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

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Abstract

This paper provides novel empirical evidence on the instrumental value of the franchise for socioeconomically-disadvantaged minorities. We show that the 1965 Voting Rights Act contributed to the well-documented improvements in economic status experienced by black Americans after 1950. Counties subject to federal election oversight witnessed reductions in the black-white earnings gap of around 5.5 percentage points between 1950 and 1980. Our analysis of mechanisms suggests that minority political influence improved blacks' relative position through increased public employment, fiscal redistribution, as well as through implementation and enforcement of group-favoring labor market policies, such as affirmative action and anti-discrimination laws.

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1 Introduction

A half-century ago, U.S. Supreme Court Chief Justice Earl Warren hailed the right to vote as "preservative of [all] other basic rights," social, civic, and economic.[1] This description reflects a belief in the franchise's power to make politicians accountable for helping all citizens, including the most socially and economically vulnerable minorities, achieve a better standard of living. His view also finds support in positive theories of voting, which claim that extending the franchise to marginalized groups should increase pro-poor redistribution and in turn reduce inequality (Romer 1975; Meltzer and Richard 1981).

Yet, empirical evidence on the economic value of political voice for disadvantaged minorities remains thin. We bring evidence to bear on this question by examining one of the major episodes of minority enfranchisement during the past century: the passage of the 1965 Voting Rights Act (VRA). The VRA outlawed discrimination at the voting booth directed against racial minorities in the U.S. (in particular, black Americans). As a consequence of this major civil rights law, the size of the black electorate increased almost overnight – particularly in the southern U.S., where voter participation had been most heavily suppressed.[2] We provide evidence on the instrumental value of the vote by studying the VRA's impact on labor market inequality. Gaining equal access to employment opportunities was, by a wide margin, the most important policy issue to black Americans at the time of re-enfranchisement (see Figure I). As such, examining whether political influence changed black Americans' economic fortunes after decades of suppression is sensible.

We identify the causal effect of voting rights on earnings inequality using temporal and spatial variation in federal voting rights protection. While the VRA's blanket ban on voting discrimination applied nationwide, the statute designated a subset of counties and states (mainly in the South) for active government intervention to ensure black participation at the ballot box (see Figure II, top-left panel). Of course, Congress did not randomly choose the jurisdictions subject to this stringent oversight. Moreover, while some factors that led to federal oversight are observable to the researcher (such as laws or economic conditions), many are not (such as local racial animus). These factors were also potentially correlated with racial differences in economic status. We alleviate concerns about correlated unobservables using a quasi-experimental design that compares "like" with "like." Rather than comparing outcomes across all covered counties and uncovered jurisdictions, which would largely amount to a comparison of South to North, we focus on adjacent county pairs (both

[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 exposition, we also include Arizona, despite its location in the Southwest, as it was one of the major areas affected by the treatment variation.

across states and within states) – where one county is protected under the VRA, and the other is not.[3] This approach allows us to control for time-varying, smoothly-changing unobservable conditions (such as cultural, political, or economic differences) that may confound estimation using coarser data (e.g., at the state level). As we explain below, our study design, which leverages both state and county-level variation in VRA coverage, necessitated the use of confidential administrative data from the U.S. Census Bureau.

Using the long-form population censuses and a generalized triple-differences design, we find that the VRA reduced the wage gap between black and white workers by around 5.5 percentage points between 1950 and 1980 (p-value < 0.05). This treatment effect is driven primarily by rising black wages and comes at no significant cost to black employment. A remaining concern for isolating the effect of any single Civil Rights-era reform is separating the effects of other major reforms from this period (Donohue and Heckman 1991; Smith and Welch 1977). Indeed, the 1960s ushered in several major federal reforms designed to ameliorate economic inequality – including the Equal Opportunity Act (1964), the Civil Rights Act (1964), the Fair Labor Standards Act Amendments (1966), and the Fair Housing Act (1968).[4] To alleviate concerns about concurrent legal changes and unobserved state institutional differences, as well as differential economic trends, we show that similar effects are observed for a within-state experiment, as well as during a later expansion of voting rights protection to over 200 counties in Texas, Arizona, and other counties within the Southwest.

After documenting a causal relationship between black political power and economic status, we probe potential economic channels through which political incorporation may operate. We focus first 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; 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.

In addition to this direct channel, both the increased likelihood of employment within government and the wage premium within that sector put upward pressure on *overall* black earnings in the private sector.[5] To demonstrate this point, we show that the VRA's largest private sector earnings

[3] We discuss the nationwide variation in VRA coverage at length in Section 2.

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

[5] The increased public sector employment of black Americans put upward pressure on previously low black wages

effects took place in occupations where private firms faced competition with the government for black labor. 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.

The VRA also affected 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 interventions that aimed to reduce black-white income disparities, such as anti-discrimination, affirmative action, and social welfare policies. Finally, we consider *how* the right to vote affects political power – whether it is by changing the incentives faced by all politicians (i.e., a shift in a locality's "median voter"), or by increasing the presence of black elected officials who privilege black voters' interests. In our view, the weight of the evidence favors the former.

Contributions

Our paper contributes to a relatively under-explored intersection of research within economic history, political economy, and labor economics. First, we provide novel evidence that minority political empowerment contributed to the rapid improvements in labor market inequality over the second half of the twentieth century. This evidence contributes to a large body of existing knowledge regarding the sources of racial earnings convergence after 1950. Existing studies generally emphasize one of two general hypotheses. Research by Smith and Welch (1989a) 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]. In a similar vein, Butler and Heckman (1977) argue that black wages improved partly because Great Society-era social programs, from which black families benefited disproportionately, reduced the labor force participation of low-skilled black workers.

On the demand side, several economists suggest that active state intervention contributed measurably to improving labor market outcomes. Studies have documented effects of numerous civil rights-era regulations, such as the 1964 Civil Rights Act (CRA), the creation of affirmative action requirements, and the expansion of the minimum wage (Freeman 1973; Leonard 1990; Donohue and Heckman 1991; Chay 1998; Derenoncourt and Montialoux 2018). However, while Donohue and Heckman (1991) allude to the importance of black political power for enforcing these policies, there has been no quantitative test of the role played by the VRA. This paper thus provides the

by improving their bargaining power – both by dampening private labor supply and improving the outside-option wage.

[6]Smith and Welch (1989a), 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) also show that improving school quality played a role, but they argue that anti-discrimination laws likely played a significant role as well.

first empirical evidence that minority political power may also have contributed to the reductions in black-white economic inequality observed during the period.

In this regard, our findings provide new empirical evidence about the possible economics impacts of minority political representation. Positive political theorists – including Romer (1975), Roberts (1977), as well as Meltzer and Richard (1981) – suggest 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. By showing that the VRA improves black earnings, we show that political incorporation can in fact confer direct redistributive benefits to the marginalized group receiving political rights.

Finally, we contribute to work examining the relevance of public sector agencies for understanding of black employment trends. Numerous studies, mainly outside of economics, have suggested that government jobs provided a source of economic mobility for black Americans after 1960 (Katz, Stern, and Fader 2005). Figure III highlights the increasing relevance of the public sector for black workers by demonstrating rapid transition into the public sector beginning in the mid-1960s. This trend is especially pronounced within the South.[7] This movement is not necessarily surprising, though, since the public sector offered any given black worker a substantial wage premium (Freeman 1976; Hout 1984). Our study provides novel evidence that political influence is one causal factor related to the increasing share of black workers within the public sector. The closest paper to ours in this regard is a study by Henderson (2017), who documents that restrictions to immigrant voting rights during the nineteenth century improved the public-sector employment prospects of native workers. Our findings, while focusing on a relaxation on restrictions to voting rights, are similar in spirit. 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

[7] The black share of public employment grew particularly sharply in the Deep South post-VRA (Figure C.2). The time path of white public-sector employment, on the other hand, is unchanged. These raw statistics provide *prima facie* evidence that the VRA was associated with the changing racial composition of the government workforce.

improve minority employment outcomes in both the private and public sectors. Our study differs from these studies by examining the effect of citizen political empowerment more generally (via the franchise), rather than effects stemming only from the election of minority politicians.

2 Context: The Jim Crow South and Passage of the VRA

The Fifteenth Amendment to the U.S. Constitution, enacted following the American Civil War, granted the right to vote to all men, regardless of "race, color, or previous condition of servitude." Armed with the right to vote, black Americans (located mainly in the South) prospered economically, during the period known as the Reconstruction (Logan 2018). By the early 1900s, nearly every state in the South had responded to black voting rights by enacting *de facto* suffrage restrictions. These laws, commonly-referred to as "Jim Crow" laws, took many different forms – such as poll taxes and literacy tests that were administered in a discriminatory manner by county election officials, as well as whites-only party primaries.[8] As a result of these political restrictions, most eligible black adults could not vote before the civil rights era.

The political suppression of black Americans is widely considered in economics to be a major cause of the persistent black-white disparities in earnings, education, and health that emerged during the first half of the twentieth century (Roback 1984; Sundstrom 2007; Wanamaker 2017). Disenfranchisement led to significant reductions in the quality of schools attended by black children, likely affecting later-life outcomes (Margo 1982; Kousser 1980; Pritchett 1989; Naidu 2012). Jim Crow politics also allowed states and localities to openly exclude blacks from government payrolls and facilitated the passage of regulations that segregated blacks and whites on most dimensions of private economic life, including employment (Johnson 1943). Cities in Louisiana, for example, enacted legislation that legally restricted the employment of longshoremen to persons who were not registered voters; in essence, they excluded black workers from this relatively well-paying profession. These represent just a few examples of how, as Alston and Ferrie (1993) explain, "the entire machinery of the state became an instrument with which to coerce blacks," both economically and socially.

The right to vote thus became a centerpiece of the American Civil Rights movement for socioeconomic equality during the 1950s and 1960s. In 1965, President Lyndon Johnson signed the VRA into law, restoring for black Americans (and other racial/ethnic minorities) the right to vote. Enacted to give life to the Fifteenth Amendment, the VRA forbids all electoral structures that deny racial minorities the "opportunity...to participate [equally] in the political process and to elect representatives of their choice." Even today, it remains the chief statutory tool for attacking racial

[8] See Perman (2001) for a history of Southern minority disenfranchisement.

discrimination in voting.

The key enforcement provisions of the VRA are Sections 2 and 5. Section 2, which applies nationwide, eliminated Jim Crow-style voting restrictions that denied the right to vote on account of race. This section of the statute also provided voters nationwide with a private right of action 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 was for five decades the strongest provision of the VRA, and is the provision that gives us purchase for identifying the impact of black political power.[9] This provision gave federal authorities extraordinary oversight powers to protect minorities' right to vote in areas of the country (primarily in the South) where their rights had been most suppressed. "Covered" counties and states were required to "preclear" any change to electoral procedures with the U.S. Attorney General or the U.S. District Court for D.C.[10] Just as important, Section 5 also allowed for the appointment of federal examiners to oversee the political process in covered jurisdictions and ensure there was no blocking of black voters from registration lists or polling places. This use of federal examiners was crucial for ensuring that previously discriminatory jurisdictions could no longer "foot drag" to register eligible black voters (Karlan 1989). Within two years of passage, the use of examiners under the VRA contributed to the voter registration of more black Americans than at any point since the ratification of the Fifteenth Amendment (Davidson and Grofman 1992).

Jurisdictions to which Section 5 applied were defined in the VRA's coverage formula (Section 4(b)) to include any city, county, or state that used a test or device (e.g., a literacy test) and had less than a 50 percent turnout in the 1964 presidential election. Section 5 thus initially applied to counties in Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia, 41 of North Carolina's 100 counties, and one county in Arizona. Amendments to the VRA in 1970 and 1975 (henceforth, the "VRA Amendments" or "Amendments") extended coverage to several more counties in the South and Southwest, including in Florida, Oklahoma, Arizona, and New Mexico, as well as all counties in Texas.[11]

Our main analysis will thus compare economic outcomes for residing in political jurisdictions covered under the VRA to those residing in uncovered jurisdictions. Before proceeding to our

[9] Section 5 is described by election law scholars as "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."

[10] To obtain federal approval, jurisdictions were required to demonstrate that a proposed political change had neither a discriminatory "effect" or "purpose" on black voters. The Supreme Court case *Beer v. United States* defined discriminatory effect as "retrogression," or any change that reduced the minority voters' opportunity to elect candidates of choice.

[11] A handful of jurisdictions in Michigan, 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 black-white earnings inequality, we do not consider these jurisdictions.

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 Motivation: Economic Effects of Political Power

The redistribution and deployment of state resources is a key function of government. The VRA created such incentives for Southern politicians to respond to the needs of black American communities for the first time since Reconstruction. Electoral incentives in the post-VRA South were particularly acute since black voters tended to be geographically compact and relatively homogeneous in their political preferences (Keech 1968).[12]

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) (see Figure I). As such, dedicating government resources to ensuring equal economic opportunities, particularly within the labor market, was a plausible policy area for government action. The net impact of black enfranchisement on earnings and employment would in turn depend on the sum total of the changes in redistributive policies following passage of the VRA. Existing research suggests a few candidate channels – both direct and indirect – through which representation could improve labor market outcomes. On the labor supply side, minority-preferred politicians were more likely to invest in previously under-resourced institutions in black neighborhoods, such as schools and hospitals (Cascio and Washington 2014). Increasing the quantity and quality of these institutions would tend to improve the earnings potential of black workers (Card and Krueger 1992).

Participation in politics also ensured that black families, who were disproportionately poor relative to their white neighbors, would benefit from new transfer programs launched under LBJ's War on Poverty (Bailey and Duqette 2014). Past studies show that welfare schemes like the Aid to Families with Dependent Children (AFDC) program and the Food Stamps Program (FSP) played a key role in improving black male earnings by making labor supply more elastic (Smith and Welch 1989b; Donohue and Heckman 1991). Importantly, though, the War on Poverty was designed to facilitate black influence over program administration – suggesting a key role for minority political power in ensuring a fair distribution of benefits. Indeed, during the Congressional deliberations that ultimately culminated in the VRA's passage, Representative Adam Clayton Powell (the liberal Harlem politician) argued that one of the main reasons for federal voter protection was to ensure

[12]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.

the expanding safety net would not become "a war on poverty for white people only."

Black political representation could also have positively affected the demand-side of the market for black workers. Politicians exercise significant authority over the distribution of contracts, which were used frequently in the post-Civil Rights era to increase minority hiring (Santoro and McGuire 1997). Under electoral pressure from black voters, politicians may also have channeled resources toward the enforcement of civil rights equal employment opportunity law and affirmative action mandates, consistent with evidence from sociology and public administration (Stainback, Robinson, and Tomaskovic-Devey 2005; Shulman 1984). Politicians also had resources to improve the earnings of black workers *directly*, through government hiring. Unlike private-sector firms, government hiring decisions are shaped not only by efficiency goals, but also equity and other explicitly political considerations (Blank 1994; Alesina, Baqir, and Easterly 2000; Enikolopov 2014; Chen, Henderson, and Cai 2017). Particularly in the South, where black workers faced substantial pay discrimination in the private workforce, public-sector agencies offered significant wage premia.

As we discuss in greater detail in Section 6, increased opportunities for federal, state, and municipal employment may also have exerted indirect pressure on private-sector employers. Government employees often comprise a meaningful share of a local labor market (on average, around 20 percent). A positive shock to black public-sector demand could thus put upward pressure on private sector wages by either tightening the market for black workers in the private sector, or by improving black workers' outside option due to the higher pay offered in government. Such inter-sector wage effects are now well-documented in other settings (Moretti 2010). In the context of changes to local industrial composition, for example, Beaudry, Green, and Sand (2012) document that positive sectoral demand shocks generate substantial cross-sectoral general equilibrium effects on worker wages. When accounting for such effects, total wage effects are 3–4 times larger than effects measured by just considering the direct effects of increased labor demand. We thus consider both the direct and indirect ways in which VRA-induced public labor demand may have improved relative black earnings.[13]

4 Research Design

4.1 Data

A comprehensive accounting of how the VRA affected black economic status required us to assemble a large number of data sources, including some which are being used for the first time. Our main data are the restricted-access United States Decennial Censuses of the Population (DEC), accessed

[13] We modify the Beaudry, Green, and Sand (2012) approach to focus on the interaction of public- and private-sector wages in Appendix B in the context of political pressure.

from within the Berkeley Research Data Center (RDC). The RDC maintains the complete long-form samples from 1950 on, allowing us to assess the impact of the VRA on labor market outcomes using data 15 years before and after the VRA's passage.[14] The complete long-form data make up a 20 percent sample of the U.S. population (except for 1950, which is only a 1 percent sample).

