40] In Appendix Table A.4, we repeat this exercise for the 1st dimension of DW-Nominate, which measures a representative's overall conservativeness across all issues (i.e., not limited to only race-based issues). We find little evidence that the VRA made representatives more favorable to generally Democratic interests, although representatives become more liberal within those places where the eligible minority vote share was higher (Column (3)).

(close to 5 p.p., significant at the 5 percent 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 percent of what similarly skilled white workers earned in terms of wages), Figure VI suggests that the VRA did indeed improve racial equality within the labor market. Table III 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 IV 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 percent level. Moreover, the increase in black relative wages is not part of an overall *decline* in wages within VRA counties, as Figure VI shows. 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 ($p<0.03$). 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 percent 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 evidence that the expansion and protection of black political rights had a beneficial effect in terms of promoting socioeconomic equality.

The magnitude of our estimated effect appears reasonable compared against the existing literature on drivers of racial wage and earnings gaps. Wage ratios within our sample (conditional on worker characteristics) increased from around 55 percent to just above 80 percent between 1960 and 1980. Our estimates account for around one-fifth of the decline in the adjusted wage gap. This effect is only within the South (the contribution would likely be smaller if we to considered nationwide wage convergence). By contrast, Card and Krueger (1992) find that about 15-20 percent 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 accounted for around 10-20 percent of black-white wage convergence during this period. Finally, another 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 percent of the reduction in the racial earnings gap. Importantly, minority political power may have either contributed to or been complementary to *any* of these other channels. We find support for such complementarities, as we discuss below in Section 6.2.

Examining effects year by year using an event-study design allows to explore the linearity of treatment effects – useful for thinking about mechanisms (a point we will return to later) (Kose, Kuka, and Shenav 2019).[41] Figure VII presents the visual display of these estimates. We observe little change in relative wages in the 10-year window prior to a county becoming VRA-covered – suggesting that outcomes in covered and uncovered were not following different trends prior to VRA coverage. Note, however, that the effects of the VRA emerge relatively soon after coverage takes effect, with treated counties experiencing a 5 percent reduction in the wage gap by five years post-coverage. The rapid improvements we observe are consistent with work by Donohue and Heckman (1991) and Card and Krueger (1992), who suggest that civil rights legislation (including the VRA) produced immediate benefits for black Americans. 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 that would be observed in future labor market cohorts.

In Appendix Table A.7, 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. 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. Finally, we estimate Equation 1 controlling only for county-by-race fixed effects, to show the effects are not present because our empirical model is fully saturated. In this case, the effects are even stronger.

5.3 Robustness of the Main Finding

In this subsection, we probe the strength of our main results. We highlight some of the main checks here, and discuss additional analyses in Appendix E.

[41] In particular, we estimate:

$$\log(Y_{ict}) = \sum_{t=-2}^{2} \mu_t \times [\text{VRA}_c \times \mathbb{I}\{r(i) = \text{Black}\}] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \tag{4}$$

where c, t, Y_{ict} reference county, Census year, and the same dependent variables as before. The parameters of interest are the four μ'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 the reference year.

5.3.1 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 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 V, we estimate our preferred specification separately for the 1965 and 1975 coverage rounds. Column (1) provides our benchmark estimate from IV. 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 also 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. State-level policy changes affect labor markets and may confound our estimated treatment effect. Using the relatively even variation in VRA coverage that existed within North Carolina (41 of its 100 counties were covered), we can allay concerns about unobserved institutional changes at the state level.[45] We thus focus on North Carolina

[42] Two provisions of the CRA were pertinent in our setting. Title II of the CRA was the provision that outlawed discrimination within places of public accommodation. Title VII of the CRA was the provision outlawing discrimination by employers.

[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. 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). Moreover, we also exclude political jurisdictions in New Hampshire, Michigan, and New York, due to small black samples. Arizona and Texas alone account for over 50 percent of the additional coverage.

[44] The set of states that contain control counties beginning in 1975 consists of California, Oklahoma, New Mexico, Nevada, and Utah.

[45] Covered N.C. counties were selected for protection based on their 1964 voter turnout rates. Those with turnout below 50 percent became covered by Section 5. We view this threshold as unlikely to have been chosen with the

(N.C.) as a single-state case study of the impact of the VRA on black labor market outcomes.[46] Column (4) of Table V presents the result 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. 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 1975 Amendments, these results corroborate our main finding.

5.3.2 Further Ruling Out Confounders

One concern for our analysis is that voter protection may change the composition of the population in covered counties, leading to changes in measured labor market performance. For example, 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-status black families are migrating to covered 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 VI 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 2017). 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 similar concern related to our empirical strategy is the possibility of spillover effects between VRA and non-VRA counties that may bias our estimate in either direction. If labor markets are integrated across pairs of counties, labor prices may equilibrate – leading to an underestimate of

coverage of any particular county in mind.

[46] In a study conducted concurrently with ours, Fresh (2018) documents voter turnout effects that are similar in sign and magnitude to our results on political participation.

[47] The direction of any migration-induced effect of the VRA is theoretically ambiguous. If in-migrants were substitutable with native black workers, the increase in supply would *dampen* our estimated effect of the VRA. On the contrary, if there was positive selection into migration (Boustan and Margo 2009), we may overestimate the effect of the VRA by analyzing black workers who positively select into VRA counties. Out-migration of whites ("white flight") may also exaggerate the magnitude of our finding (Boustan 2010).

any treatment effect. Alternatively, the positive selection of black workers who migrate or commute in response may lead us to overestimate the effect of treatment. This latter possibility would be consistent with an "unintegrated labor market," where commuting is possible.

We find no evidence for possible cross-border spillover bias. To demonstrate this, we examine whether the effects of the VRA are similar for matched-pair and "interior" counties. Interior counties refer to all counties within a given state that are contained (partially or fully) within our matched-pair sample, excluding the border counties – in other words, all counties that are interior to the matched-pair counties. Using both the primary and interior samples, we estimate the following spatial-differenced specification used in (Dube, Lester, and Reich 2010):

$$\log(Y_{ict}) - \log(\overline{Y_{ct}}) = \alpha + \beta(\text{VRA}_{ct} \times \text{Black}_{ict}) + (\mathbf{x}_{ict} - \overline{\mathbf{x}_{ct}})\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{r(i)t} + \epsilon_{icr(i)t} \quad (5)$$

In essence, β indicates the degree to which effects for border and interior counties are different. Results based on Equation 5 are presented in Table VII. The coefficient of interest, VRA × Black, is presented in Column (1) and is small in magnitude (roughly 1 p.p., statistically significant at the 5 percent level). To the extent that 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. Robustness checks for these results are presented in Table A.8.

Finally, in Appendix Table A.9, we also test explicitly whether there is differential migration within the matched-pair sample directly using DEC data on a person's place of residence five years earlier. The regression is similar to estimating Equation 3, but using as an outcome an indicator variable for whether a person left a covered jurisdiction for a non-covered jurisdiction (or vice versa). As Column (1) shows, net out-migration is actually declining in treated 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. While the net flow of black workers appears to be negative in VRA counties relative to white workers, black workers in VRA counties are still less likely to move overall the sum of VRA and VRA × Black. 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: How Black Political Power Affected the Labor Market

We interpret the results in Section 5 as the reduced-form effect of black Americans' political representation on economic status. This intervention reduced wage inequality by nearly 6 p.p. In this section, we examine mechanisms through which minority political power operates to improve labor

market outcomes.