Our main outcome of interest is workers' wage earnings. Following previous studies, we focus on adult males between ages 18 and 64 who are full-time, full-year (FTFY) workers (i.e., worked more than 44 weeks at least 40 hours) (Donohue and Heckman 1991; Card and Krueger 1992).[15] In addition to providing larger samples, the restricted-use population censuses provide information on county of residence. This information allows us to exploit the county-level variation in treatment and to implement our county-pair empirical design. We also use other individual and household-level information from DEC, such as worker type (public vs. private), occupation, place of work, migration status, and educational attainment. We exclude self-employed workers in the main analysis, although we do examine this population when considering mechanisms related to government contracting and affirmative action.

We demonstrate a "first stage" on political outcomes using a few different sources. First, we rely on voter turnout data (the fraction of eligible adults who vote) for each presidential election between 1948 and 1980. The data come from the Interuniversity Consortium for Political and Social Research (ICPSR) and Dave Leip's Atlas of U.S. Presidential Elections; voting-eligible population estimates are obtained from Gentzkow et al. (2011). A limitation of these data is that we cannot be sure whether any changes in participation are driven by black or white voters. Thus, to show changes in overall participation most likely redound to the benefit of black voters, we use two other data sources. First, we use race-specific voter registration data that are available only for a subset of Southern counties for the time period 1960–1970. Second, we measure changes in district-level political ideology using the 87–99th Congresses from the DW-NOMINATE (DW-NOM) project initiated by Poole and Rosenthal (2001). We focus on the civil rights/race-related dimension (second dimension) of the DW-NOM scores, which collapse Congressional voters on race-related issues to a single liberal-conservative dimension (where more negative scores indicate more racially-liberal politicians, on race-related issues). Both sources are discussed in greater depth in Appendix A. We do not examine party vote share effects given that Southern partisan realignment began during this time (Kuziemko and Washington 2018).

Finally, we also make use of additional data sources to probe mechanisms further, including a few novel data sources. To examine whether the VRA augmented existing anti-discrimination and

[14] This period also is the main one in which black progress in the labor market was observed (Bound and Freeman 1992).

[15] Estimates including both women and men are presented in Appendix C.

affirmative action policies, we use historical establishment-level data from the Equal Employment Opportunity Commission (EEOC), which we digitized from information obtained from the National Archives. County-level statistics on government expenditures come from the U.S. Census of Governments (COG). To examine effects on the supply of minority politicians, we digitize over 10 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.[16] We supplement this source with data from Matthews and Prothro (1966), who collected information about black elected officials in the early-1960s South.[17] To probe redistributive political channels, we use locality-level data on electoral rules from the International City/County Municipal Association (ICMA). County-level control variables are based on public-use Decennial Census estimates. Further details on these data as well as other data construction can be found in the Data Appendix.

4.2 Empirical Framework

Our goal is to evaluate whether the political empowerment of black Americans improved their (relative) status within the labor market, and what policy levers affected these changes. The main obstacle we face is that VRA-covered jurisdictions were not randomly singled out for additional voter protections. Section 5 coverage was deliberate: the VRA targeted the "worst of the worst" in terms of political discrimination against racial and ethnic minorities. It is thus possible that the unobservable characteristics (social, cultural, economic, or political conditions) that led to coverage were also correlated with economic outcomes, biasing our estimate of the VRA's effect. For example, 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 alleviate concerns about unobservable confounders, we focus our analysis on the subset of adjacent county pairs that straddle Section 5 state and county boundaries. This approach is increasingly used in observational studies of policies that vary across space such as the minimum wage and health insurance expansions (Feigenbaum, Hertel-Fernandez, and Williamson 2018; Clinton and Sances 2018). The intuition is that many cultural, political, or economic conditions – any of which may affect the economic outcomes of interest – are likely to vary smoothly rather than discretely at jurisdictional boundaries. Two counties separated by a border either across or within a state should appear more similar than groups of counties far away, or than entire states. As such, zooming in on only neighboring counties allows us to compare "like" with "like" jurisdictions,

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

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

reducing concerns about confounders to causal estimates based on data using the entire universe of counties.[18]

With the set of adjacent VRA and non-VRA counties, we use a simple OLS regression framework to compare changes in political and economic outcomes between VRA-covered and uncovered counties, before and after the VRA took effect. For our main hypothesis, our identifying assumption is that blacks' labor market performance would have evolved similarly in VRA and non-VRA counties in the absence of the VRA taking effect. Potential threats to identification include any omitted factor correlated with the passage of the VRA that affects the outcomes of interest (earnings, turnout, etc.). 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. We measure such characteristics prior to adoption of the VRA (using 1960 levels).[19] County characteristics are interacted with linear and quadratic time trends. VRA coverage applied to counties within part or all of 21 states. We thus cluster at the county level for inference.[20]

We can indirectly assess the identifying assumption in a few ways. First, following Hornbeck and Naidu (2014), we examine pre-period trend and level differences between VRA and non-VRA counties. We first estimate how trends in various economic, demographic, and social characteristics before the VRA relate to future coverage. Specifically, we estimate the following regression:

$$\mathrm{Y}_{ct} - \mathrm{Y}_{c(t-a)} = \beta \mathrm{VRA}_c + \epsilon_c \tag{1}$$

where Y is a county characteristic that may suggest a confounding trend, and VRA is an indicator for whether a county is covered under the VRA. Table C.1 demonstrates that, in terms of many different economic, social, and demographic characteristics, there are significant trend differences in between neighboring VRA and non-VRA counties; this is true both with and without state fixed effects (Columns 1 and 2, respectively). Notably, when we examine *all* VRA and non-VRA counties (pooled), we find that the trend differences often *are* significant. To provide further evidence against trend differences, 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.[21]

We also provide additional corroborating evidence that our research strategy better approxi-

[18] In our setting, state-level analyses would be complicated by the fact that there is substantial within-state heterogeneity in VRA coverage (Ang 2018).

[19] 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.

[20] For robustness, we cluster our standard errors at the state level, as well as bootstrapping our standard errors to show the stability of statistical inference.

[21] 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.

mates an "apples-to-apples" comparison in which we should be less concerned about unobservable institutional differences that may confound our estimation. Table I presents summary statistics for our sample – including average county characteristics based on census data 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 I 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 right at VRA county/state borders. All difference-in-means tests produce differences that are not significantly different from zero.

5 Main Results: The Impact of the VRA on Politics & Economics

5.1 How Did the VRA Change Politics?

The effect of the franchise on socioeconomic wellbeing depended on making government accountable to black voters. We thus begin by assessing the first-stage impact of the VRA on both participation and representation.[22] To demonstrate that the VRA mobilized black voters as intended, we use voter turnout for presidential elections as our outcome, as is standard in this literature (Ang 2018). We use the following difference-in-differences (DD) specification:

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

where c indexes county, 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. α provides an unbiased estimate of the VRA's causal effect on voter turnout – under the assumptions of parallel trends, that there are no concurrent institutional changes across covered and uncovered counties, and that there are no geographic spillovers between counties.[23] In addition to Tables C.1 and I, we provide

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

[23] Spillovers are a concern if black voters move to counties covered by the VRA. We demonstrate below that migration levels are small, and thus unlikely to confound our estimated effects (see Section 5.2.2).

further evidence in support of the identifying assumptions below.

Table II presents the results for the first-stage analysis. Consistent with the VRA mobilizing a new block of eligible voters, we find statistically significant increases in eligible voter turnout resulting from minority voting rights protection under the VRA. Our estimated effect sizes range from around 6.5 to 11.5 percentage point increases (p.p.) between 1948 and 1980, with estimates generally significant at the 1 percent level.[24] Estimated effects are also robust to excluding control variables (Column 1), as well as to the inclusion of state time trends (Column 3).

Because voter turnout data is not race-disaggregated, our estimated effect may co-mingle the effect of black enfranchisement with any post-VRA effect on white voters (due to racial "backlash," for example). We thus provide a few supplementary analyses that suggest the VRA's impact is being driven by the mobilization of black voters. First, we adopt the approach of Cascio and Washington (2014), who argue (and demonstrate) that the political effects of removing literacy tests should be stronger in areas with higher black population shares. Consistent with this view, Column (4) suggests that for every 10 percent increase in black population share, turnout increased by 2 percent. This finding suggests the VRA increased participation at least in part through the mobilization of black voters, since one expected to observe stronger treatment effects in VRA counties with larger numbers of newly-enfranchised voters.

Figure V (top-right panel) shows these results dynamically, using event-time dummies interacted with both the fraction of the population that is black and an indicator for counties covered by the VRA (as well as baseline controls and state linear time trends). The graph provides visual support of a first-stage political effect, as well as support for the parallel trends assumption.[25] In the Appendix, we also draw on data for a subset of counties for which race-specific registration data exists; we re-estimate Equation 2 using this data. As Columns (1) and (2) of Table C.4 show, the VRA led to increases in registration of both white and black voters. However, the statute clearly produced much larger increases in black registration (Column 3).

Further evidence that the VRA improved black representation is found in Appendix Figure C.1, as well as in Panel B of Table II. We estimate a district-level analogue of Equation 2 to show that elected officials responded to the VRA by supporting the preferred policies of black constituents. Data on politicians' activity at the state/local level is limited, so we instead examine Congressional behavior. We focus on the second dimension of the DW-Nominate score, which

[24] In Appendix Table C.2, we demonstrate that results are unchanged when examining all counties within the states comprising our sample (not only the subset of county pairs).

[25] We note, though, that there also seems to 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.

collapses Congressional roll-call votes on issues related to race and civil rights to a single score that is increasing in conservativeness. Districts are defined to be any district that contains at least one VRA-covered county, as in Ang (2018). The estimates are negative and significant (p-value $<$ 0.10), indicating that the VRA made Congressional representatives 6–8 p.p. more racially liberal (Columns 1 and 2). We also find that racial conservativeness is decreasing in minority population share (Column 3), though this interaction is not statistically significant.[26]

Our first-stage findings largely confirm existing work in political science and economics regarding the effects of the VRA (Schuit and Rogowski 2017; Fresh 2018; Thompson 2015). There has been less evidence, however, examining what specific provisions of the VRA are "doing the work" – the striking down of disenfranchisement devices under Section 2b, or affirmative federal intervention in election under Section 5. To demonstrate an effect of affirmative federal enforcement, we exploit the fact that within the set of covered jurisdictions, the DOJ had the authority to send civil servants to Southern jurisdictions. These officials had the authority to ensure there was no black voter denial. We thus collect data indicating the counties to which election officials were sent (see Appendix A for details). As Table C.6 demonstrates, the VRA's political turnout impact is enhanced (relative to the set of covered jurisdictions) where federal enforcement agents monitored the voting process. Moreover, in the bottom left panel of Figure V, we evaluate the effect of federal examiners *within* the set of covered counties, and again find a significant positive effect of federal election oversight on political participation.

To summarize the evidence here, our results suggest that the VRA significantly improved both black political participation and representation. Moreover, these effects stem at least in part from the active intervention of the federal government to promote minority participation and ensure that Jim Crow-style suppression did not return.[27] We turn now to examining our central hypothesis: Did political empowerment produce tangible economic gains for poor black Southerners?

5.2 Impact of the VRA on Black-White Earnings Inequality

As Figure I 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 thus focus on analyzing the VRA's impact on black Americans' (relative) wage earnings.

Because we observe the race of individual workers in the population censuses (and we know that

[26] In Appendix Table C.5, 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).

[27] In the Appendix Table C.8, we also show that the wage gap effects of the VRA were *also* stronger in those counties where federal examiners ensured non-discriminatory treatment at the polls.

the VRA was designed to re-enfranchise *black* voters specifically), we modify Equation 2 to focus on the economic outcomes of black relative to white workers. In other words, we estimate the causal effect of the VRA on the black-white racial wage *gap*. This difference-in-difference-in-differences (DDD) specification, estimated using individual-level data, thus takes the form:

$$\text{Economic Outcome}_{ict} = \beta \left[\text{VRA}_{ct} \times \mathbb{I}\{r(i) = \text{Black}\}\right] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{ict} \quad (3)$$

Previous indexes remain the same, and $r(i)$ now indexes the race of person i ($\mathbb{I}\{r(i) = \text{Black}\}$ is an indicator for whether a worker is black).[28] The primary dependent variable is the log hourly wage measured for person i in year t (though we also analyze other outcomes).[29] The interaction $[\text{VRA}_{ct} \times \mathbb{I}\{r(i) = \text{Black}\}]$ (which we will refer to as VRA × Black for ease of exposition) takes a value of 1 if an individual is a black worker in a VRA-covered county after the law takes effect, and 0 otherwise. We include all race, county, and year fixed effects, and two-way interactions. We control for observable skills using years of education, experience (defined using a worker's reported age minus education years minus five), and experience-squared. We allow returns to skills to vary by year (γ_t) to allow for changes in the wage structure (Katz and Autor 1999). 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.

The parameter of interest is β, which captures the impact of the VRA on black workers' (relative) wages, conditional on education and experience. If the VRA improved minority economic outcomes, we would expect $\beta > 0$. A benefit of our design using matched county pairs is that we can control for non-institutional factors that are unlikely to vary discretely at jurisdictional borders, and which may affect the racial wage gap – such as culture, prejudicial attitudes, and other 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 pattern must not be consistent across neighboring counties.[30]

Table III presents our main results, where the coefficient β on VRA × Black indicates the

[28] 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.

[29] Hourly wages are constructed from DEC data on wage income earned last year, weeks worked last year, and hours worked last week. In 1980, the Census Bureau started recording usual hours worked, which we use for this year.

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

impact of the VRA on black wages relative to white wages – i.e., whether the VRA reduced the racial wage *gap*. Across numerous specifications, the results suggest that the VRA caused a statistically significant improvement in black earnings. Column (1) presents our baseline estimates, using only individual worker characteristics and the full set of fixed effects discussed above. The VRA caused a statistically-significant 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 labor market (geographically-speaking). Columns (2) through (5) show that this effect is robust to the choice of specification. Column (2) re-estimates the baseline model with the inclusion of several county-level controls.[31] 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, to allow for local trends in earnings. The results again suggest the VRA caused a statistically significant increase in relative black wages, of around 5.6 p.p. Finally, the effect is similar in magnitude (less than 1 p.p. smaller) even when we include county-by-race linear time trends (Column 5). Overall, these results are strong evidence that the expansion and protection of black political rights improved the socioeconomic position of black Americans. For brevity, we relegate further analysis of the robustness of these primary findings to Appendix C.3.

Figure VI suggests that the increase in black wages is not part of an overall decline in earnings within VRA counties.[32] The reduction in black-white earnings inequality is driven primarily by an *increase* in black wages. Wage trends in the decade before a county becomes covered are similar for both black and white workers, supporting the identifying assumption that outcomes would have evolved similarly in the absence of treatment. After the VRA takes effect, we observe a mean increase in the wages of black Americans (close to 5 p.p., significant at the 5 percent level), as well as a modest *reduction* in white wages of approximately 1 p.p. Appendix Table C.7 shows that these results are stable to multiple potential specifications. A modest reduction in white earnings should not surprise us, given an abundance of evidence on the substantial race premium reaped by white workers in the Jim Crow South (Sundstrom 2007).

For the remainder of the paper, we focus on our main specification, which identifies the VRA's effect on the wage gap. Examining the main effect year by year informs thinking about possible mechanisms underlying the main results (a point we return to later) (Kose, Kuka, and Shenav 2019).[33] In particular, Figure VII suggests the impact of the VRA emerges relatively soon after

[31] 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 and quadratic time trends.

[32] A limitation of using the administrative Census micro data is that we are limited to the censuses only beginning in 1950.

[33] In particular, we estimate:

taking effect, with treated counties experiencing significant reductions in the wage gap within five years. This relatively rapid improvement is consistent with work by Donohue and Heckman (1991) and Card and Krueger (1992), who suggest that civil rights legislation (including the VRA) produced benefits for black Americans in relatively short order. This timing also suggests that our results are likely not due exclusively to investments in human capital-building institutions, such as schools, which would be observed in future rather than contemporary labor market cohorts.

The direction and magnitude of our estimates are consistent with research on the determinants driving racial earnings convergence during the Civil Rights era. 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 thus account for around one-fifth of the decline in the adjusted wage gap (this effect is within-South; the contribution would likely be smaller if we were to consider nationwide wage convergence). By contrast, Card and Krueger (1992) find that around 15–20 percent of the declining racial wage gap came due 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, Derenoncourt and Montialoux (2018) show that the 1966 expansion of minimum wage coverage under the Fair Labor Standards Act explains more than 20 percent of the racial earnings gap reduction. Minority political power may have well complemented any of these other channels, or contributed independently. We find support for complementarities, as we discuss in Section 6.2. Moreover, in Appendix Section C.3.2, we discuss in greater depth evidence suggesting our results are consistent with Deep South "catch-up," rather than a surpassing of other workers by black workers in VRA counties.

While our analysis focuses on black men to facilitate comparison with the previous literature, there is also ample anecdotal evidence of historical employment discrimination against black women. Before 1960, it was difficult for a black woman to get a job as a clerical worker, regardless of her credentials. In a 1940 Women's Bureau survey, more than 50 percent of employers reported that they had a company policy against hiring black women as clerical workers (Goldin, 1990). Weaver's classic study of the plight of black labor documents the exclusion of blacks from skilled blue-collar jobs, particularly in the South (Weaver, 1946). In Table C.13, we show that that VRA raised relative earnings for men and women. When we expand the sample, we again find that black-white

$$\log(Y_{ict}) = \sum_{t=-1}^{2} \mu_t \times [\text{VRA}_c \times \mathbb{I}\{r(i) = \text{Black}\}] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \tag{4}$$

where c, t, Y_{ict} are defined similar as above. The parameters of interest are the four μ's, which test for mean shifts in individual economic outcomes post-VRA (adjusting for pre-existing trends). The period before the VRA takes effect in a county (typically 5 years before) is the reference year.

gap declines by around 5 log points, consistent with our main findings.

5.2.1 1975 VRA Amendments & Within-NC Variation

As in most DD designs, one threat to the research design is the possibility of concurrent institutional changes that vary exactly at VRA county/state borders. For example, in 1964, Congress passed the Civil Rights Act (CRA), one of the major laws outlawing discrimination in hiring or pay.[34] Unlike VRA coverage, the CRA applied nationwide. Any policy changes that apply evenly nationwide should not confound our estimation, although potential heterogeneity in the CRA's effect are possible. Even heterogeneous effects of the CRA across space, though, should be mitigated using the neighboring-county design – particularly since economic inequality was primarily a *within*-South phenomenon (Donohue and Heckman 1991). It is nevertheless possible that other civil rights laws had their strongest effects in covered counties, given that the VRA targeted the "worst of the worst" in terms of existing voting discrimination.

We alleviate concerns about both unobserved institutional changes and economic trends by showing that our main finding holds for different subsamples of VRA coverage. First, we focus on Congressional amendments to the VRA that extended Section 5 protections to 283 additional counties. Voters in these additional counties received federal voting rights protection in either 1970 or 1975, several years after the main reforms of the civil rights-era were passed. Newly-covered counties included all of Texas and Arizona, as well as parts of Florida, New Mexico, Oklahoma, and 10 other states across the country.[35] We use the VRA Amendments to split the sample into DEC respondents within initial and later-affected counties (and their respective neighboring counties).[36] Panel B of Table III presents estimates for our preferred specification, Equation 3, separately by the 1965 (main VRA) and 1970/1975 (Amendments) coverage rounds (Columns (2) and (3), respectively). The relative wage effect of the VRA for initially-covered counties is similar in magnitude (7 p.p., $p<0.05$) to that of Amendment-covered counties (4.5 p.p., $p<0.1$). Column (1) repeats the benchmark estimate for reference.[37].

Finally, we limit our analysis to the DEC subsample who resided in North Carolina (NC), the

[34] Two provisions of the CRA are relevant to us. Title II outlawed discrimination in places of public accommodation. Title VII outlawed private-sector employment discrimination.

[35] The expansion covered three of the four census regions. However, because our focus is on the effects of black-white economic inequality, we focus on VRA coverage in the South. Specifically, we exclude Alaska and South Dakota, where the VRA targeted discrimination toward Native Americans (these states had very small black populations). Moreover, we also exclude covered jurisdictions in New Hampshire and Michigan, due to the small samples of black respondents – making difficult compliance with official Census disclosure requirements.

[36] States with control counties for later-affected areas include California, Oklahoma, New Mexico, Nevada, and Utah.

[37] In Appendix Section C.3, we provide estimates from several other specifications to demonstrate the robustness of these subsample-based estimates (see Table C.11)

only state with even treatment heterogeneity (top-right panel, Figure II).[38] Column (4) of Table III re-estimates the main regression for the within-NC set of county pairs. The results are similar compared to our overall results, and in fact are slightly larger in magnitude (an 11 p.p. effect; $p<0.01$). We bolster our confidence in this within-NC result even further by using the within-NC VRA coverage formula, in the spirit of a regression discontinuity design. NC counties became protected based on having election turnout of below 50 percent in 1964. Focusing on counties close to this threshold, we document similar findings in Appendix Table C.14.[39] To summarize, the results in this subsection suggest that our primary effects are not confounded by differential trends, additional policy changes, or unobserved state-level changes. Several additional robustness checks are discussed in Appendix C. For example, corresponding to our discussion of the impact of federal election examiners above, we also show similar downstream effects in terms of black labor market outcomes (see Appendix Section C.3.3). We also find that our results are effects are stronger in more urban areas (Appendix Section C.3.4), consistent with mechanisms that we discuss below, such as public employment.

5.2.2 Robustness & (Ruling Out) Potential Confounders

In this subsection, we briefly examine potential confounders to our analysis, reserving a more detailed discussion of these results to the Appendices (see Appendix C.5). One major concern in our setting is the possibility of migration. 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).[40] Changes in wages may reflect higher-status black families migrating to covered counties, which may in turn imply that positive earnings impacts of the VRA are affected by changes in the types of individuals working in covered counties rather than direct government action (Isen, Rossin-Slatar, and Walker 2017).

We provide several pieces of evidence, however, that weigh against migration as a confounder. First, we find little evidence of a compositional shift in the underlying population characteristics

[38] 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.

[39] Details on this subanalysis are provided in Appendix C.3.8.

[40] Indeed, the *out*-migration of black Americans followed political disenfranchisement during the era of Jim Crow, as documented by Naidu (2012) and Margo (1980). The direction of any biased effect due to migration is theoretically ambiguous, though. 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, we may overestimate the effect of the VRA by analyzing black workers who positively select into VRA counties (see, for example, Boustan and Margo (2009)). Out-migration of whites ("white flight") may also exaggerate the magnitude of our finding (Boustan 2010).

of VRA-covered counties, as indicated in Table IV. Each column presents an estimate testing for the VRA's effect on the following (average) population 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 each measure of county composition are small in magnitude and statistically insignificant, suggesting that the VRA's effect on wage equality is not an artifact of compositional changes.[41]

Second, we examine the possibility of spillovers from VRA to non-VRA counties (or vice versa), which may bias our estimate in either direction. If labor markets are integrated across pairs of counties, labor prices may equilibrate – leading us to underestimate any effect. Alternatively, the positive selection of black workers who migrate/commute in response may lead us to overestimate the treatment effect. This latter possibility would be consistent with an "unintegrated labor market," where commuting is possible. To test for spillovers, we compare the effects of the VRA for both the main sample (based on county pairs) and the complement sample based on respondents in "interior" counties. Here interior counties refer to all counties within a state that are contained (partially or fully) within our matched-pair sample, but excluding the border counties. Panel C of Table III (Columns 2–3) provides estimates relating the VRA to (relative) wages, excluding pair-year fixed effects. These results demonstrate that the measured effects are similar for both the interior and border county samples.[42]

We also estimate the degree to which effects for border and interior counties are different, using a similar spatial-differenced specification used in (Dube, Lester, and Reich 2010):

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

The effect is small in magnitude (see Column (1) – roughly 1 p.p., statistically significant at the 5 percent level). To the extent that this subanalysis suggests an amplification of our main effect, it is relatively small. Moreover, we directly rule out cross-VRA commuting in Table C.19, which shows the relationship between the VRA and a worker's commuting status (indicating whether a worker is commuting from residence in a VRA to a non-VRA location, or vice versa). This test suggests the that the lack of voter protection does not affect the relative likelihood that black workers commute for work.[43]

Finally, we test directly for effects of the VRA on black-white migration and commuting differences (see Appendix C.4). In Appendix Table C.18, we examine directly whether there is differential migration using Census data on a person's place of residence five years prior to the census survey is

[41] In Table C.15, we report similar findings using the individual-level micro data. Moreover, as Table C.16 shows, there are also no race-specific changes in the underlying population.

[42] We apply a series of robustness checks, with results in Appendix Table C.17.

[43] Details on the commuting analysis can be found in the Appendix.

conducted.[44] We find quantitatively small (less than 1 p.p.) changes in the likelihood of migrating between covered and non-covered jursidictions. Indeed, 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). Table C.19 shows our analysis of the relationship between the VRA and a worker's commuting status (indicating whether a worker is commuting from residence in a VRA to a non-VRA location, or vice versa). We find a small, statistically-insignificant effect of the VRA on the relative likelihood of commuting across VRA boundaries for work.[45]

5.2.3 Price or Quantity Changes? Extensive-margin Effects of the VRA

Finally, we consider how civil rights policy may effect the extensive margin of employment, and how this may complicate the interpretation of our main finding. In particular, numerous studies have suggested that a variety of institutional changes (criminal justice policy, welfare policies, etc.) may have led to selective withdrawal from the labor force (especially of black workers) (Heckman and Butler 1977; Brown 2006; Chandra 2003). One reason we are not overly concerned about this possibility is that for the time period we examine (the 1960/70s), schooling was essentially uncorrelated with men's labor force participation (Bayer and Charles 2018). Nevertheless, given the possibility that changes in labor force participation may affect the interpretation of our main finding, we examine the relationship between the VRA and extensive-margin measures of labor market activity. For these tests, we use the entire sample of working-age black and white men. We reestimate our primary specification for the following outcomes: (1) whether a worker had positive earnings, (2) whether a worker was not in the labor force, (3) whether a worker had been unemployed in the previous year, (4) number of weeks worked last year, (5) usual hours worked, and (6) whether a worker was employed full-time (and the full work year).

Table V suggests that the VRA had little effect on the relative labor supply of black workers in our sample. Across several extensive-margin outcomes, we detect modest effects that are typically insignificant statistically. We observe a modest increase in the likelihood of positive earnings, and precisely-estimated zero effects on both the relative likelihood exiting the labor force and being unemployed. We also observe very modest reductions in time spent at work (around 1 week and 0.5 hours per week reductions) – both of which are statistically insignificant.[46] The exception to the insignificant findings within this subanalysis, however, concerns being employed full-time in the labor force. We consistently find that the VRA has a negative effect on the likelihood of

[44] In other words, whether a person changed locations relative to five years earlier by the time the census was taken.

[45] See Appendix A for details on data construction.

[46] In Appendix C.5, we further demonstrate the robustness of our results. Specifically, we again allow for the returns to human capital to vary by race, and also control for state-specific trends.

full-time employment. However, this reduction in FTFY work for black employees seems to be offset by increased *overall* earnings for both part-time and full-time workers (Column 7). These two pieces of evidence are consistent with marginal employers being forced to reduce hours/weeks of employment for black workers due to rising black labor costs. In sum, though, it appears that labor supply responses are not a major confounder of our measured effect of the VRA on black wages.

6 Mechanisms: How Political Power Affects the Labor Market

We interpret the main results in the previous section as the reduced-form effect of the VRA on black earnings. As we previewed in Section 3, there are a few ways that government, after becoming responsive to black voters, may contribute to black Americans' improved labor market performance. The main channels we test include redistribution (i.e., access to better jobs in government), better enforcement of private-sector anti-discrimination laws, and improvements in human capital.

6.1 Public-sector Employment

We focus first on how the VRA directly led to rising black wages: via government hiring.[47] The VRA was enacted against the backdrop of the post-war, nationwide expansion of government (Figure IV) (Berry, Grogger, and West 2015). Moreover, public-sector employment had the potential to improve black labor market outcomes directly by offering better access to managerial, professional, and semi-professional/clerical jobs than did the private sector, where employment segregation and discrimination was pervasive (Frazier 1957; Hout 1984; Katz, Stern, and Fader 2005; Laird 2017).[48] Black men working in the public sector earned *twenty* percent more than their private sector peers ($p < 0.01$) (see Panel B of Table VI, which comes from a regression of log wages on a public sector indicator). The value of a government job for white workers, while positive, was smaller (Column 2).

6.1.1 Impact of the VRA on Government Hiring

Minority political power (as proxied by both minority candidates and voting strength) has long been thought to be an important determinant of public sector diversity (Eisinger 1982; Nye, Rainer, and Strat 2015). Moreover, the secular growth of government during this period (Figure IV) opened up new jobs without the need to displace current white workers (Krislov 1967). Causal work,

[47] 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.

[48] Government agencies were viewed as (relatively) nondiscriminatory given both stringent wage/promotion rules and voters' ability to punish discriminatory agencies (Collins 1983; King).

however, on the determinants of the rapid race-specific compositional change of the public sector seen in Figure III remains scant. Existing research, while largely anecdotal, suggests that public sector jobs were a primary source of redistribution that black constituents sought to influence after reenfranchisement (Wright 2013). Maynard Jackson, for example, was elected mayor of Atlanta in 1972 on the promise of hiring/promoting minority workers within City Hall (and the promise of government contracts conditioned on minority hiring).[49]

With the benefits of public-sector work in mind, we explore whether government employment was one mechanism through which the VRA improved the wages of black workers. We use the DEC "Class of Worker" variable, which indicates whether a worker is employed in the private or public sector.[50] We modify our primary specification, estimating the following linear probability model, where the outcome is a dichotomous government worker indicator:

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

β indicates how the VRA changed the relative likelihood that black workers were employed by government (the coefficient of interest is similar to Equation 3 above). Results are presented in Table VI, and suggest that the VRA increased black public-sector employment by 2–4 p.p. Our preferred specification (Column 1) suggests that the VRA increased the likelihood of a black worker being employed by government by 3.8 p.p. ($p<0.01$). This effect is robust to the inclusion of state trends and worker controls, and also to restricting to the cross-border and NC-only samples (Columns 2–5).[51] Building political pressure to enforce equal opportunity in the public sector may have also led to better pay within government, either through promotions or reductions in discrimination. In an appendix, we provide modest support for this channel. Appendix Table C.23 suggests that VRA coverage reduced the black-white wage gap within government by around two percentage points – a positive effect, but less than the effect for the private-sector wage gap.

We can rule out that black workers' increased likelihood of public-sector employment is due to differential growth of government across VRA and non-VRA counties. In Appendix Table C.22, we use data on the size of the public-sector labor force at the county level (from the U.S. Census of Governments). Using Equation 2, with the share of total population employed by government as the outcome, we find a small, statistically insignificant effect of VRA coverage on local public-sector size. We thus interpret the estimates in Table VI as a change in public sector composition, rather

[49]The value of public employment to black Americans extends back even further. For instance, one of Martin Luther King's central policy goals during the Birmingham Campaign of 1963 was to pressure local governments to hire black workers (Jackson 2007).