6.1 Public Sector Employment

We first focus on a direct mechanism: the hiring of black workers within government. The VRA was signed into law against the backdrop of a rapid expansion of government size nationwide during the second half of the 20th century (Figure VIII) (Berry, Grogger, and West 2015). This seemingly secular growth of government opened up new job vacancies that were especially valuable to minority workers. Public sector employers provided better access to high-paying managerial and professional jobs than what was available to black workers in the private sector, where employment segregation and discrimination were present (Frazier 1957; Hout 1984; Katz, Stern, and Fader 2005; Laird 2017).[48] To illustrate the value of public sector employment, in Table VIII we present estimates of the 1960 public sector wage premium for both black and white workers within our sample. These results, based on estimating a simple OLS regression of log wages on a public sector dummy, demonstrate that black workers in the public sector earned substantially more than their private-sector counterparts, conditional on worker traits. Black government workers in the South earned 20 percent more than their private sector counterparts ($p < 0.01$). The premium is drastically lower, however, for white workers.

Given these relative benefits of working in the public sector, one way that the state could respond to black economic disadvantage was through government hiring. We thus show the change in black political power facilitated with the passage of the VRA contributed to the increased presence of minority workers within the public sector. Our findings are consistent with studies by Alesina, Baqir, and Easterly (2000) and Enikolopov (2014), which demonstrate how political considerations often influence public sector hiring practices. Minority political strength (as proxied by both minority candidates and voting strength) has long been considered an important determinant of minorities' representation within the bureaucracy (Eisinger 1982; Nye, Rainer, and Strat 2015). Moreover, the secular growth of government opened up new job vacancies for minorities without the need to displace current white workers (Krislov 1967). Public sector employment contributed to improvements in black Americans' labor market outcomes directly (by offering those employees better-paying jobs) and indirectly (by improving the outside option for *other* black workers). We demonstrate both channels.[49]

[48] 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). 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. State agencies were viewed as enforcers of nondiscrimination – unsurprisingly, given the ability of voters to punish discriminatory government agencies (King 2012).

[49] In Appendix B, we conceptualize in more detail the process through which a positive change in minority political power increases minority public-sector hiring, as well as how this increase in labor demand affects overall black wages, including within the private sector. This conceptual and empirically-validated account of minority politics and increased public employment is consistent with historical evidence (Wright 2013). Maynard Jackson, for example, became Atlanta's first black mayor in 1972 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

6.1.1 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.[50] To analyze whether the VRA increased the relative likelihood of a black worker being employed within the public sector, we modify our primary specification and estimate the following linear probability model:

$$\mathbb{I}(\text{Public Employee} = 1)_{ict} = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)t} + \epsilon_{icr(i)p(c)t} \quad (6)$$

The dependent variable is an indicator for whether an individual is employed in the public sector. β now indicates how the VRA changes the likelihood that black workers will be employed by the public sector, relative to white workers (the coefficient of interest is similar to Equation 1 above). Results are presented in Table IX, 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 percent level). Columns (2)-(5) demonstrate that this effect is stable to the inclusion of state trends and worker controls, and also that the results hold for the cross-border and NC-only samples. Overall, our analysis suggests that the VRA increased the likelihood that blacks would receive a public sector job by between 2 and 4 percent.[51] We can 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 A.12 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 is likely not a confounding factor for this result. OLS regression estimates show 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. We test for this in the Appendix, modifying our wage regression to account for heterogenous effects of the VRA on public and private-sector workers.[52] Results for this test of heterogeneity are presented in Table A.13. Summing up the coefficients on VRA $\times$ Black is 0.02, suggesting that

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minority hiring). However, the value of public-sector employment to black Americans in the South extends back even further. For example, 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 workers (Jackson 2007).

[50] 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.

[51] In the Appendix, we provide a number of robustness checks. For instance, Table A.11, shows that in absolute terms, black workers were also more likely to be employed in government.

[52] Specifically, we estimate the following specification:

$$\log(W_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \text{Public}_{ict}) +$$
$$\beta_3(\text{VRA}_{ct} \times \text{Public}_{ict}) + \beta_4(\text{VRA}_{ct} \times \text{Public}_{ict}) + \beta_4\text{Public}_{ict} +$$

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 racial disparities even *within* the public sector.

6.1.2 Minority Public Sector Hiring and Overall Minority Wages

We have thus far shown that the VRA (1) improved the overall labor market status of black workers in terms of relative wages, (2) improved the likelihood that a black worker would be employed in the public sector (which commanded a higher wage than working in the private sector), and (3) improved wages within the public sector for black workers. A given local labor market's public and private sectors do not function in isolation from one another. To the extent that the VRA created a positive "demand shock" for black workers in the public sector, one might expect some degree of upward pressure on the private wages of black workers. As such, in this subsection we establish the connection between improved performance of black workers in the public sector and improved economy-wide wages. We establish this connection in two steps. We first 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. We then calculate 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.

We first provide 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). Ideally, we would use exogenous changes in local public-sector labor demand across VRA and non-VRA counties to examine how the magnitudes of our main findings changes. Lacking this type of variation, we instead leverage inter-occupation heterogeneity in the exposure of private-sector employers to competition from government for the labor supplied by black workers. Specifically, we test whether black workers in occupations that experience greater public-sector growth (a proxy for increased public labor demand) over the sample time period (1960-1980) also observe differentially greater wage gains.[53] The intuition for this test of heterogeneity is that the public-sector channel of private-sector wage improvement will be strongest in occupations where there are more governmental job vacancies (that can be reallocated to minority workers). These are the jobs where private firms face the largest increase in competition for black workers.

We create the proxy for (cross-occupational) public-sector demand by sorting occupations into

$$+ \mathbf{X}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icr(i)p(c)t} \quad (7)$$

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.

[53] Not all occupational categories are populated across years. We restrict our sample to workers in occupations present in 1960, 1970, and 1980. VRA does not predict selection into this subsample.

quartiles of national public-sector employment growth between 1960 to 1980 (the fraction of workers who work in the public sector within a given occupation).[54] Logistically, we first define public sector growth by occupation as follows:

$$\Delta PubSec_{o,-i} = \%PublicSector_{o80,-i} - \%PublicSector_{o60,-i} \tag{8}$$

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.[55]

If the public sector is a channel that contributes to *overall* improvement in black workers' wages, one would predict greater convergence in the top quartile of public-sector growth. As Table X 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 percent 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 9, but interacting VRA $\times$ Black with a continuous measure of sector growth by occupation, rather than using quantiles. The effects are similar.

Having highlighted how the public sector can raise minority income in both the public and private sectors, we decompose how much of our average treatment effect on relative wages 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

[54] We sort occupations using the Census 1950 occupational classification system.

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

$$\begin{aligned}\log(\text{Y}_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \Delta\text{PubEmp}_{cq1}) \\ +\beta_3(\text{VRA}_{ct} \times \text{Black}_{ict} \times \Delta\text{PubEmp}_{cq4}) + \beta_4(\text{VRA}_{ct} \times \Delta\text{PubEmp}_{cq1}) + \beta_5(\text{VRA}_{ct} \times \Delta\text{PubEmp}_{cq4}) \\ +\beta_6(\text{Black}_{ict} \times \Delta\text{PubEmp}_{cq1}) + \beta_7(\text{Black}_{ct} \times \Delta\text{PubEmp}_{cq4}) \\ + \beta_8\Delta\text{PubEmp}_{cq1} + \beta_9\Delta\text{PubEmp}_{cq4} + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t}\end{aligned} \tag{9}$$

The identifying assumption in this test for heterogeneous effects 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. That is, there is no factor that simultaneously: (i) differentially affects blacks relative to whites, (ii) differentially affects VRA counties at the border, (iii) has differential effects over time similar to the VRA, (iv) affects occupations with high national public-sector demand growth, and (v) 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 related to 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.

consist of more than the mechanical effect of having a higher-paying government job. Beaudry, Green, and Sand (2012) demonstrate that accounting only for direct effects may underestimate the true effect of changes in the public sector due to the VRA – due to the existence of spillover effects to the private sector. To account for these effects, we consider a labor market model with public employment à la Mortensen-Pissarides, described in detail in Appendix B.