[50]The Census Bureau did not distinguish between federal, state, and local public workers prior to 1970, so we define "public" as any worker employed by government in any year.

[51]In the Appendix, we provide a number of robustness checks (see Section C.6) Moreover, Table C.21 shows that in absolute terms, black workers were also more likely to be employed in government.

than as politicians simply expanding the public sector.

6.1.2 Minority Public Sector Hiring and Overall Minority Wages

Given the existing 18 p.p. (relative) premium and a 3.8 p.p. relative increase in the likelihood of public employment, we can approximate the contribution of public employment to minority wages:

$$\underbrace{18\%}_{\text{Relative Public Premium}} \times \underbrace{3.8\%}_{\text{Likelihood of Pub. Emp.}} = 0.6\%$$

Thus, the better earnings prospects account for just over 10 percent of the VRA's average treatment effect. Public and private-sector organizations, however, do not function in isolation from one another within a local labor market (Smith 1980). To the extent the VRA functioned as a positive "demand shock" within the public sector, one may expect upward pressure on overall wages, as private firms now faced greater competition for black labor. We thus also document how the VRA's effect on black public-sector hiring affected overall wages, including within the private sector.

We begin with reduced-form evidence that the VRA's effect on minority public-sector employment did in fact put upward pressure on private-sector wages. 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.[52] 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 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).[53] Logistically, we first define public-sector growth by occupation as follows:

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

which denotes the change in relative change in demand for public sector workers within occupation

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

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

o. We construct these measures at the national level. We then split all occupations within our sample into quartiles, giving us $\Delta\text{PubSec}_{q,-c}$, which we relabel as ΔPubEmp_{cq} for simplicity of notation. We then use these measures in a modification to our primary specification (Equation 3) in which we examine heterogeneous effects by exposure to increased public-sector demand.[54]

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 VII shows, that is indeed the case. β_3 (the coefficient on $VRA_{ct} \times Black_{ict} \times \Delta PubEmp_{cq4}$) is positive and both statistically and economically significant, 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. Conversely, β_2 (the coefficient on $VRA_{ct} \times Black_{ict} \times \Delta PubEmp_{cq1}$) is negative and also both statistically and economically significant, indicating the reverse, that black private-sector workers in occupations experiencing low competition from the public sector experienced close to no wage gains. We interpret these results as evidence of substantial spillovers from public-sector hiring gains to the private sector. For robustness, we repeat our estimation of Equation 8, but interacting VRA $\times$ Black with a continuous measure of sector growth by occupation, rather than using quantiles. The effects are similar.

In the second step of this exercise, which we describe in detail in Appendix C.6 (which relies on the formal labor market model described in Appendix B), we calculate the general equilibrium effects that arise from the improved bargaining position of black workers in the private sector due to the improvement in outside option that comes from a large fraction working for the government. As we previewed in Section 3, the impact of the VRA on labor market outcomes will consist of more than the mechanical effect of having a higher-paying government job. Beaudry, Green, and Sand (2012) demonstrate that accounting only for direct effects may underestimate the true

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

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

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.

effect of industrial changes, with general equilibrium effects being 3–4 times the size of direct effects. Similarly, accounting only for the direct effects of the public sector due to the VRA would underestimate the true effect of 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 and C.6.4. 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 comprise a minority of a given area's labor force, making it unlikely that this channel could account for the majority of our observed treatment effect. As such, we also consider whether political influence improved black earnings through direct intervention in the private labor market. Perhaps the most important factors for the abatement of racial wage inequality were anti-discrimination (i.e., Title VII of the CRA) and affirmative action policies (Donohue and Heckman 1991; Chay 1998). Moreover, political institutions have long shaped the effectiveness of the CRA such as through the appropriation of enforcement resources, as well as through legislative oversight (Wood 1990; Dávila and Bohara 1994; Stainback, Robinson, and Tomaskovic-Devey 2005). Furthermore, anecdotal evidence suggests that minority political activism in the aftermath of reenfranchisement shaped the initial force of anti-discrimination laws. From 1966 to the early 1970s, the EEOC investigated nearly 80,000 complaints of employment discrimination – filed largely in the South, by political groups such as the NAACP (Minchin 2015).[55]

Because there is no granular, systematic data on either affirmative action policies or Title VII enforcement,[56] we use several sources of cross-sectional heterogeneity to examine whether the VRA is associated with larger wage reductions in areas where a greater share of the workforce was exposed to federal anti-discrimination laws. We begin with two straight-forward sources of heterogeneity: the fraction of private-sector small and large establishments, motivated by econometric evidence demonstrating how federal anti-discrimination interventions were less well-enforced for small employers (Carrington, McCue, and Pierce 2000). Under Title VII, the mandatory reporting of workplace racial composition only applied to establishments with more than 25 employees

[55] We do not rigorously interrogate in this paper the precise way that improved political representation (via black voting rights) improved legal enforcement. Rather, we rely on findings from political science and sociology documenting that bureaucratic enforcement of anti-discrimination laws such as the CRA depended on political factors (Wood 1990; Dávila and Bohara 1994). We 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.

[56] 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.

(this was later changed to 15 employees). Assuming this threshold to be a proxy for government oversight, we construct proxies for enforcement exposure based on the size distribution of establishments using data from the City and County Data Books (for the year 1962).[57] Using this proxy, we test for heterogeneity in the impact of the VRA in the likelihood of enforcement. As Column (1) in Table C.25 indicates, the VRA has a smaller effect (p-value < 0.10) in areas where a greater fraction of the private-sector workforce is less likely to be monitored by the EEOC for employment and pay discrimination. As expected, Column (2) indicates that this pattern is reversed when using heterogeneity by fraction of establishments that are large (greater than 100 employees). The coefficient of interest, on the interaction VRA $\times$ *Black* $\times$ Frac. Large Est., is positive, suggesting that black relative wages are higher in counties where firms were *more* likely to be targets of EEOC enforcement actions (Carrington, McCue, and Pierce 2000), although this effect is not statistically significant. While admittedly these heterogeneity tests may raise concerns related to correlated unobservable factors (e.g., factors correlated with the size distribution of private firm establishments), our findings here are consistent with previous work on CRA/affirmative action enforcement patterns.

To ameliorate these concerns, in Columns (3)–(4), we account for correlated unobservable factors (i.e., factors that are correlated with the presence of large establishments) by exploiting the change in the establishment size threshold for Title VII coverage from 25 to 15 employees. Using the CBP data on establishments both above and under 20 employees, we construct proxies for the distribution of firms just above and below the CRA-mandated reporting requirement (details about the construction of this estimated exposure of county workforce to the reporting requirement is discussed in Appendix C.7). Results using this additional proxy indicate the VRA's impact on relative black wages were larger in counties that were more-exposed to the CRA and federal affirmative action requirements, again consistent with the findings of Carrington, McCue, and Pierce (2000). Our main estimate is also more precise (significant at the 5 percent level).

Collectively, our analysis in this subsection does suggest that minority electoral power contributed to black Americans' improved labor market standing through its effect on enforcement of existing laws. Local and state governments also directly enacted separate policies designed to improve the earnings of black workers. One common form of local affirmative action conditioned state contracts on the employment of women and minorities (Nay and Jones 1989).[58] Existing evi-

[57] Further details on data construction are provided in Appendix A. The source data is the U.S. County Business Patterns, which was published irregularly between 1949 and 1964, but annually after that.

[58] Other policies at the state and local level likely had indirect effects on minority economic status. For example, in the 1980s, the Georgia legislature provided tax breaks to government contractors who employed black workers and subcontracted with black-owned businesses. 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). We consider this channel by testing for

dence suggests that local-level AA programs are more likely to be enacted in cities governed by an elected mayor rather rather than an appointed city manager (Santoro and McGuire 1997).[59] This is because elected mayors are more subject to pressure from black voters than appointed bureaucrats, and so are more likely to use AA policies (and by extension, contracts) as a form of redistribution. In Section 7, we show that the VRA did indeed have a stronger effect in cities governed by mayors, who do in fact face electoral incentives.

6.3 Access to Public Assistance Programs

In Section 3, we discussed another potentially complementary role of minority voting rights: facilitating the access to President Lyndon Johnson's War on Poverty programs. During the 1960s, Congress enacted statutes that launched Medicare and Medicaid, as well as increased access to food support (the Food Stamps Program), Social Security, and other welfare benefits. As the seminal papers by Smith and Welch (1989b) and (Donohue and Heckman 1991) point out, the income support provided under the War on Poverty exerted a strongly-upward pressure on the earnings of black workers, who were disproportionately low-income. Relatedly and relevant to our examination here, research in political science, economics, and sociology suggests that political mobilization after the VRA shaped the implementation of these social programs (Andrews 2001). Recent work by Bailey and Duqette (2014) show that funds to fight poverty under the Great Society program were allocated in a manner consistent with the goals of fighting racial discrimination: poorer areas and those with a greater share of nonwhite residents received systematically more funding. We thus test for this channel. Appendix Table C.27 provides evidence consistent with improved access to social assistance being another channel through which black voting power operates to improve black workers' economic wellbeing (detail of this exercise are discussed at greater length in the appendix).

6.4 Changes in Human Capital

The VRA may also have improved black earnings by improving the skills (and hence the earnings prospects) of black Americans. Several studies have documented how franchise expansions in the U.S. 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.

the effect of the VRA on relative black entrepreneurship, as proxied by self-employment. We find modest evidence that the VRA did, in fact, increase high-skilled entrepreneurship (i.e., self-employment for those with at least a high school degree).

[59] There is to our knowledge no comprehensive data source on local and state AA programs.

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 (Figure VII). Second, when we directly test for educational changes using DEC data on workers' educational attainment, we detect no statistically significant effect on black-white worker education gap (Table C.28, Columns 1–3). Third, we reestimate our primary specification while accounting for the VRA's impact on education and experience. To do this, we include interaction between the VRA and education/experience in Equation 3. Results are presented in Columns (4)–(6). If the VRA was affecting wage inequality through its effect on black workers' human capital, we would expect these interactions to partly absorb our main effect. Instead, we observe is virtually no change in the parameter of interest, $VRA \times Black$. While this test is imperfect given that we are controlling for an outcome, the results are nevertheless suggestive that the VRA's effect on black wages is not arising (at least exclusively) throughout improvements in human capital. Finally, in Columns (7) and (8), we find no discernible effect of the VRA on the composition gap of minority workers that have either a high school or college-level education.

7 How Does Minority Political Power Operate?

We close our analysis by considering how the VRA changed politics itself to make government responsive to previously-neglected black workers. Recent studies suggest two main candidates that may describe how political forces shape economic outcomes in our setting. Models of distributive politics suggest that policy choices reflect the preferences of the electorate, and in particular, the "median voter" (Downs 1957; Meltzer and Richard 1981; Lindbeck and Weibull 1987; Cox and Mccubbins 1986). Consistent with these models, Cascio and Washington (2014) show that larger post-VRA shifts in economic redistribution in counties with higher black population shares. The intuition is that in counties where 40 percent of the voting population is black, politicians will be more likely to respond to minority redistributive demands than in counties that are only 5 percent black.

We probe this channel by estimating a regression that tests for greater government responsiveness (i.e., sharper reductions in the black-white wage gap) in counties with higher black population shares. Column 6 of Table III provides evidence suggestive of this mechanism for our primary earnings effects, in the spirit of Cascio and Washington (2014). The coefficient on the term VRA $\times$ Black $\times$ %PopBlack is positive and significant, suggesting that black workers benefited from enfranchisement relatively more in jurisdictions where elected officials faced greater electoral incentives from black constituents. We observe a similar heterogeneous effect for public sector

employment (final column of Table VI).

The basic median voter model also suggests a sharp change in effect when a place is majority-black. As such, we also test for nonlinearities in the heterogeneous strength of minority voting power by dividing counties into 10 percent bins by black population shares.[60] Figure VIII graphically displays results consistent with the distributive politics channel. For counties that have small black population shares (i.e., less than 20 percent black, which is roughly the median in our sample), the VRA's effect on the earnings gap is relatively small. Between 20 and 40 percent black population shares, the positive effects increase. Finally, 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. While this finding is suggestive due to potentially correlated unobservables, it is consistent with black voting power tipping sharply in places where black voters 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 percent of the local population).

We further corroborate the role of strong political incentives by using heterogeneity in local government structure. Recent research documents how the selection method for public officials has important effects on policy outcomes, including hiring and redistribution (Besley and Case 2003). City governments in the United States come in two main forms: (1) the mayor-council form, where a duly-elected mayor functions as the government's chief executive; and (2) the council-manager form, in which a city manager appointed by the local council runs the government. Because mayors face reelection incentives, they are more likely to engage in targeted redistribution, such as public sector employment, for political gain (Enikolopov 2014).

We thus test whether the benefits of minority political power are more heavily concentrated in mayor-council cities, using proprietary data from the International City Managers Association (ICMA), which conducts a nationwide survey to determine which cities are run by mayors vs. city managers. As shown in Table C.29, both wage and employment gains caused by the VRA are differentially larger in mayor-run cities – again consistent with our finding that changing electoral incentives after the VRA contributed to black economic progress.[61] This subanalysis is also consis-

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

$$\log(Y_{ict}) = \sum_{j=1}^{5} \alpha_j \left[\%\ \text{Pop. Black}_c = j\right] \times \left[\text{VRA}_{ct} \times \text{Black}_{ict}\right] + \mathbf{x}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icp(c)r(i)t} \qquad (9)$$

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 percent, $j = 2$ indicates a county where 1960 black population share is between 20 and 30 percent, and so on; $j = 5$ indicates a county where 1960 black population is over 50 percent.

[61] This finding also augments the work of Enikolopov (2014), suggesting that political selection structure affects not just the size but the *composition* of the public sector workforce.

tent with research in public administration suggesting that local affirmative action policies geared toward improving minority labor market opportunities are more likely to be enacted in mayor-council cities, as these are cities where politicians will be affected by the black electorate (Santoro and McGuire 1997). These results thus confirm further that the VRA complemented other labor market interventions of the civil rights period.

The "citizen-candidate" model of minority representation suggests an alternative (non-mutually-exclusive) political channel that rationalizes our results (Besley and Coate 1997; Pande 2003; Duflo and Chattopadhyay 2004). Under this theory of political influence, changes in politician identity lead to the implementation of minorities' preferred policies (in this case, the hiring of minorities to government jobs or the provision of government contracts that improve black workers' private sector prospects). Strong evidence provided recently by Beach, Jones, Twinam, and Walsh (2018) provide support for an "identity politics" model of representation. Using evidence from recent California city councils, the authors show that black council-members improve the quantity and quality of public goods in black communities.

Anecdotal evidence suggests that after 1965, the number of black Americans in elected office rose steadily (JCPES 2000). However, there is relatively 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 substantive representation, which might become manifest through improved minorities' socioeconomic outcomes. We test the plausibility of this channel by testing whether the VRA increased the presence of black officials in elected office differentially in covered areas. 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).[62]

Table VIII 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, as might be expected. Given data constraints, we cannot separate the mechanisms of descriptive representation and distributive politics. We do not believe, however, 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. Research within political science suggests 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,

[62]See PBS (2000).

also produce benefits for their communities. We demonstrate this in Table C.31. In summary, although the results in this section are primarily suggestive (i.e., we cannot effectively rule out all correlated unobservables for these tests of heterogeneity), we believe 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 as potential complementarities between political power and the enforcement of private sector labor market policy.

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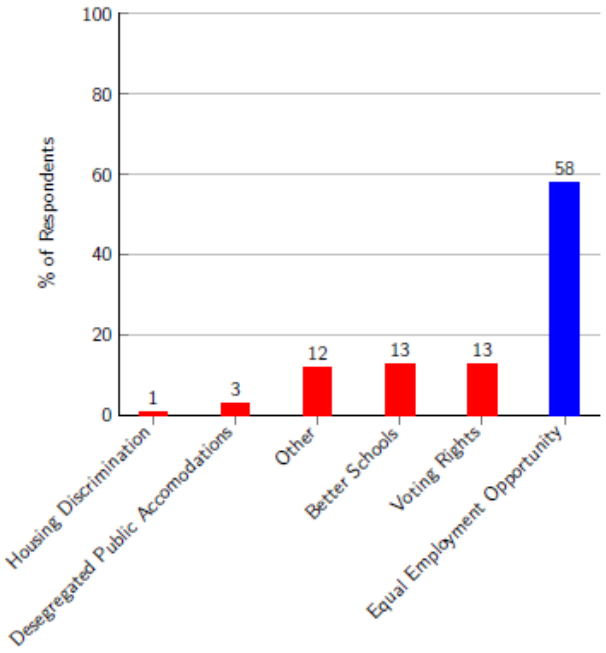
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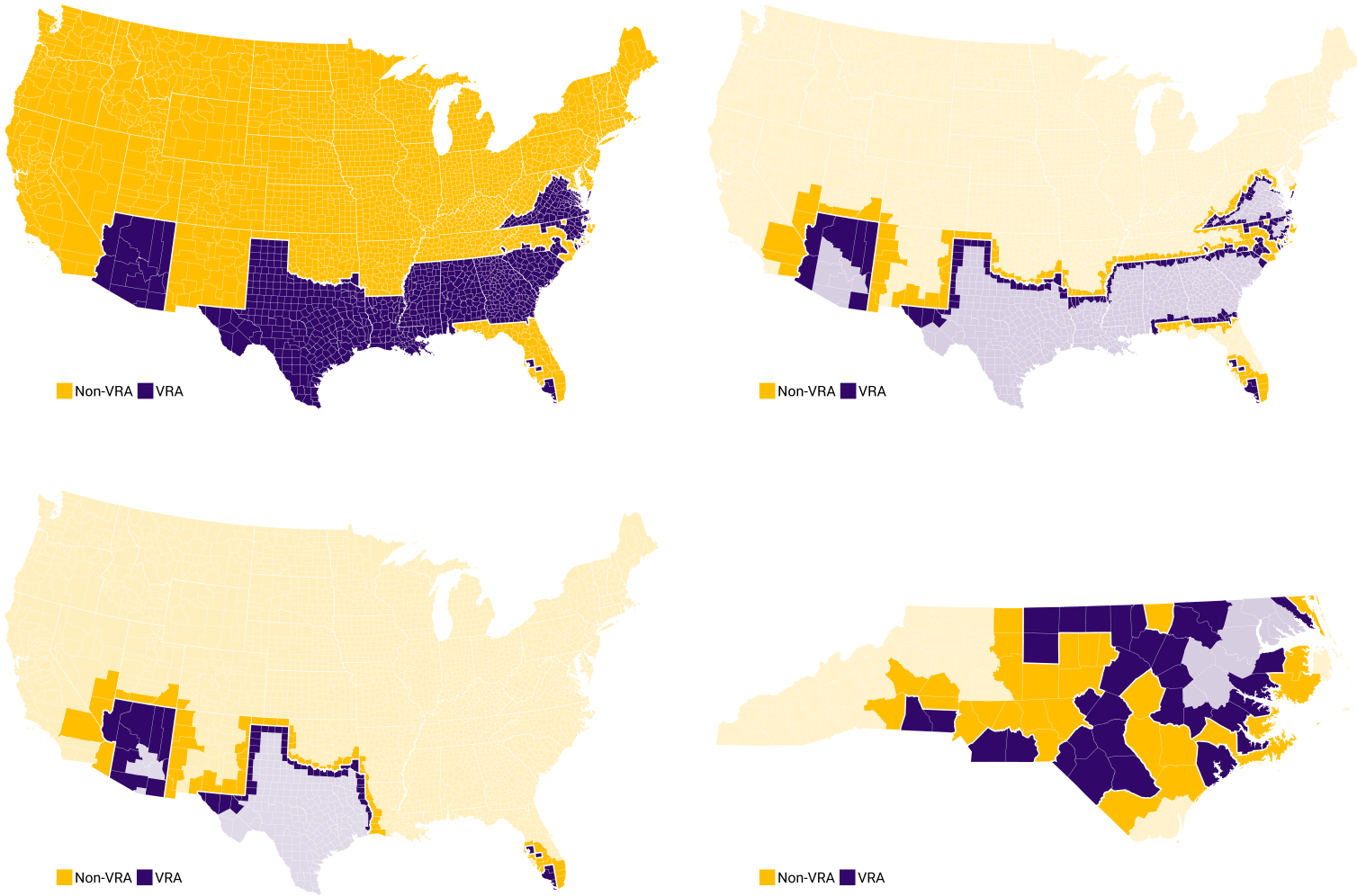
Figures

Figure I: Political Attitudes of Black Americans(1963) "Which Political Issue is Most Important to You?"



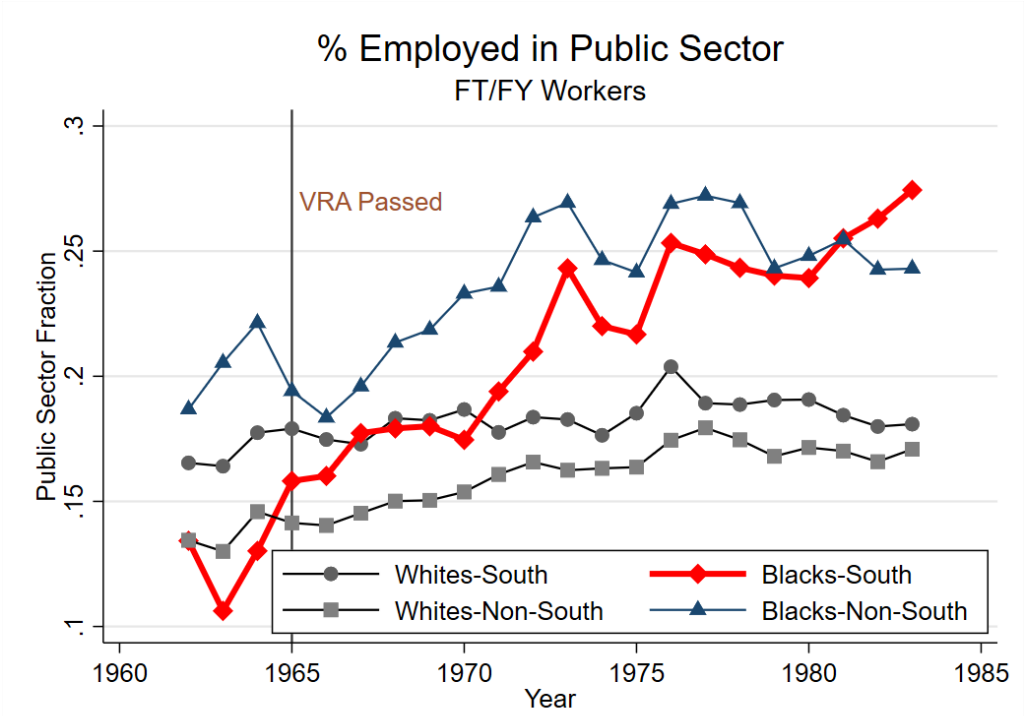
NOTES: Graph presents results from a national survey conducted by the National Opinion Research Center. Interviewers contacted black respondents, and asked them the following questions: "Here is a list of things that Negro groups working for equal rights frequently want. Which do you think is the most important to work for now?" Source: NORC Survey SRS-160 , May, 1963.

Figure II: VRA Covered Jurisdictions (Nationwide and County-pair Samples)



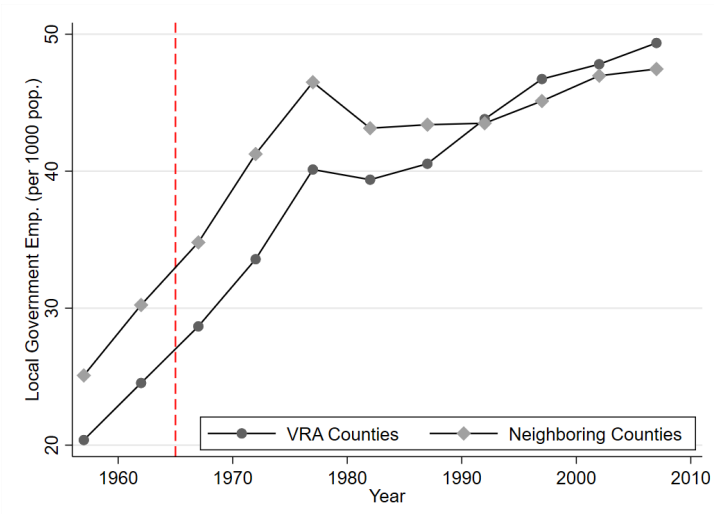
NOTES: The top-left figure indicates all counties that were/were not covered by Section 5 of the 1965 Voting Rights Act (VRA) during the period of analysis. The top-right figure presents the main sample of counties used for analysis. The bottom figures present the primary subsamples used to demonstrate the robustness of empirical findings. The bottom-left panel indicates 1975 VRA Amendments matched county pair sample, and the bottom-right panel indicates NC-only matched county pair sample. Source: U.S. D.O.J.

Figure III: Public Sector Workforce - By Race



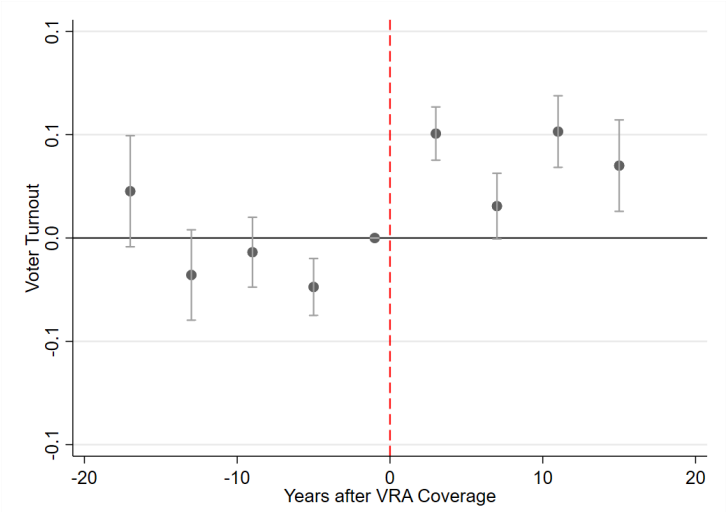
NOTES: Figure presents the fraction of full-time, fully-year workers employed as public workers, according to the Current Population Survey (CPS), by race and region. Source: CPS.

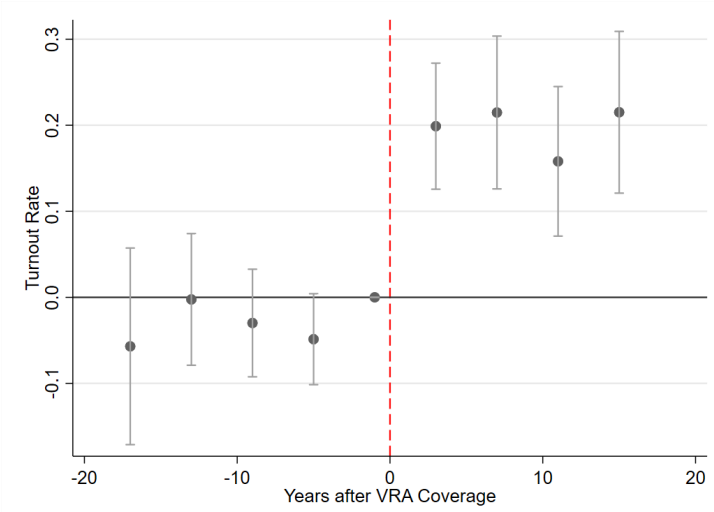
Figure IV: Local Government Growth, 1957-2007

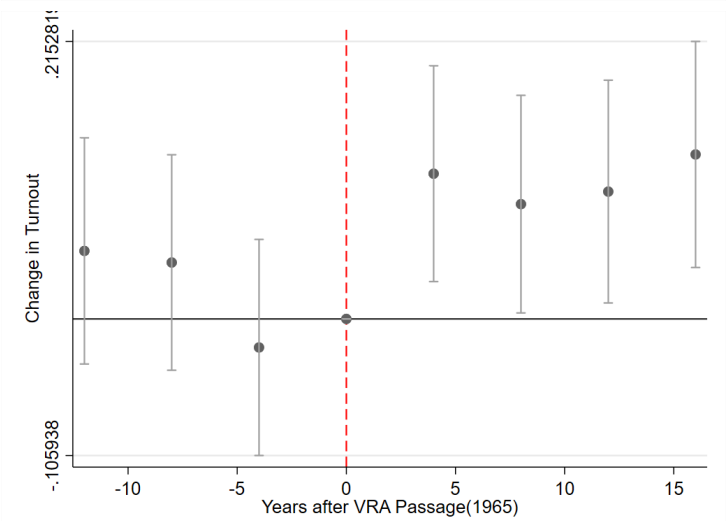


Notes: Figure presents the number of employees per 1000 people in for the sample of neighboring VRA and non-VRA counties, restricting to counties with populations of larger than 10,000. Source: U.S. Census of Governments (COG).

Figure V: First-Stage Effect: Impact of the VRA on Various Measures of Turnout

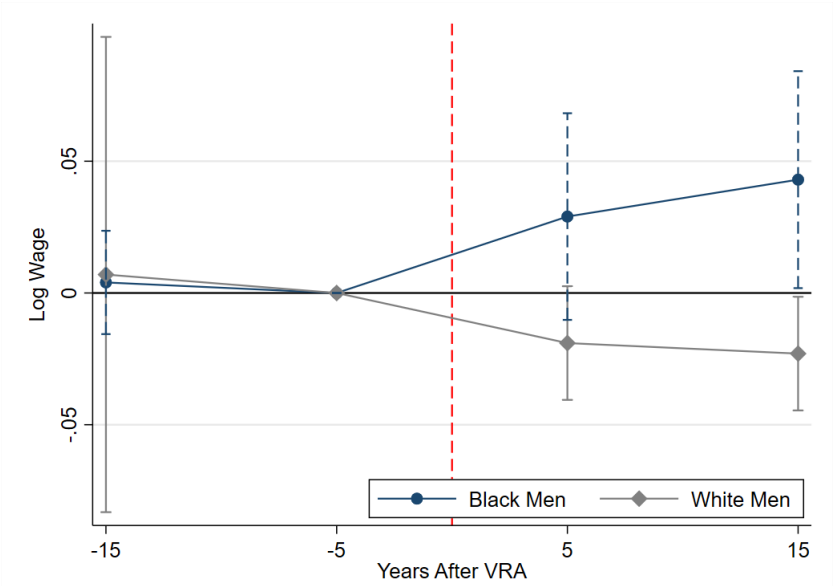






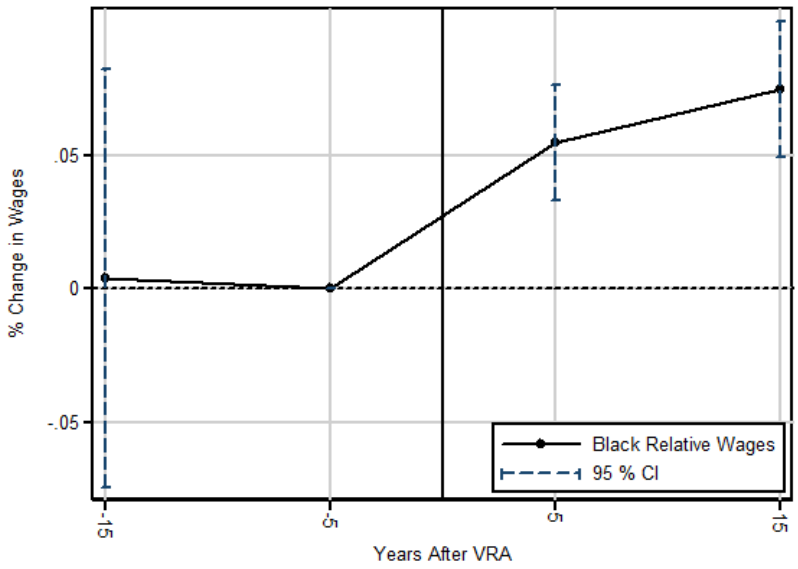
Notes: Figure presents event-time estimates of how VRA coverage interacted with pre-VRA black population relates to voter turnout. The dependent variable (y-axis) is the voter turnout in presidential elections, and the independent variable (plotted) is the interaction between between the ever-VRA-covered indicator, a year indicator, and the pre-VRA percentage of the population that is black within a county. 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 ICPSR and Gentzkow et al. (2011).