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, as we have done in previous sections. In the first step, the estimating equation we use is given by Equation (10), 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:

$$\begin{aligned}\log(Y_{ict})^{Private} &= \alpha_0 + \alpha_1 Black_{ict} + \alpha_2 \overline{PubEmp}_{ct,black} + \alpha_3 \overline{PubEmp}_{ct,white} \\ &+\alpha_4 \overline{PubEmp}^2_{ct,black} + \alpha_5 \overline{PubEmp}^2_{ct,white} + \alpha_6 \overline{PubEmp}_{ct,black} \times \overline{\log(W_{ct,black})}^{Public} \\ &+\alpha_7 \overline{PubEmp}_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_8 \overline{PubEmp}^2_{ct,black} \times \overline{\log(W_{ct,black})}^{Public} \\ &+\alpha_9 \overline{PubEmp}^2_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_{cont} X_{ict} + \epsilon_{i,c,p(c),t}\end{aligned} \tag{10}$$

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:

$$\frac{Var\big(\beta_1'(VRA_{ct} \times Black_{ict})\big)}{Var\big(\beta_1(VRA_{ct} \times Black_{ict})\big)} \tag{11}$$

We find that changes in public sector hiring explain between 29 and 35 percent of the reduction in the private-sector wage gap following the VRA.

6.2 Anti-Discrimination and Affirmative Action Regulations

Government jobs typically comprised a minority of a given area's labor force, and as such, it is unlikely that this mechanism could entirely account for our observed treatment effect. Existing research suggests ways in which political power could affect black economic outcomes through *direct* intervention in the private-sector labor market.

Perhaps the most important factors for the abatement of racial wage inequality were anti-discrimination and affirmative action policies (Donohue and Heckman 1991; Chay 1998). Title VII of the CRA outlawed employer discrimination in pay, hiring, and promotions on the basis of race. Affirmative action policies (AA) – adopted at all levels of government – encouraged (or even required) minority hiring. For example, beginning in 1965 Executive Order 11246 required that federal government contractors maintain AA plans that explicitly outlined a contracting firm's minority and women employment goals. Firms with unacceptable plans were barred from future federal contract bidding. The federal agency responsible for enforcing legal requirements and ensuring workplace equality was (and remains today) the Equal Employment Opportunity Commission (EEOC). The EEOC in the mid-1960s possessed the authority to investigate and negotiate complaints of discrimination by private establishments.

We examine whether the VRA complemented anti-discrimination and affirmative action laws, due to the improved likelihood of enforcement. This prediction is based on research within labor history and sociology suggesting that the implementation of anti-discrimination policy was historically a "politically mediated" process, dependent on political actors (Stainback, Robinson, and Tomaskovic-Devey 2005). From 1966 through the early 1970s, for example, the EEOC investigated nearly 80,000 complaints of employment discrimination, filed in the South primarily by political activist groups such as the NAACP (Minchin 2015).[56]

There exists little granular, systematic data on either affirmative action policies or Title VII enforcement.[57] 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.

To test for the existence of a policy enforcement channel of the VRA, we construct a county-level measure of local workforce exposure to the mandates of federal anti-discrimination policies. We use historical data on the fraction of a county's manufacturing establishments subject to federal monitoring from the 1962 U.S. County Business Patterns. Research by Carrington, McCue, and Pierce (2000) suggests that anti-discrimination and affirmative action policies regulating the labor market were less well-enforced for small employers. This approach uses 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

[56] We do not take a 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.

[57] 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.

exposure to CRA enforcement within a given county (details about our construction of the estimated exposure of a county's workforce to civil rights legal mandates is discussed in Appendix C).

Table XI presents the results of this test for heterogeneous effects by county-level exposure to federal anti-discrimination legislation. The hypothesis we are testing is whether black political empowerment augmented the effectiveness of Title VII (as measured by the *ex-ante* enforcement likelihood). 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 (limiting to manufacturing workers) on relative black wages are larger in counties that are arguably more-exposed to 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 Title VII Exposure is only meant to be a proxy for the presence 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 the breaking down of labor market segregation.[58] In Column (3) of Table XI, 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. 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 find support for this possibility, which is discussed at length in Appendix D.

At the state and local levels, minority political pressure also led governments to enact their own policies to improve the labor market status of blacks (and racial minorities generally). For example, a more common form of affirmative action conditioned state contracts on the employment of women and minorities (Nay and Jones 1989; Santoro and McGuire 1997). Other policies at the state and local level 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]

We are unaware of any comprehensive data source on local and state AA programs. Existing studies, however, suggest that local-level AA programs are more likely to be enacted in cities where an elected mayor has primary executive authority within city government, rather than an appointed

[58] 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.

[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)).

city manager. Because elected mayors are more vulnerable to electoral pressures than appointed bureaucrats, they are more likely to use AA policies (and by extension, contracts) as a form of redistribution. In the next section, we will use geographic variation in a city's form of government (mayor vs. city manager) as a source of treatment effect heterogeneity to demonstrate that the effects of the VRA on black wages are stronger in those localities where one would expect minority electoral power to influence how politicians govern.

6.3 Changes in Human Capital

Finally, we analyze whether improvements in skills are a channel through which voting and political influence translated into the improved earnings prospects of black Americans. Several studies have documented how franchise expansions in the U.S., including both the VRA and state-level laws extending voting rights to women, have led politicians to increase spending on education and health (Miller 2008; Cascio and Washington 2014; Kose, Kuka, and Shenav 2019). An implication of these studies is 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 VII shows, the effects of the VRA are apparent within five years of voting rights 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 that would have been observed in the labor force with a longer time lag).

We directly test this channel using DEC data on workers' educational attainment (Table A.15). First, we detect no statistically significant effect on black-white worker education gap (as measured by the highest level of education a worker achieves) (Columns (1)-(3)). 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 (Columns (4)-(6)). If the VRA was affecting wage inequality through its effect on human capital, then one might expect the interactions of VRA and human capital traits to absorb some of 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. (main results remaining statistically significant at the 5% level). Finally, in Columns (7) and (8), we find no discernible effect of the VRA on the composition gap of minority workers that have either high school or college.

7 How Does Minority Political Power Operate?

The previous discussion of channels focused on the labor market mechanisms (supply and demand) that gave rise to improvements in wage equality. We have conducted relatively little exploration, though, of how the VRA changed politics itself in a way that benefited black workers. In this final section, we examine these political channels, providing further confirmation that the VRA was an independent contributor to the labor market improvements of the civil rights period.

7.1 Spatial Competition

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).

Theories of spatial competition suggests that black enfranchisement via the VRA would have increased public expenditures for black communities (Lindbeck and Weibull 1987; Cox and Mccubbins 1986). As Cascio and Washington (2014) point out, these models of distributive politics suggest that larger post-VRA shifts in economic status should occur counties with higher black population shares in treatment areas that in control counties. The intuition is that counties in which 40% of the voting population is black, politicians will be more likely to respond to the redistributive demands of black voters than counties where only 5% of the electorate is black. If black constituents vote cohesively, then local politicians would face stronger electoral incentives to attend to the policy needs of this voting block. These needs often include the distribution of resources, including greater support in terms of employment, contracts, and other policies.

Table XII provides evidence suggestive of this mechanism, in the spirit of Cascio and Washington (2014). The coefficient on the term VRA × Black × %PopBlack is significant and positive, suggesting that black workers benefited from political power relatively more in jurisdictions where there they could exert more pressure on elected officials. We also test for nonlinearities in the heterogeneous strength of minority voting power by dividing counties into 10 % bins by black population shares. It is possible, for example, that black constituents' ability to affect policy jumps discretely at or around 50% (when it becomes a majority within a given city – and so in principle can elect all politicians who elected are elected citywide).

We thus estimate the following regression for heterogeneous effects of the VRA on black relative wages:

$$\log(Y_{ict}) = \sum_{j=1}^{5} \alpha_j \left[\%\ \text{Pop. Black}_c = j\right] \times \left[\text{VRA}_{ct} \times \text{Black}_{ict}\right] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \tag{12}$$

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 IX. 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_3, and α_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 power of black Americans tipping sharply in places where they comprise a majority of the electorate. 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). We also note that these results are suggestive that local government matters.