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



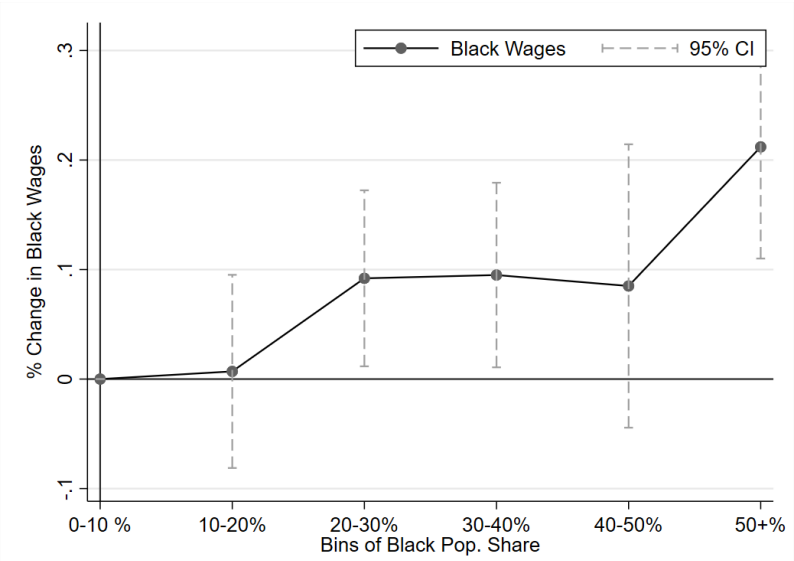
NOTES: Figure presents event-time estimates of how VRA coverage affects wages for black and white workers separately. Regressions include education and experience controls, county and county pair-year fixed effects, and baselines controls interacted with linear and quadratic trends. Estimates are normalized 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: 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: Summary Statistics - County Characteristics in 1960

	(1)	(2)	(3)	(4)
Variable	**Non-VRA Counties**	**VRA Counties**	**Mean Difference**	**P-value**
Interior Counties				
Median Age	29.09	26.50	2.59	0.00
% Pop. Black	0.07	0.27	-0.20	0.00
% Pop. Urban	0.26	0.32	-0.06	0.00
Median Income	3501.85	3409.05	92.80	0.21
Farm Share	0.23	0.20	0.03	0.00
% Employed	0.31	0.33	-0.02	0.00
% Employed Male	0.72	0.69	0.03	0.00
% Manufacturing	0.18	0.20	-0.02	0.05
% Trade	0.05	0.05	0.00	0.02
% 25 y.o.-HS Educated	0.28	0.28	0.00	0.61
Rep. Party Voteshare	0.62	0.67	0.05	0.00
Birth Rate	0.02	0.02	0.00	0.49
Net Migration	-0.18	-0.17	-0.01	0.35
Border Counties				
Median Age	27.92	25.98	1.94	0.00
% Pop. Black	0.17	0.20	-0.03	0.10
% Pop. Urban	0.31	0.33	-0.02	0.51
Median Income	3697.65	3764.80	-67.05	0.68
Farm Share	0.19	0.21	-0.02	0.34
% Employed	0.32	0.33	-0.01	0.11
% Employed Male	0.70	0.69	0.01	0.13
% Manufacturing	0.18	0.20	-0.02	0.25
% Trade	0.05	0.05	0.00	0.59
% 25 y.o.-HS Educated	0.29	0.29	0.00	0.84
Rep. Party Voteshare	0.61	0.63	0.02	0.08
Birth Rate	0.02	0.02	0.00	0.40
Net Migration	-0.13	-0.14	0.01	0.72

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

	Panel A: Voter Turnout (Presidential Elections)					
	(1)	(2)	(3)	(4)	(5)	(6)
VRA	.115	.104	.072	.061	.071	.046
	(.010)	(.009)	(.008)	(.012)	(.012)	(.011)
VRA × 1960 Black Pop. Share				.003	.002	.002
				(.000)	(.000)	(.000)
N	2651	2651	2651	2651	2651	2651
Controls		X	X		X	X
State Trends			X			X

	Panel B: Legislator Ideology (House of Representatives)					
	(1)	(2)	(3)	(4)	(5)	(6)
VRA	-.08	-.06	-.01	-.05	-.04	.03
	(.04)	(.03)	(.03)	(.06)	(.04)	(.03)
VRA × 1960 Black Pop. Share				-.13	-.12	-.06
				(.20)	(.15)	(.14)
N	1699	1699	1699	1699	1699	1699
Controls		X	X		X	X
State Trends			X			X

Notes: This table presents regression coefficients from 6 separate regressions per panel, one regression per column-row cell. Observations in Panel A are county-year, and observations in Panel B 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 the VRA (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 or district. 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 and either county or 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 Black Relative Wages, 1950-1980

	Panel A: Main Analysis (County Pair Sample)					
	(1)	(2)	(3)	(4)	(5)	(6)
VRA × Black	0.055	0.058	0.056	0.056	0.048	-0.033
	(0.027)	(0.027)	(0.28)	(0.28)	(0.35)	(0.030)
VRA × Black ×						0.338
1960 Black Pop. Share						(0.084)
County-level Controls		X	X	X	X	X
State Trends			X			
County Trends				X		
County-by-race Trends					X	
N	673000	673000	673000	673000	673000	673000

	Panel B: Subsample Analysis, 1950–1980			
	(1)	(2)	(3)	(4)
VRA × Black	0.058	0.071	0.046	0.114
	(0.027)	(0.035)	(0.032)	(0.038)
Sample	Full Sample	1965 VRA	1975 VRA	NC
County-level Controls	X	X	X	X
N	673000	530000	150000	180000

	Panel C: Spillover Effects, 1960–1980		
	(1)	(2)	(3)
VRA × Black	0.064	0.045	0.011
	(0.030)	(0.018)	(0.005)
Sample	County Pairs	Interior	Difference
County-level Controls	X	X	X
N	670000	3741000	670000

Notes: This table presents regression coefficients from ordinary least squares regressions relating the VRA to (relative) wages. Each column-row cell indicates a separate regression. An observation is an individual in a given Census year. The dependent variable is the log hourly wage, and the independent variable in all specifications (except Column (6) of Panel A) 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). In Column (6) of Panel A, the independent variable is VRA × Black × 1960 Black Pop. Share , which includes a further interaction for the 1960 fraction of the population that is black. 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 IV: 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)
County-level Controls	X	X	X	X
N	600	600	600	600

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 V: The Effect of the VRA on Additional Employment Outcomes

Outcome:	(1) 1(Have Income)	(2) 1(Not in Labor Force)	(3) 1(Unemployed)	(4) Hours Worked
VRA × Black	0.008	-0.005	-0.003	-0.402
	(0.011)	(0.014)	(0.007)	(0.603)
N	1,469,000	1,469,000	1,469,000	1,469,000
County-level Controls	X	X	X	X

Outcome:	(5) Weeks Worked	(6) 1(FTFY Worker)	(7) Log(Earnings)
VRA × Black	-0.953	-0.038	0.127
	(0.978)	(0.18)	(0.046)
N	1,469,000	1,174,000	1,469,000
County-level Controls	X	X	X

Notes: This table reports estimates of OLS regressions examining the impact of the VRA on black (relative) employment outcomes, focusing on labor supply. The dependent variable in each column is an indicator for either having positive income, not being in the labor force, for being unemployed, or for being a full-time, full-year worked as defined in the text, or total hours worked, weeks worked last year, or log total earnings in Columns (4), (5), and (7), respectively. All regressions include county baseline controls, county-year, county-race, and county pair-year-race fixed effects. Standard errors are in parentheses, 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; each is measured at the 1960 level and interacted with linear and quadratic time trends. Column (7) also controls for worker education/experience and hours worked last year. Column (3) omits workers who are not in the labor force, and Column (6) omits workers who are either in the labor force or are unemployed. ***,**, 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 Public Sector Employment, 1950-1980

	Panel A: Public Sector Employment					
	(1)	(2)	(3)	(4)	(5)	(6)
VRA × Black	0.038	0.020	0.038	0.027	0.027	-0.046
	(0.009)	(0.01)	(0.009)	(0.012)	(0.012)	(0.023)
VRA × Black ×						0.147
1960 Black Pop. Share						(0.062)
County-level Controls	X	X	X	X	X	X
State Trends			X			
Sample	Full CP	Full CP	Full CP	NC	NC	Full CP
N	673000	673000	673000	175000	175000	673000
	Panel B: 1960 Public Sector Wage Premium					
	(1)	(2)				
Public Worker	0.029	0.196				
	(0.003)	(0.009)				
Sample	White Only	Black Only				

Notes: Panel A of this table presents regression coefficients from 6 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 of interest in Columns (1)–(5) 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). In Column (6), the independent variable is VRA × Black × 1960 Black Pop. Share , which includes a further interaction for the 1960 fraction of the population that is black. 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 border county-pair (CP) sample, or the North Carolina-only (NC) sample. Panel (B) provides estimates from an OLS regression of log wages on an indicator for whether a person was employed by government. The sample here is restricted to Census respondents in 1960 (sample counts suppressed by the Census Bureau). ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table VII: 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)
Worker Sample	Private	Public
County-level Controls	X	X
N	153000	54500

Notes: This table presents regression coefficients from 2 separate regressions, one per column. Each coefficient is an estimate from an OLS regression relating the VRA to (relative) black wages, examining heterogeneity by quantiles of 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 of interest are the (2) interactions between VRA, the race indicator Black, and whether a respondent works in an occupation that is in either the first or fourth quartile of overall 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 VIII: 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
County-level 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.

Appendices

Part
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A Data Appendix

Individual-level earnings/employment (U.S. Decennial Censuses of Population): The primary data source for this project are the long-form Censuses of Population, accessed through the University of California Research Data Center (RDC). The advantage of these data is that they provide the rich set of worker outcomes for various cohorts with more detailed information for place of residence than is available within public-use Census samples, which are representative at the state level.

Voter turnout: County-level election data come from the Interuniversity Consortium for Political and Social Research (ICPSR) and Dave Leip's Atlas of US Presidential Elections.

County Characteristics: Data on county-level characteristics, including the share of the population that is black, the unemployment rate, the county percentage of 25 year old or more with less than a high school level-education, and percent urban/agricultural come from the U.S. County and City Data Book Consolidated File (County Data, 1947-1977).

Voter registration: These data come from reports of the U.S. Commission on Civil Rights and the NAACP Voter Education Project, both of which collected statistics to track changes in black political participation during the civil rights era. Although the registration data comprise only a subset of southern counties (and even smaller subset when restricted to our matched pair sample), we nevertheless examine these data to provide evidence that the VRA's effects were concentrated among black residents, as intended.

Legislative ideology: To test for the impact of the VRA on political responsiveness to constituent concerns, we use the DW-Nominate (DW-NOM) data. DW-NOMINATE is a multidimensional scaling technique which collapses legislative roll-call voting into a twodimensional ideal point. The first dimension of NOM is commonly considered to be the contemporary liberal-conservative measure (scaled from -1 to 1). The second dimension, which we examine in the main analysis (see Section 5), historically tracked policy issues that cut across party lines and relate to support for civil rights and other race-related issues.[63] Political scientists have argued that after 1980, the 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 Rogowski (2017). Results are available upon request.

Local public finances: Data on local public finances comes from the U.S. Census of Governments series for 1957, 1962, 1967, 1972, 1977, and 1982. The data on county government expenditure relate only to county governments and their dependent agencies, and do not include amounts for other local governments within or among county areas. We use data on total amount of direct expenditure on social welfare, as well as data on local intergovernmental transfers from both the state and federal governments (following Cascio and Washington (2014)).

Local-level form of government: Data on local-level government structure are based on survey data from the International City/County Municipal Association (ICMA). ICMA conducts regular surveys of US municipal governments regarding the number and type of council seats in each city. These surveys – which have been employed by Baqir (2002) and Trebbi, Aghion, and Alesina (2008) – are based on mail-in surveys administered by the ICMA to municipalities in the one or two years prior to publication. Surveys contain questions regarding forms of government. We use data based on the year 1981, since it is the only data available in electronic format. Because the data are at the city level (identified by Census FIPS place codes), we code a county as being either "mayor-council" or "single-member district" city councils if the cities for which there is data comprise more than half of the overall county's population.

[63] The DW-NOM score is also correlated with ideological measures derived from campaign finance data (Bonica, 2014) as well as Congressional speech data – including *after* controlling for political party (Gentzkow et al., 2016).

Black elected officials: Data on the number/share of black elected officials (by type of office) come primarily from our digitization of various additions of the the National Roster of Black Elected Officials, published by the Joint Center for Political and Economic Studies (JCPES). The JCPES annual "Black Elected Officials: A National Roster" was published each year beginning in 1969.[64] 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.[65] The JCPES data contains the number of elected officials at all levels of government and we focus on city and county-level politicians. Because places vary in the number of elective offices, we collect the total number of elected office officials at county level as reported by the Census of Governments. For whether the VRA increased the number of black politicians, we digitized data from various volumes published by the Joint Center for Political and Economic Studies.

Proxies for civil rights activism: We proxy for black political activism using an indicator for the presence of an NAACP chapter, another black race organization, and a black college in the county. These data are compiled by Matthews and Prothro (1963), and were provided to us by Jim Alt.

Federal contractor presence: We created county-level exposure to contractors using data from the EEOC's EE0-1 reports digitized for the year 1968 (the first year in which we could determine whether establishments were the recipients of federal contracts). To our knowledge, this is the first time these data have been used.

[64] In the process of conducting 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.

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

B Conceptual Framework: Political Power, Public Sector Employment, and Earnings Disparities

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, following the standard Mortensen-Pissarides framework. We augment the standard model to account for redistributive pressure among two groups of voters by incorporating hiring in the public sector.

Public sector and social 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 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 the 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:

$$\frac{\gamma}{q(\theta)} = \frac{p - w}{r + \delta} \tag{B.10}$$

$$w = \frac{\gamma\beta}{1 - \beta}\frac{r + \delta + m(\theta)}{q(\theta)} - m_g\frac{b - w_g + \theta\frac{\gamma\beta}{1-\beta}}{r + \delta + m_g} + b \tag{B.11}$$

Equation (B.10) is the familiar job creation condition. As expected, labor market tightness decreases with wage and increases with the productivity level. Equation (B.11) is the wage equation and can be rewritten as:

$$w = \underbrace{\frac{\gamma\beta}{1 - \beta}\frac{r + \delta}{q(\theta)}}_{\text{Surplus Distribution}} + \underbrace{\frac{r + \delta}{r + \delta + m_g}\Big(\overbrace{b}^{\text{Reservation Utility}} + \overbrace{\theta\frac{\gamma\beta}{1 - \beta}}^{\text{Private Sector Gains}} \Big) + \frac{m_g}{r + \delta + m_g}\overbrace{w_g}^{\text{Public Wage}}}_{\text{Social Planner Component}} \tag{B.12}$$

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's Optimization Problem

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 (B.10), 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 whites, 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.*

C Additional Results – Including Figures and Tables

Summary of Additional Analyses:

C.1 Summary Statistics

C.2 Additional First-stage Results – Turnout, Registration, & Representation in Congress

In this subsection, we provide several robustness checks of our first-stage effects on turnout and politician responsiveness (described in the main text).

C.2.1 Effects on Presidential Turnout - All Counties

C.2.2 Effect of the VRA on Turnout for Congressional Races

We demonstrate the validity of our first-stage effect on turnout by estimating Equation 2 using data on voter turnout for Congresional contests. Results are presented in Table C.3, and corroborate our main findings. Columns (1)–(3) examine the border county sample, first without controls (beyond fixed effects), then adding baseline controls, and finally adding state linear trends. Columns (4)–(6) repeat this exercise for the set of all counties in the sample states used in our analysis. Columns (7)–(8) examine heterogeneity by initial black population share, confirm that turnout is higher in "more treated" counties (i.e., counties where more of the population is able to vote after VRA passage).

C.2.3 Effect of the VRA on Black & White Voter Registration

It would be ideal to use voter registration by race as our main proxy of black political empowerment in the main analysis. Unfortunately, registration data are both infrequent and missing for a large number of Southern counties (and some entire states). Nevertheless, for the years 1960, 1964, 1966, and 1968, we have measures of registered individuals and the relevant voting age population for white/non-white persons, and use it accordingly to provide additional evidence of political empowerment in Table C.4. Sources include: United States Commission on Civil Rights and the Voter Education Project. This data was provided to us by James Alt, and are the data used in Alt (1994).

C.2.4 Impact of the VRA on Legislator Ideology

In the main text we show that the VRA led to greater support for civil rights legislation, consistent with recent work by Schuit and Rogowski (2017). One might also expect, though, the VRA to change the overall (Democrat vs. Republican) political ideology of elected congressional representatives, as measured by the first dimension of the DW-NOMINATE score. The first dimension is commonly understood to be a measure of *overall* conservatism (scaled from -1 to 1), rather than a racially-oriented measure. As Table C.5 indicates, 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)).

C.2.5 Effect of VRA Examiners:

As we described in Section 2, there are several sections that comprise the VRA statute – many of which had the potential to affect politics, and in turn government action and redistribution. Section 2a, which gives citizens a right of action to sue for voting discrimination, applied nationwide. Section 2b (the provision of the VRA analyzed by Cascio and Washington (2014)) eliminated Jim Crow literacy tests in all or parts of 6 southern states. Section 5, however, applied to all of the

2b states, as well as several counties in states that did not employ literacy tests as of 1964. Its coverage was also expanded in both 1970 and 1975. Moreover, any covered jurisdictions under Section 5 were automatically subject to Section 6 and 7. These provisions of the VRA gave the federal government the authority to send federal examiners into covered jurisdictions to enforce constitutionally protected voting rights. Election law scholars have long argued that the combined threat of both preclearance and active federal intervention into elections was a powerful deterrent against racial discrimination in voting.

The bundled nature of the VRA treatment makes isolating the impact of Section 5 challenging. To provide evidence that this specific provision itself had a measurable effect on Southern politics, we examine the effect of the use of federal examiners, which was limited to a subset of counties within the set of Section 5-covered jurisdictions. We use data from Alt (1994) indicating where examiners were sent *within* the South. Results are presented in Table C.6. In all three regressions, we find that the federal government sending examiners had an *additional* effect beyond Section 5 coverage. As such, to the extent one is concerned that the observed effect of Section 5 is due entirely to the removal of literacy tests, the results here cast doubt on that view. Election examiners are a form of more active state intervention than removal of a disenfranchising device that clearly had an (additive) mobilizing effect for black voters.

C.3 Additional Reduced-form Results – Earnings and Employment (including Additional Tests of Heterogeneity

C.3.1 Robustness - Main Effects

In this subsection, we examine the effect of the VRA on hourly wages for both black and white workers using a simple DD framework. As we see below in Table C.7, after the VRA takes effect, we observe a mean increase in the wages of black Americans (close to 5 p.p., significant at the 5 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), these results suggest that the VRA did indeed improve racial equality within the labor market; these results are stable to multiple potential specifications as shown by Columns (1) and (2). Unsurprisingly, the main effect (Panel C) is approximately equal to the additive effect of rising black wages (Panel A) and stable/slightly declining white wages (Panel B).

C.3.2 Is the VRA Leading to Leveling Up or Surpassing?

As our main results indicate, the VRA does not lead to a complete closing of the racial earnings gap. Some may be interesting in knowing, though, whether the VRA is leading to a catch-up of Deep South areas to its neighbors, or an improvement of conditions *beyond*. We provide evidence of the former in Table C.10 below, in which we examine the heterogeneous treatment effect by a county's initial level of racial resentment. In Columns (1)–(2), we examine whether our effects are stronger in places where a larger fraction of black agricultural workers worked as sharecroppers rather than landowning farmers. This a proxy for lack of initial wealth – and the results indeed suggest that the effects were stronger in counties where black workers were unlikely to have any meaningful wealth. In Columns (3)–(4), we find that the effects of the VRA are stronger in places that likely had more intense racial animosity, as proxied for by the number of lynchings in a given county as calculated by Tolnay and Beck (1995), who collected data on every lynching nationwide after Reconstruction and through the 1930s. Finally, in Columns (5)–(6), we test whether the effects of the VRA were stronger in places that were already adopting civil rights-friendly policies, as proxied for by whether the county school system was already desegregated prior to 1960s. We find that the effects are stronger in places where schools remained segregated. Collectively, these results suggests that the VRA was facilitating catch-up of more discriminatory places to less-discriminatory places.

C.3.3 Section 5 vs. Section 2: Heterogeneous Effects by Election Examiner Presence

Corresponding to Section C.2.5 above, we show in this section that the economic effects of the VRA are also stronger in areas where the federal government took affirmative steps (using federal election examiners) to ensure black voter participation. In Table C.8, we show that in terms of both wages and public sector employment, the VRA has differentially larger effects in covered counties where there were also examiners sent (Columns (1) and (2)). The use of examiners also has statistically significant average treatment effects (Columns (3) and (4)).

C.3.4 Section 5 vs. Section 2: Heterogeneous Effects by Urban Population

C.3.5 Controlling for Institutional Differences

Differences in state-level human capital-building institutions within the South may confound out estimates (Card and Krueger 1992). In this section, we show that even allowing for such differences do not affect our main conclusion. Results from numerous additional specifications are provided in Table C.12 below. To address the possibility that returns to education differed dramatically between Southern states, as suggested by Card and Krueger (1992), we allow for different returns to human capital (education and experience) by state (Column 1), as well as by race (Column 2). We then allow the returns to human capital to vary by both race and geography (Column 3). 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.

C.3.6 Extending Sample to Include Men and Women

Our analysis in the main text focuses on how the VRA changed the wage outcomes of black men, in order to facilitate comparison with the previous literature (Smith and Welch 1989b; Donohue and Heckman 1991). However, a large literatures similarly documents historical employment discrimination against black women during the first half of the twentieth century, as well. In Table C.13, we show that the VRA raised relative earnings for men and women. When we expand the sample to include all male and female workers, we confirm that the black-white gap declines by around 5 log points, consistent with our main findings.

C.3.7 Robustness of Subsample Analysis

In Table C.11 below, we probe the robustness of the main findings when limiting to various subsamples of our data, determined by time and place of coverage.

C.3.8 Robustness of North Carolina Analysis

In this subsection, we probe the robustness of the North Carolina results in particular. This follows the work of Fresh (2018) and Thompson (1986); the former uses a generalized DD approach, and the latter uses a matching procedure and the differential application of coverage within North Carolina. In Table C.11 below, we probe the robustness of the main findings when further limiting the North Carolina subsample, in the spirit of a regression discontinuity design (RDD). NC counties became protected based on having election turnout of below 50 percent in 1964. As Fresh (2018) argues, the small sample of counties does not allow for the use of an RDD approach. Nevertheless, we estimate the local average treatment effects (LATE) for those counties close to the 50% threshold for coverage to assess the stability of the main results given the known assignment-to-treatment process. We estimate the optimal bandwidth to be around 7%. Focusing on counties "close" to this threshold, we document similar findings in Appendix Table C.14. In conducting this LATE analysis, we ran into Census disclosure problems, given that we were limiting to a small number of counties, with thinner cells. As such, we had to make a few modifications to the sample in

order to disclosure our results here. First, we expanded the sample to include *all* workers with positive earnings, rather than simply full-time, full-year workers. Second, we expanded the sample to include men and women. Given the evidence provided in the main text that our core findings are not driven by these sample choices, we interpret the results in this subsection as being supportive of our primary findings, as well. As Column (1) and (2) show, the VRA increased relative wages in North Carolina by about 12 p.p., in line with our main estimates. Moreover, the VRA increased the likelihood of public employment for black men by 4–4.5 p.p. (a slightly larger effect than our main N.C. public sector estimate). As the results indicate, these results are unchanged when we expand the bandwidth slightly (to =/- 10% of the 50% threshold).

C.4 Confounders related to Cross-Border Spillovers

Here we present numerous robustness tests to rule concerns related to cross-border spillovers. First we repeat our test for whether the VRA affects population characteristics using individual level data. We also show that the effects are similar when restricting the sample to black respondents (see Tables C.15 and C.16).

C.4.1 Migration

To examine whether population movement due to the VRA is either a confounder or a mechanism driving our rests, we examine the effect of the VRA on migration status as defined in the long-form Census (Table C.18). Specifically, we re-estimate Equation 2, but use as the 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 $\times$ Black. Given that such movement is not driven by positive selection (Column 4), we believe it is most likely that immigration of black workers would lead us to underestimate our effects.

C.4.2 Commuting

Another plausible channel through which the VRA may be affecting wages can be through differential commuting patterns and compensation. Black workers in untreated places were more likely to commute to VRA-covered due to improved economic opportunities. These can appear through two different margins, an increase or decrease in the relative number of commuting workers, and differences in wages for commuting workers. In addition, changes in commuting patterns can have implications for our understanding of how the VRA affected the bargaining position of black and white workers. Table C.19 shows our analysis of the relationship between the VRA and a worker's commuting status (indicating whether a worker is commuting from residence in a VRA to a non-VRA location, or vice versa). Column (1) shows the effect of VRA $\times$ Black on the likelihood of being a commuter, while column (2) shows heterogeneity in the effects of the VRA on relative black wages along commuter status. There are two main takeaways. First, the VRA does not affect the relative commuting status of blacks relative to whites – the coefficient in column (1) is small and statistically insignificant. Second, commuting status does not appear to mediate the effect of the VRA on the relative wages of blacks. There does appear to be a modest (although statistically insignificant) increase of about .03 log points for commuting status workers following the passage of the VRA. Taken together, these two takeaways imply that: (1) blacks commuting from a VRA-covered place of residence benefit from the passage of the VRA, regardless of VRA status of their workplace, suggesting increases in bargaining power survive statelines; and (2) the

bargaining power of commuting white workers does not appear to be affected by passage of the VRA, as following passage of the VRA they do not commute to non-VRA places for a lower wage (and arguably command a slightly higher wage).

C.5 Impact of the VRA on Extensive-margin Employment Measures - Robustness

C.6 Impact of the VRA on Public Sector Employment

C.6.1 Additional Robustness Checks

Table C.20 below provides numerous robustness checks as described in Section 6.1.

C.6.2 Impact of the VRA on Overall Levels of Public Sector Employment

A lingering question raised by our results in Table VI in the main text is whether the improved likelihood of black workers' employment in government is due to politicians expanding the of the bureaucracy, or channeling new hires toward black instead of white workers. Studies by Enikolopov (2014) and Alesina, Baqir, and Easterly (2000), for example, suggest that political factors can influence the size of government; these studies say little about the demographic composition of the public sector, and how it is influenced by political factors. In this section, we provide evidence suggesting that black workers' increased likelihood of public sector employment is *not* due to differential growth of government across VRA and non-VRA counties. To test this, we rely on data on public employment from the U.S. Census of Governments (COG), which reports data for all local governments at 5-year intervals (for years ending in 2 and 7). We use COG public employment data for 1962 to 1982 (the earliest year available). Data are county aggregates, representing all local governments in a county area. According to Berry et al. (2005), one advantage of this type of aggregation is that can ignore shifting functional responsibilities across types of local governments over time.

We estimate Equation 2, where the outcome is now the fraction of total population that is employed in government at the county level (multiplied by 1000). Results are presented Appendix Table C.22, indicating a precisely estimated zero effect of the VRA on government size.

C.6.3 Effect of the VRA on Public Sector Earnings

Only after the Civil Rights revolution began did black Americans working for government agencies begin to receive better pay, as well as other benefits – including a steady pension, long-term job security, and regularly scheduled opportunities for upward mobility to professional and managerial jobs. Building political pressure to enforce equal opportunity in the public sector may have also led to better pay *within* these government jobs–either through promotions or reductions in discrimination within jobs. As such, the impact of minority hiring in the public sector may come not only by improving the likelihood of employment but also by improving minority public workers' wages. We test for the impact of political empowerment directly by modifying our wage regression to account for heterogenous effects of the VRA on public and private sector workers. Specifically, we modify our wage regression to account for heterogenous effects of the VRA on public and private-sector workers. We estimate the following specification:

$$\log(W_{ict}) = \beta_0 + \beta_1(\text{VRA}_{ct} \times \text{Black}_{ict}) + \beta_2(\text{VRA}_{ct} \times \text{Black}_{ict} \times \text{Public}_{ict}) + \beta_3(\text{VRA}_{ct} \times \text{Public}_{ict}) + \\ + \beta_4 \text{Public}_{ict} + + \mathbf{X}_{ict}\boldsymbol{\gamma} + \mu_{cr(i)} + \mu_{ct} + \mu_{p(c)r(i)t} + \epsilon_{icr(i)p(c)t} \quad \text{(B.13)}$$

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.

Results for this test of heterogeneity are presented below (in Table C.23). Summing up the coefficients on VRA × Black is 0.02, suggesting that VRA coverage reduced the wage gap by around two percentage points, less than the private sector wage gap. These estimates are consistent with anecdotal evidence of declining racial disparities even *within* the public sector.

C.6.4 Quantifying the Contribution of Public Sector to Black-White Wage Convergence

Having had highlighted how the public sector can raise minority income in both the public and private sectors, we could 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?

Using the framework above, 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 (B.14), which is a rewritten version of Equation (B.12), for blacks and whites independently, after approximating the weights in the government-driven component of the wage using second-order Taylor expansions:

$$\begin{aligned}\log(Y_{ict})^{Private} &= \alpha_0 + \alpha_1 Black_{ict} + \alpha_2 \overline{PubEmp}_{ct,black} + \alpha_3 \overline{PubEmp}_{ct,white} \\ &+\alpha_4 \overline{PubEmp}^2_{ct,black} + \alpha_5 \overline{PubEmp}^2_{ct,white} + \alpha_6 \overline{PubEmp}_{ct,black} \times \overline{\log(W_{ct,black})}^{Public} \\ &+\alpha_7 \overline{PubEmp}_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_8 \overline{PubEmp}^2_{ct,black} \times \overline{\log(W_{ct,black})}^{Public} \\ &+\alpha_9 \overline{PubEmp}^2_{ct,white} \times \overline{\log(W_{ct,white})}^{Public} + \alpha_{cont} X_{ict} + \epsilon_{i,c,p(c),t}\end{aligned} \tag{B.14}$$

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:

$$\frac{Var\big(\beta_1'(VRA_{ct} \times Black_{ict})\big)}{Var\big(\beta_1(VRA_{ct} \times Black_{ict})\big)}. \tag{B.15}$$

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

C.6.5 Public Sector Growth, by Occupation

C.7 Testing Legal Enforcement Channels (i.e., labor market regulation)

C.7.1 Background on Anti-discrimination Laws Regulating the Labor Market

Title VII of the CRA outlawed race-based discrimination in pay, hiring, and promotions. The ability of government to eliminate economic discrimination thus depends not only on the passage of anti-discrimination laws, but also their enforcement.[66] Motivated by the aforementioned findings in political science and public administration, we thus examine whether the VRA complemented anti-discrimination and affirmative action laws. 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).

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; its primary tasks remain today to investigate, conciliate, and litigate charges of employment discrimination. The agency receives and investigates claims of discrimination on behalf of charging parties. Moreover, affirmative action policies (AA) – adopted at all levels of government – encouraged (or even required) minority hiring. 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 OFCCP historically ensured private compliance with affirmative action mandates.