7.1.1 Hetereogeneity by Local Government Structure

We also probe the distributive politics channel using locality-specific information on government structure. Specifically, we show that our measured effects are stronger in cities where the chief executive faces greater electoral incentives.

City governments in the United States come in two main forms: (1) the mayor-council form, where the mayor is elected directly by the city and functions as the chief executive of the government; and (2) the council-manager form, where the legislative and executive functions of government fall to the city council, which may appoint a city manager to administer city services and determine the composition of the bureaucracy. The former governmental setup concentrates powers in the hands of the mayor, who is elected and thus cannot removed by the city council. One may hypothesize that because mayors face reelection incentives, they are more likely to engage in targeted redistribution for political gain – while on city managers, on the other hand, are appointed bureaucrats and are thus less likely to engage in politically-motivated redistribution (perhaps due to stronger career concerns). Recent empirical evidence lends credence to this prediction, *in particular* with respect to public sector employment. Enikolopov (2014) demonstrates, for example, that a greater fraction of workers are employed by the public sector in cities with elected mayors rather than city managers.

We adopt a similar strategy, using mayor-council local government as a source of heterogeneity to test whether the benefits of minority political power are larger in those cities where the chief executive was elected. The directional effects of this test are presented in A.16 – the regression estimates are currently under embargo at the U.S. Census Bureau (awaiting disclosure review). The results suggest that both the wage improvements and public employment gains that black Americans achieved post-VRA were differentially larger in mayor-led cities. These results augment the work of Enikolopov (2014) but suggesting that government and political structure affect not just the size but the *composition* of the public sector workforce. The differentially larger effect

of the VRA on wages in mayor-run cities is also consistent with existing research suggesting that local affirmative action policies that improve minority labor market outcomes are more likely to be enacted in mayor-council cities (Santoro and McGuire 1997).

7.2 Descriptive Representation

Identity-based or "citizen-candidate" models provide a plausible alternative (though not mutually exclusive) theoretical channel through which the VRA might have affected redistribution. Minority enfranchisement may have increased the presence of minority politicians, who in turn implemented the preferred policies of the group (such as hiring of minorities within government jobs, or the provision of government contracts). Recent work by (Beach, Jones, Twinam, and Walsh 2018), suggests that black politicians can improve the quantity and quality of public goods in black communities.[60] 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 limited *causal* evidence on whether the increase in black office-holding was due to the passage of the VRA, and perhaps more importantly, whether descriptive representation improved the substantive representation, which might become manifest through improved minorities' socioeconomic outcomes. 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 this end, 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]

Table XIII provides results from estimating the impact of VRA coverage on the presence of black elected officials within a county (using several measures, both for the border pair and full county sample). The results clearly indicate an increase in the number of minority elected officials,

[60] A rich literature within development economics has also considered the effects of descriptive representation. 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.

[61] In the process of conducted our study, we were pointed to an excellent new working paper by Bernini, Facchini, and Testa (2018), who also 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).

as might be expected.

Given data constraints, we cannot separate the mechanisms of descriptive representation and distributive politics. We do not believe, though, that descriptive representation is the primary political mechanism at work in this setting. As previous research has pointed out, the number of black politicians holding office did not change overnight. Rather, the increase was gradual – unlike the changes in employment outcomes that we observe. Based on the American politics literature, we believe that counties that had sufficiently large minority populations as early as 1960 were more likely to ultimately elect minority candidates, and as the previous subsection highlights, also produce benefits for their communities. We demonstrate this in Table A.17. In summary, although the results in this section are primarily suggestive (i.e., we cannot effectively rule all correlated unobservables for these tests of heterogeneity), we believe (cautiously so) that the empirical evidence is consistent with models of distributive politics as argued in Husted and Kenny (1997) and Cascio and Washington (2014).

8 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 VRA 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 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.

Appendices

A Additional Evidence of Post-VRA Government Responsiveness to Black Voters' Interests

In this section, we provide further evidence that the VRA affected policy outcomes in favor of black voters – in this case, at the local levels. Table A.5 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).[64] Prior to the mid-1960s, black Americans were often denied access to social programs. 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. During Congressional debates that led 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 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 A.5). 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.

[64] 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.

B Conceptual Framework

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.

B.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, V, obeys:

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

while the value of a filled vacancy, J, follows:

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

where w are the wage flow paid to the worker. 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, W, and unemployment, U, 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:

$$w = \underbrace{\frac{\gamma\beta}{1-\beta} \frac{r + \delta}{q(\theta)}}_{\text{Surplus Distribution}} + \underbrace{\frac{r + \delta}{r + \delta + m_g} \Big(\overbrace{b}^{\text{Reservation Utility}} + \overbrace{\theta \frac{\gamma\beta}{1-\beta}}^{\text{Private Sector Gains}} \Big) + \frac{m_g}{r + \delta + m_g} \overbrace{w_g}^{\text{Public Wage}}}_{\text{Social Planner Component}} \tag{15}$$

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, which we label social planner component. The latter component can be thought of as a weighted average of the public wage on the one hand, and reservation utility and private sector gains in proportion to the tightness of employment conditions (demand premium) on the other, where the weights are determined through public hiring.

B.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:

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

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

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

From conditions *(i)* if there is an increase in black workers' political strength, the politician needs to increase black public hiring, m^b_g, relative to white public hiring, m^a_g, 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:

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

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:

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

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.

C Constructing County-level Measure of Exposure to Civil Rights Act-Title VII & Federal Affirmative Action Requirements

As we discuss in Section5, there is currently no readily-available data that allows us to examine how political influence due the VRA improved the enforcement of anti-discrimination policies within the private labor market, such as Title VII of the Civil Rights Act. To indirectly test this hypothesis, we leverage previous research documenting that federal anti-discrimination and affirmative policies were better-enforced within establishments that were large enough to fall under the more stringent oversight of the EEOC. In particular, the Equal Employment Opportunity Act (EEOA) of 1972 extended civil rights coverage to employers with 15-24 employees. We use data on establishments both above and under 20 employees drawn from the U.S. County Data Books to estimate the probability of workforce exposure to CRA enforcement within a given county. Because we cannot exactly observe the number of establishments subject and not subject to the amended Title VII, we estimate the likelihood of exposure using the following 3-step methodology.

(1) First, we create estimates of the probability of a worker being in a small-to-medium establishment (SME - less than 100 employees). Assuming an uniform distribution for establishment size for SMEs and setting total employment equals the expected value of employment from that distribution, we can retrieve the proportion of small and medium establishments subject to EEOA. For estimating the parameters of the distribution we use a sample with no large establishments.

(2) Second, using this distribution, we estimate the number of medium establishments subject ot the 1972 EEOA (between 20 and 25 workers), and similarly the number of small establishments subject to the EEOA (between 15 and 20 workers). From (1), we obtain that the probability of being a small firm conditional being an SME (less than 100 employees) is 12.3%. The probability of being a medium firm conditional on being an SME is 15.2% . Also from (1), small firms constitute 82% of all SMEs, and medium firms the remaining 18%. This means that 15% of all small firms and 84% are affected by the EEOA.

(3) Finally, we define our CRA penetration measure (TitleVIIExposure in Table XI) as:

$$TitleVIIExposure = \frac{\text{estimated number of wokers in firms affected}}{\text{total number of workers}}.$$

D 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 *62* 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.[65]

[65] The estimating equation is thus:

$$\log(OccScore_{ict}) = \beta_0 + \beta_1(VRA_{ct} \times White_{ict}) + \mathbf{x}_{ict}\boldsymbol{\gamma} + (\delta_c \times \delta_t) + (\delta_r \times \delta_c) + (\delta_{p(c)} \times \delta_r \times \delta_t) + \epsilon_{i,c,p(c),t} \quad (16)$$

Results are presented in Table A.14. 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 (results available upon request). 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.

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.

E Additional Tables

See below (following main tables)

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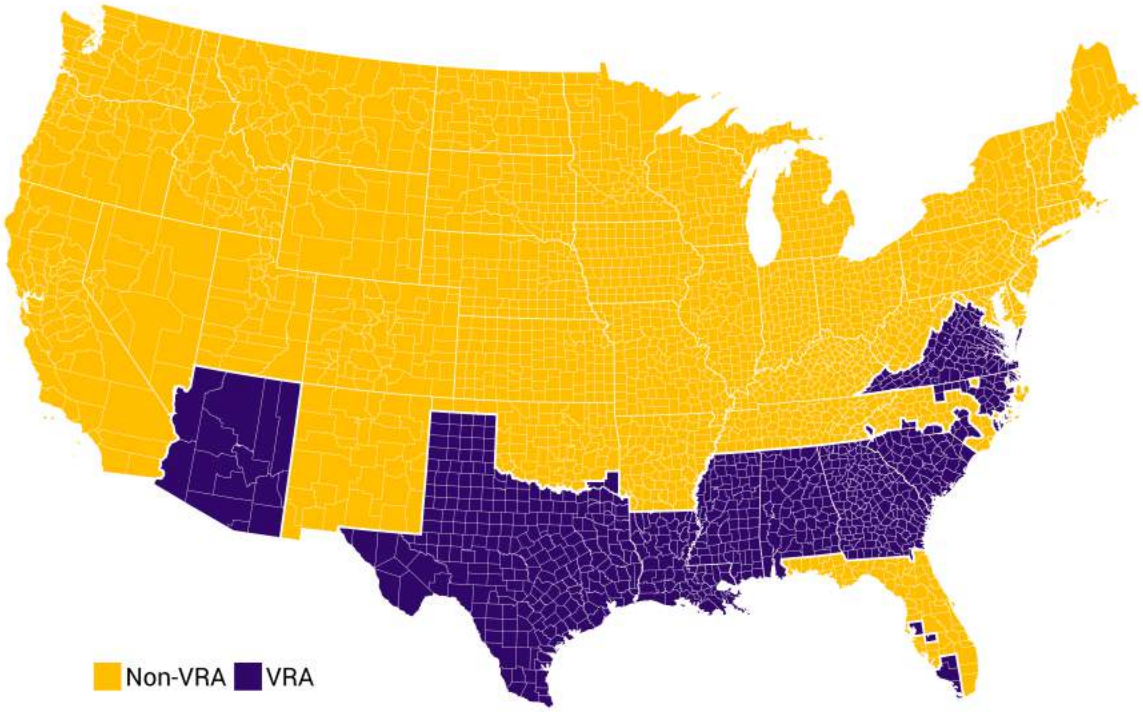
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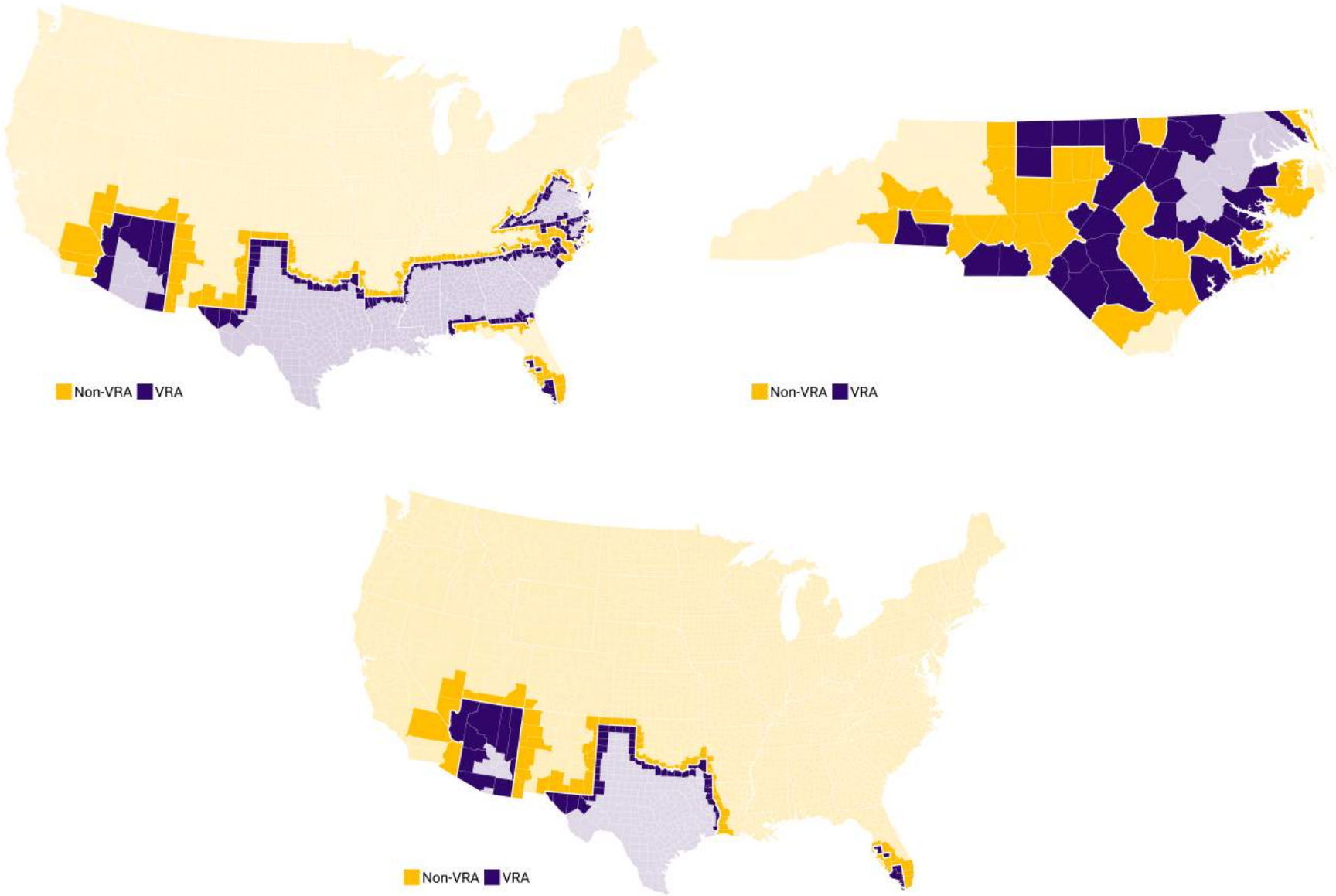
Figures

Figure I: U.S. Counties by VRA Section 5 Coverage



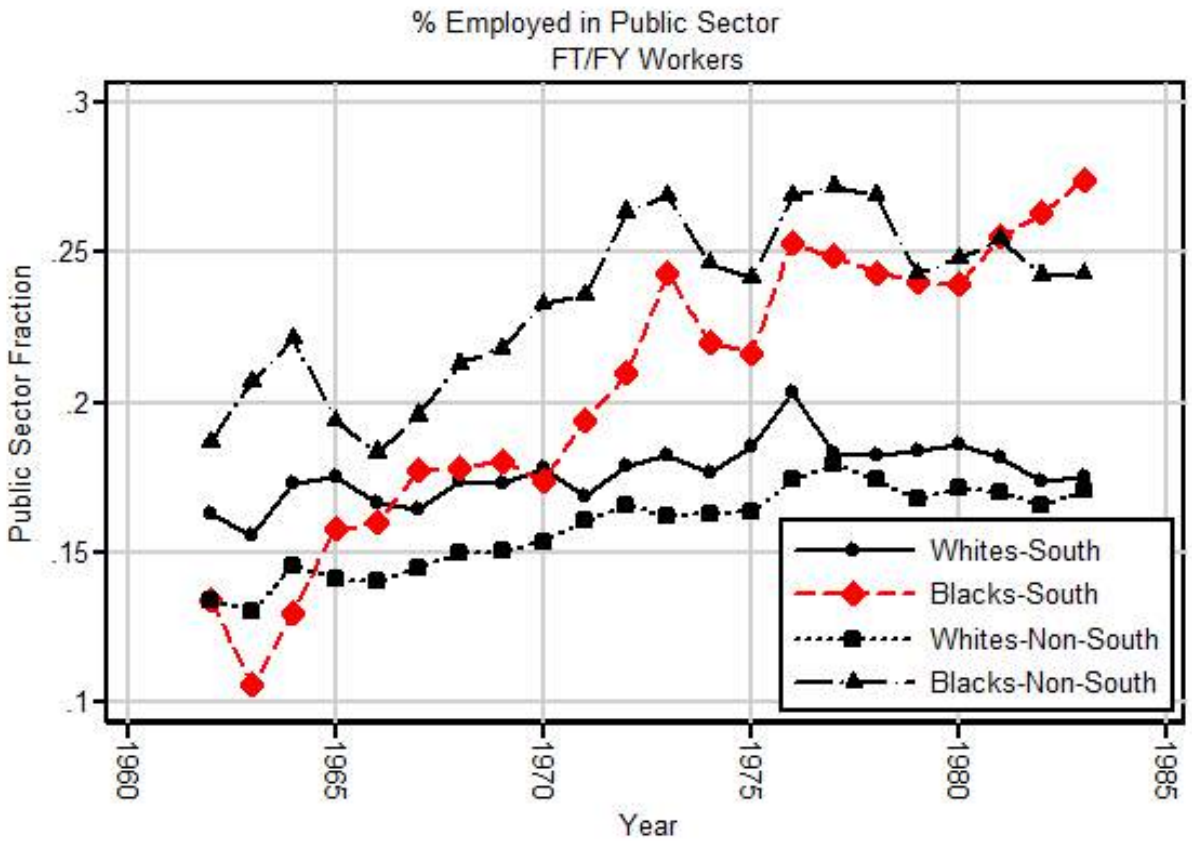
NOTES: Figure indicates all counties that were/were not covered by Section 5 of the 1965 Voting Rights Act (VRA) during the period of analysis. Source: U.S. D.O.J.

Figure II: VRA Covered Jurisdictions



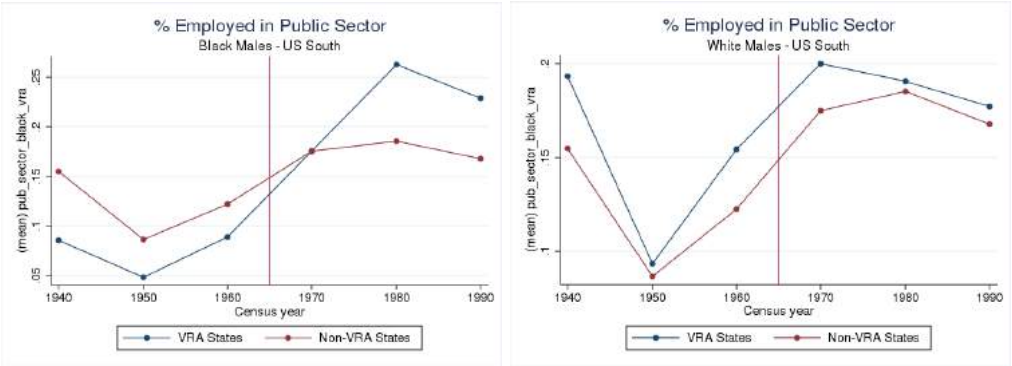
NOTES: Figure presents the main sample of counties used for analysis, as well as the primary two subsamples used to demonstrate the robustness of empirical findings. Top left panel indicates full matched county pair sample, top right panel indicates NC-only matched county pair sample, and bottom panel indicates 1975 VRA Amendments matched county pair sample.

Figure III: Public Sector Workforce - By Race



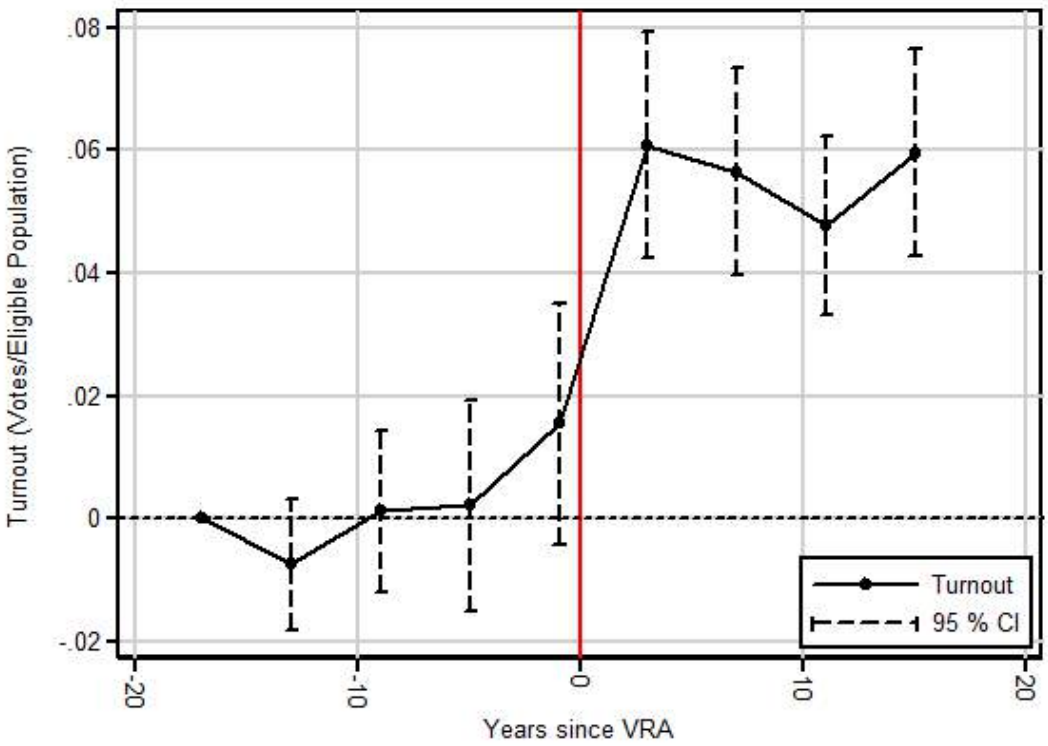
NOTES: Figure presents the fraction of full-time, fully-year workers employed as public workers, according to the Current Population Survey (CPS), by race and region. Source: CPS.

Figure IV: Public Sector Employment by Race



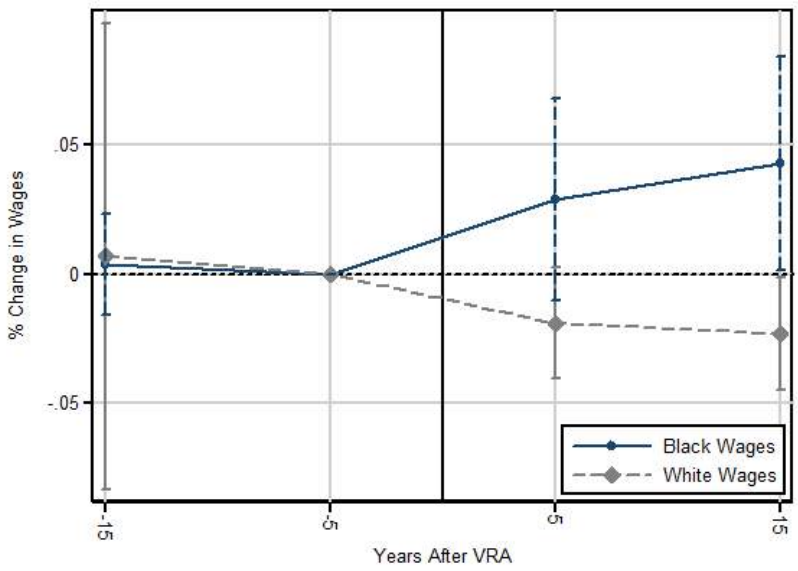
NOTES: Figure presents trends in the fraction of adults employed in the public sector by VRA coverage status, for both black and white workers. See text for details. Source: IPUMS DEC.

Figure V: Impact of the VRA on Presidential Turnout
(Heterogeneous Effects by % County Black)



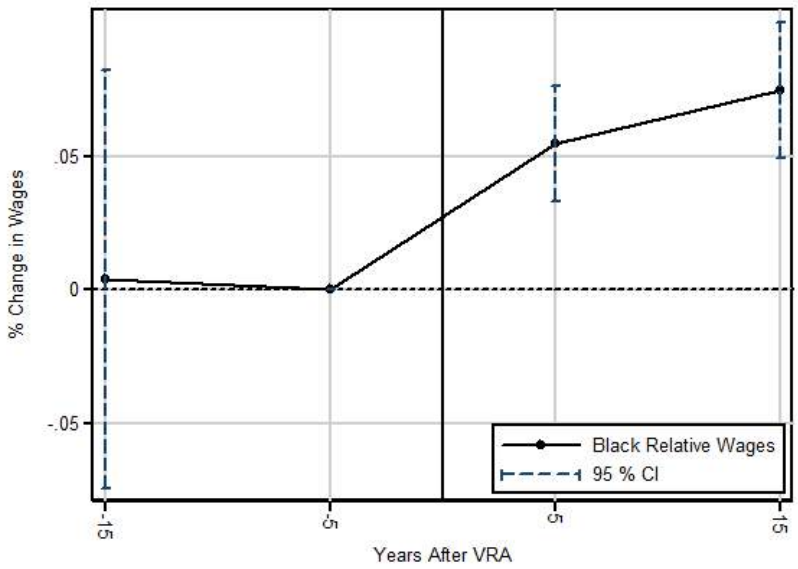
NOTES: Figure presents event-time estimates of how VRA coverage interacted with pre-VRA black population relates to voter turnout. The dependent variable (y-axis) is the voter turnout in presidential elections, and the independent variable (plotted) is the interaction between between the ever-VRA-covered indicator, a year indicator, and the pre-VRA percentage of the population that is black within a county (standardized, mean 0). Vertical bars provide 95% confidence intervals. 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. Source: County Data Books and presidential turnout data from Gentzkow, Shapiro, & Sinkinson (2011).

Figure VI: Impact of the VRA on Wages
(by Race)



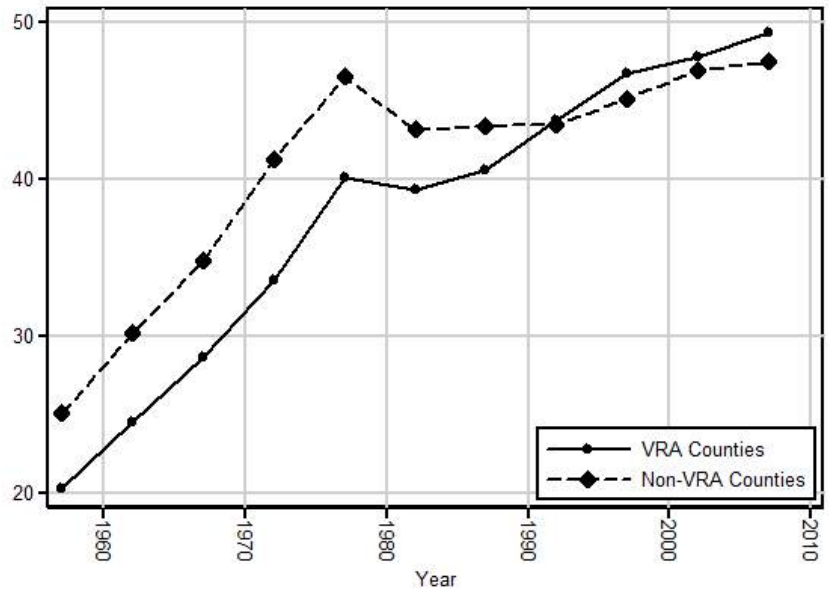
NOTES: Figure presents event-time estimates of how VRA coverage affects wages for black and white workers separately. Regressions include education and experience controls, county and county pair-year fixed effects, and baselines controls interacted with linear and quadratic trends. Estimates are normalized to five years prior to VRA coverage taking effect. Source: DEC.

Figure VII: Impact of the VRA on the Black-White Wage Gap: Event Study Estimates



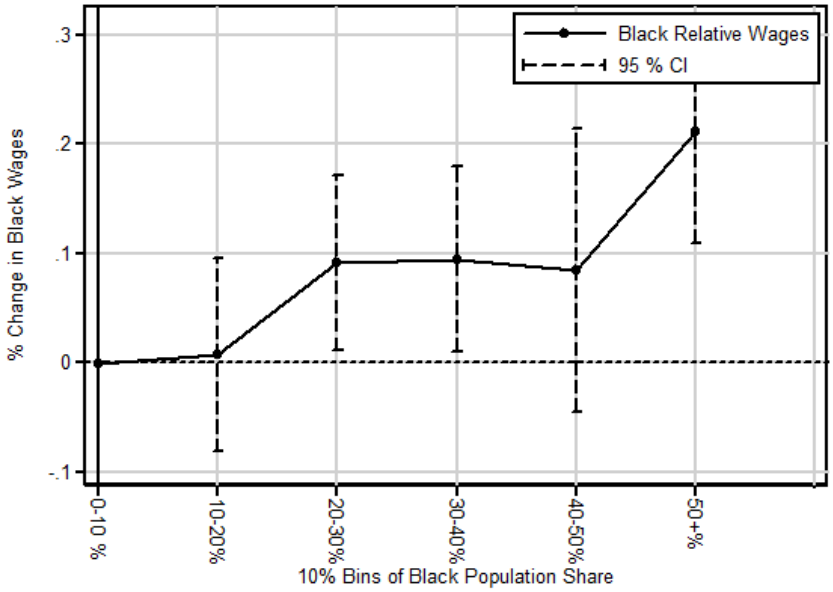
NOTES: Figure presents event-time estimates of how VRA coverage affects black relative wages. Regressions include education and experience controls, county and county pair-year fixed effects, and baseline controls interacted with linear and quadratic trends. Estimates are normalized to five years prior to VRA coverage taking effect. Source: DEC.

Figure VIII: 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, restricting to counties with populations of larger than 10,000. Source: U.S. Census of Governments (COG).

Figure IX: 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 presents the OLS regression coefficient of the interaction between the primary explanatory variable of interest (VRA × Black) and a dummy variable for whether a respondent resides in a county with a given level of black population share indicated by the X-axis. Source: U.S. DEC.

Tables

Table I: Baseline Characteristics & Trends, by VRA Status

		Neighboring County-pair Sample			Total County Sample	
	Pre-VRA Mean	(1)	(2)	Pre-VRA Mean	(3)	(4)
Demographic Characteristics						
Population	46582.34	-0.120	0.065	82111.06	0.043	0.053
		(0.279)	(0.754)		(0.404)	(0.771)
Pop. Density	191.28	-0.012	0.058**	151.42	-0.029	0.004
		(0.111)	(0.956)		(0.575)	(0.981)
% Urban	31.87	0.183*	0.279	29.95	0.255**	0.256
		(0.110)	(0.174)		(0.000)	(0.158)
% Farmer	19.85	-0.009	0.325	21.12	-0.067	0.326**
		(0.111)	(0.097)		(0.193)	(0.047)
% Nonwhite	19.60	-0.011	-0.164	19.42	-0.136***	-0.042
		(0.111)	(0.396)		(0.009)	(0.806)
% H.S.-educ. adults	29.83	-0.121	-0.237	28.34	0.482**	-0.320*
		(0.110)	(0.240)		(0.000)	(0.061)
Economic Characteristics						
% Employed FT	69.34	-0.131	-0.128	69.91	-0.477***	-0.023
		(0.235)	(0.502)		(0.000)	(0.884)
Median Income	3732.60	0.059	-0.267	3509.47	-0.028	-0.395**
		(0.596)	(0.187)		(0.584)	(0.025)
% Construction	2.28	-0.173	-0.066	2.33	.011	0.017*
		(0.118)	(0.752)		(0.705)	(0.926)
% Manufacturing	19.54	-0.101	-0.306	19.20	-0.170***	-0.330*
		(0.363)	(0.121)		(0.001)	(0.054)
% Trades	5.38	0.115	0.077	5.30	-0.027	0.084*
		(0.299)	(0.713)		(0.595)	(0.644)
No. of Counties	329			1511		
State FE			X			X

Notes: This table presents regression coefficients from 4 separate regressions, one per column. Each column reports estimates of ordinary least squares regressions relating coverage future under the VRA to the difference in a givern county characteristic. An observation is a county-year. The dependent variable is the difference between county characteristics between 1950 and 1960, where each outcome is standardized to be mean 0, standard deviation 1 in a given year. The independent variable is an indicator for future VRA coverage. P-values are provided in parentheses. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: County Data Books (based on US Census estimates).

Table II: The Effect of the VRA on Political Participation and Representation

	Panel A: Voter Turnout (Presidential Elections)				Panel B: Legislator Ideology (DW Nominate Score)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	0.115***	0.079***	0.063***	.042***	-0.08*	-0.06*	-0.04
	(0.010)	(0.007)	(0.007)	(0.010)	(0.04)	(0.03)	(0.04)
VRA × Black Pop. Share				0.002***			-0.12
				0.000			(0.15)
Unit	County	County	County	County	District	District	District
N	2651	2651	2651	2651	1699	1699	1699
Controls		X	X	X		X	X
State Trends			X	X			

Notes: This table presents regression coefficients from 7 separate regressions, one per column. Observations in Columns (1)-(4) are county-year, and observations in Columns (5)-(7) are congressional district-year. The dependent variable is county-level turnout in presidential elections in Panel A. The dependent variable is the second dimension of the Poole-Rosenthal DW-Nominate Score, which indicate conservativeness on race-related issues. The independent variable is a dichotomous variable indicating whether a given county or Congressional district 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. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. District-level controls include the fraction of the district population that is black. All regression include year/Congress and county/district fixed effects. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table III: The Effect of the VRA on 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 6 separate regressions, 3 separate regression estimates per column, 2 regression estimates per row. Each column-row cell contains an estimate of an ordinary least squares (OLS) regression relating Voting Rights Act coverage to absolute wages by race (in Panels A and B), as well as relative wages (Panel C). An observation is an individual in a given Decennial Census year. The dependent variable is the log hourly wage, and the independent variable is either VRA (an indicator variable for whether is VRA-covered in a given Census year), or 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). Regressions in Panels A and B include county and county pair-year fixed effects. Regressions in Panel C include county-race, county-year, and county pair-year-race fixed effects. Standard errors are in parentheses and are clustered by county. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table IV: 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. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table V: 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. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table VI: 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. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table VII: 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 include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. Regressions control for race-specific returns to human capital. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table VIII: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: IPUMS Decennial Census

Table IX: 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 is 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table X: 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 × ΔPubEmp$_{60-80,Q1}$	-0.078**	-0.101**
	(0.03)	(.046)
VRA × Black × ΔPubEmp$_{60-80,Q4}$	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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table XI: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table XII: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table XIII: The Impact of the VRA on the Election of Black Politicians, 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: JCPES.

Appendix Tables

Table A.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 A.2: The Effect of the VRA on Political Participation (All Counties in Sample States)

	(1)	(2)	(3)	(4)
VRA	.148***	.098***	.066***	.019***
	(0.005)	(0.004)	(0.003)	(0.005)
VRA × Black Pop. Share				0.002***
				(0.000)
N	12848	12848	12848	12848
Controls		X	X	X
State Trends			X	X

Notes: This table presents regression coefficients from 4 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.

Table A.3: The Effect of the VRA on Political Participation
(Turnout for Congressional Races)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VRA	0.120***	0.092***	0.058***	0.153***	0.101***	0.098***	0.065***
	(0.010)	(0.009)	(0.009)	(0.005)	(0.005)	(0.012)	(0.018)
VRA × Black Pop. Share						0.001***	0.001
						(0.000)	(0.001)
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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details.

Table A.4: Impact of the VRA on Legislator Ideology (Republican vs. Democratic)

	(1)	(2)	(3)
VRA	-0.05*	-0.02	0.02
	(0.03)	(0.02)	(0.03)
VRA × Black Pop. Share			-0.21*
			(0.11)
N	1699	1699	1699
DW-Nom. Dimension	1	1	1
Controls		X	X

Notes: This table presents regression coefficients from 3 separate regressions, one per column. An observation is a congressional district-year. The dependent variable is Dimention 1 of the Poole-Rosenthal DW-Nominate Score (while Dimension 2 indicates conservativeness on race-related issues, Dimension 1 indicates overall partisan conservativeness), 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.

Table A.5: Impact of the VRA on Policy Responsiveness (Expenditures), 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. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Sources: County Data Books, 1944-1977; U.S. Census of Governments

Table A.6: 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. Each estimate is based on an OLS regression relating the VRA to black (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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.7: 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 VRA to 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 either a VRA dummy, or 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 only the 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.8: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC.

Table A.9: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.10: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.11: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC.

Table A.12: 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.13: 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)
N	673000	673000	673000
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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.14: The Effect of the VRA on Occupational Upgrading

	Outcome Variable: Occupational Income Rank		
	(1)	(2)	(3))
VRA × Black	0.053*	0.053*	0.055**
	(0.033)	(0.033)	(0.032)
N	673000	673000	673000
County-level Controls	X	X	X
Educ./Exp. Controls		X	X
VRA × Educ./Exp. 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 VRA to (relative) black wages. An observation is an individual in a given Decennial Census year. The dependent variable is the log occupational income score, as calculated in Collins (2003). 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. County-level controls include the employment rate, the adult population fraction with a high school education, the population fraction residing in urban areas, the adult population fraction working in agriculture, and median household income. Controls are measured at 1960 levels and interacted with linear and quadratic time trends. Regressions control for race-specific returns to human capital. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.15: 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 ins 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"). ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table A.16: Effects of the VRA on Relative Wages, by Local Government Structure

	(1)	(2)
Black × VRA × Mayor-Council Govt.	+0.xxx***	+0.xxx***
	(0.xxx)	
Black × VRA	+0.xxx***	+0.xxx***
	(0.xxx)	(0.xxx)
Outcome	Log(Wage)	Public Employment

Notes: This table presents regression coefficients from 2 regressions. 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 coverage 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. 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC and ICMA.

Table A.17: Effects of the VRA on Black Mayorship,
by Black Population Share, 1960-1980

	(1)	(2)
VRA	0.001	0.009
	(0.016)	(0.007)
VRA × %Black$_{over50\%}$	0.074	0.056***
	(0.054)	(0.017)
Sample	Border	Interior
N	810	3750
County-level Controls	X	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 the elction of a black mayor within a given county. The dependent variable an indicator that equals 1 if there is a black mayor in a given county in years 1960, 1971, and 1980 (pooling all cities within a county). The independent variable of interest is the interaction between the county-level black population share and the VRA indicator. 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. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Source: DEC and JCPES.