In this section, we thus examine whether the VRA complemented anti-discrimination and affirmative action laws. Given the delegation of enforcement authority to political agencies, the ability of government to eliminate economic discrimination depends not only on the passage of anti-discrimination laws, but also their enforcement. The predicted complementarity between minority political power and economic outcomes is based on research (mainly within labor history and sociology) documenting that the implementation of anti-discrimination policy was historically a "politically mediated" process, dependent on political actors (Stainback, Robinson, and Tomaskovic-Devey 2005). Qualitative research supports this hypothesis. Just after the VRA was passed, from 1966 through the early 1970s, the EEOC investigated nearly 80,000 complaints of employment discrimination – filed largely in the South, by political groups such as the NAACP (Minchin 2015).[67]

As we discuss in Section 5, though, there is currently no readily-available data that allows us to examine how political influence due to 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. We use a few different sources of cross-sectional variation in the (assumed) likelihood that employers are subject to federal civil rights regulations. Under Title VII, the mandatory reporting of workplace racial composition only applied to establishments with more than 25 employees (this was later changed to 15 employees). Assuming this threshold to

[66] Indeed, it was in part the paucity of work on black political mobilization that led Donohue and Heckman to conclude (1991: 1641): "[f]uture work will have to explore more carefully the mechanism by which the Federal antidiscrimination framework translated the command of law into significant black economic advance."

[67] 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.

be a proxy for government oversight, we construct proxies for enforcement exposure based on the size distribution of establishments using data from the 1962 County Data Books. We first construct the fraction of small establishments:

$$\text{Frac. Small Est.} \equiv ((\#\text{ Est.}, \lessapprox 20 \text{ emp.})/ \text{ (Total Establishments)}),$$

as a proxy for the fraction of the county workforce that was *least* subject to monitoring under equal employment law.[68] Using this proxy, we test for heterogeneity in the impact of the VRA in the likelihood of enforcement. We also construct a similar measure testing for the present of *large* employers – which were *more* likely to be monitored by the federal government. As expected, Column (2) indicates, the pattern is reversed when using heterogeneity by fraction of establishments with more than 100 employees; the coefficient of interest, on the interaction VRA×Black×Frac. Large Est., is positive, suggesting that black relative wages are higher in counties where firms were more likely to be targets of EEOC enforcement actions (Carrington, McCue, and Pierce 2000).

Finally, given the coarseness of the previous two measures of anti-discrimination regulation exposure, we attempt to sharply identify this complementarity by estimating the distribution of workers just above and below the cut-off for federal anti-discrimination oversight. 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 a 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 on being an SME (less than 100 employees) is 12.3 percent. The probability of being a medium firm conditional on being an SME is 15.2 percent. Also from (1), small firms constitute 82 percent of all SMEs, and medium firms the remaining 18 percent. This means that 15 percent of all small firms and 84 percent are affected by the EEOA.
(3) Finally, we define our CRA penetration measure (TitleVIIExposure in Table C.25) as:

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

Using this measure, we estimate our cleanest test of heterogeneity, which is presented in Column (4) Table C.25. As discussed in the main text, the coefficient on VRA×Black×TitleVIIExposure indicates that minority voting power did lead to more aggressive enforcement of labor market regulations in a manner that benefited black workers.

C.8 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

[68] The source data is the U.S. County Business Patterns, which was published irregularly between 1949 and 1964, but annually after that.

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 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 3, with the natural log of the occupational score instead of income.[69]

Results are presented in Table C.26. 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 within 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.

C.9 Redistribution and Public Assistance as a Mechanism

In this section, we provide further evidence that the VRA affected policy outcomes in favor of black voters – focusing on access to the safety net. Unfortunately, similar to the data for voting, there is not to our knowledge race-specific data on public assistance. We thus test whether the VRA had larger effect on the disbursement of social welfare benefits in areas with larger black population shares (i.e., areas that were "more affected" by the VRA). As shown in Appendix Table C.27, the effects of the VRA are larger in areas with more potential black voting power.

[69] The estimating equation is thus:

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

C.10 Human Capital as a Mechanism

C.11 Political Mechanisms - Additional Results

C.12 Additional Evidence of Post-VRA Government Responsiveness to Black Voters' Interests

C.12.1 Robustness

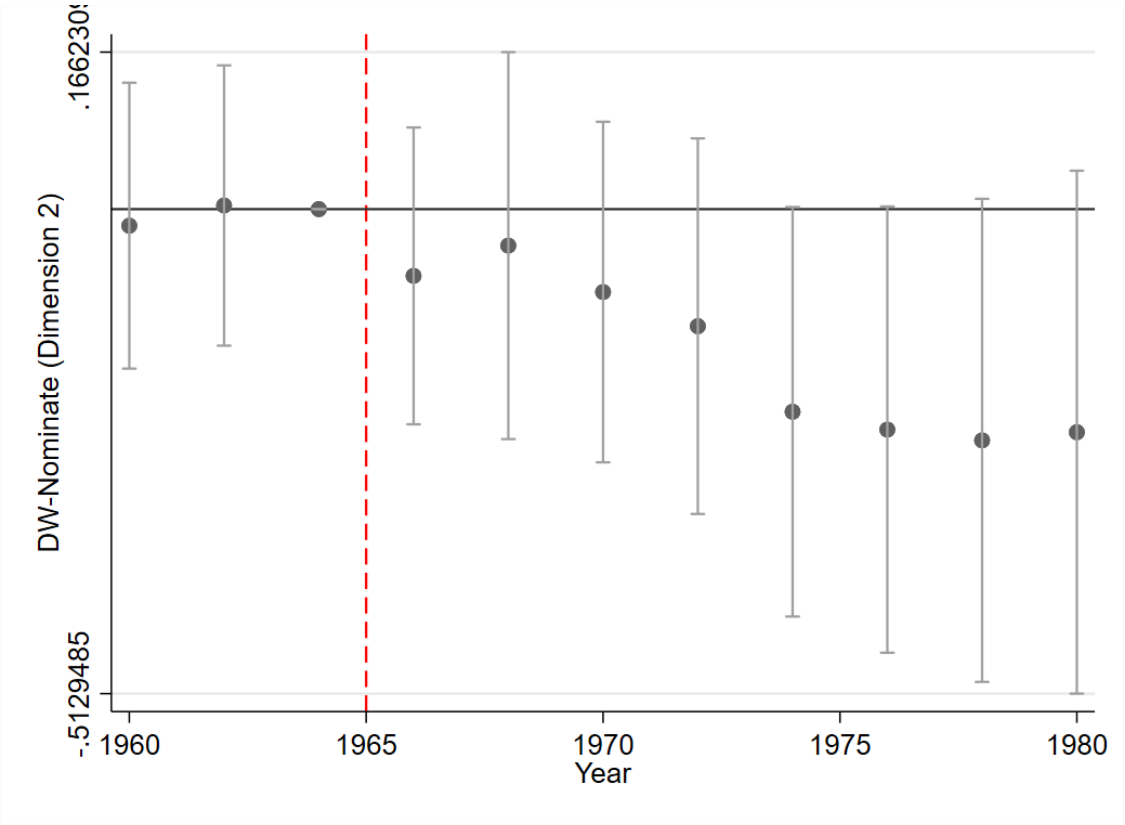
Table C.32 presents evidence of how the VRA changed the allocation government spending in a manner that benefited black communities. Panel A examines how VRA coverage affected the distribution of public assistance benefits (such as welfare or UI benefits).[70] Prior to the mid-1960s, black Americans 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. These regressions provide 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 C.32). 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 percent) 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.

[70] 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.

C.13 Appendix Figures & Tables:

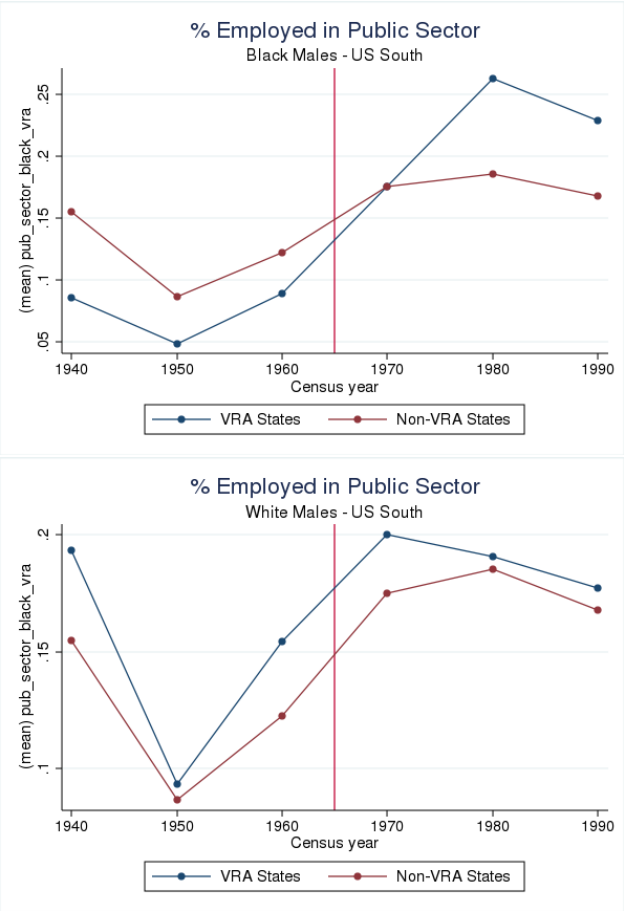
Figure C.1: **Impact of the VRA on Congressional Responsiveness**

(Source: Poole and Rosenthal)



Notes: This table presents event-study estimates for the impact of the VRA on Congressional ideology.

Figure C.2: 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.

Table C.1: 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 C.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 C.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
Sample	County	County	County	All Counties	All Counties	County	County
Sample	Pairs	Pairs	Pairs			Pairs	Pairs

Notes: This table presents regression coefficients from 8 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. 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.

Table C.4: The Effect of the VRA on Voter Registration, 1960-1968

Outcome:	(1) Black Registration	(2) White Registration	(3) Black-White Ratio	(4) Black Registration	(5) White Registration	(6) Black-White Ratio
	Panel A: Southern Counties (All)					
VRA	.158	.085	.117	.151	-.012	.160
	(.019)	(.010)	(.022)	(.032)	(.026)	(.043)
N	2179	2105	2095	2179	2105	2095
	Panel B: Southern Counties (County Pairs					
VRA	.093	.069	.060	.037	.017	.019
	(.031)	(.019)	(.036)	(.053)	(.041)	(.068)
N	563	556	551	563	556	551
Controls				X	X	X
County Trends				X	X	X

Notes: This table presents regression coefficients from 12 separate regressions. An observation is a county-year. The dependent variable is either race-specific county-level voter registration or the ratio of black registration rate to white registration rate. The independent variable is a dichotomous variable indicating whether a given county is covered under Section 5 of the VRA. Standard errors are in parentheses and are clustered by county. Regressions include county and year fixed effects. 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: Alt (1994).

Table C.5: Impact of the VRA on Legislator Ideology (DW-Nominate Dimension 1 (Conservative vs. Liberal))

	(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 C.6: The Effect of the VRA on Turnout – Heterogeneous Effects by Examiner Status

	(1)	(2)	(3)
Outcome: Presidential Election Turnout			
VRA × Examiners Sent	.23	.09	.01
	(.02)	(.02)	(.02)
VRA	.11	.09	.05
	(.01)	(.01)	(.01)
N	1400	1400	1400
Controls		X	X
State Trends			X

Notes: This table reports estimates of OLS regressions examining the impact of both the VRA and the use of federal election examiners on presidential turnout. All regressions include county baseline controls, county 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; each is measured at the 1960 level 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 C.7: 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	673000	673000
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 C.8: The Effect of the VRA on Earnings – Heterogeneous Effects by Examiner Status

Outcome:	(1) Log(Wage)	(2) Public Worker	(3) Log(Wage)	(4) Public Worker
VRA × Black × Examiners Sent	0.15	-0.00		
	(0.07)	(0.03)		
VRA × Black	0.03	0.023		
	(0.03)	(0.01)		
Examiners Sent × Black			0.20	0.04
			(0.07)	(0.03)
N	673000	673000	673000	673000
County-level Controls	X	X	X	X

Notes: This table reports estimates of OLS regressions examining the impact of both the VRA and the use of federal election examiners on black (relative) labor market outcomes. The dependent variable in each column is either the log hourly wage or an indicator for whether a worker was employed in the government sector. All regressions include county baseline controls, county 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; each is measured at the 1960 level 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 C.9: The Effect of the VRA on Employment –
Heterogeneous Treatment Effects, by Demographic Conditions

Outcome:	(1) Log(Wage)	(2) **1**(Public Worker)	(3) Log(Wage)	(4) **1**(Public Worker)	(5) Log(Wage)	(6) **1**(Public Worker)
VRA × % Urban (1960) × Black	0.308	.130				
	(0.163)	(0.063)				
VRA × % Farmers (1960) × Black			0.002	0.001		
			(0.03)	(0.02)		
VRA × % Urban (Change, 1960-1970) × Black					0.407	0.281
					(0.256)	(0.129)
VRA × Black	0.021	-0.021	0.027	-0.022	0.039	-0.018
	(0.032)	(0.016)	(0.03)	(0.02)	(0.028)	(0.013)
N	673000	673000	673000	673000	673000	673000
County-level Controls	X	X	X	X	X	X

Notes: This table reports estimates of OLS regressions examining heterogeneous effects of the VRA on black (relative) labor market outcomes, by demographic conditions. The dependent variable in each column is either the log hourly wage or an indicator for whether a worker was employed in the government sector. All regressions include county baseline controls, county 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 adult population fraction working in agriculture, and median household income; each is measured at the 1960 level 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 C.10: The Effect of the VRA on Employment –
Heterogeneous Effects by Underlying Social Conditions

Outcome:	(1) Log(Wage)	(2) Public Worker	(3) Log(Wage)	(4) Public Worker	(5) Log(Wage)	(6) Public Worker
VRA × % Black Tenancy (1940) × Black	0.10	-0.02				
	(0.03)	(0.02)				
VRA × Lynchings × Black			0.09	0.03		
			(0.03)	(0.02)		
VRA × Desegregation Order (1960) × Black					-0.21	0.02
					(0.05)	(0.05)
VRA × Black	(0.01)	(0.01)			0.07	-0.01
	(0.03)				(0.03)	(0.01)
N	673000	673000	673000	673000	673000	673000
County-level Controls	X	X	X	X	X	X

Notes: This table reports estimates of OLS regressions examining heterogeneous effects of the VRA on black (relative) labor market outcomes, by demographic conditions. The dependent variable in each column is either the log hourly wage or an indicator for whether a worker was employed in the government sector. All regressions include county baseline controls, county 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; each is measured at the 1960 level 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 C.11: 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 C.12: 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 C.13: The Impact of the VRA on the Black-White Wage Gap: Men and Women, 1950–1980

Outcome:	(**1**)	(**2**)	(**3**)	(**4**)	(**5**)
	Log(Wage)			1(Pub Emp.)	
VRA × Black	0.050	0.049	0.009	0.059	0.057
	(0.024)	(0.024)	(0.01)	(0.009)	(0.010)
Individual Controls	X	X		X	X
County Controls		X		X	X
N	1120000	1120000	1120000	1120000	1120000

Notes: This table presents regression coefficients from 4 separate regressions, one per column – extending the main sample to include both men and women who are full-time, full-year workers. Each column reports estimates from ordinary least squares regressions relating the VRA to either (relative) log wages or public employment. An observation is an individual in a given Census year. The dependent variable is the log hourly wage or an indicator for working in government, 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. 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. Individual controls in public sector regressions include years of education, years of experience, and squared years of experience. ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. See text for details. Source: DEC.

Table C.14: The Impact of the VRA on Black Employment Outcomes: North Carolina Subsample Analysis

Outcome Variable:	Log(Wage)		1(Public Emp.)	
	(**1**)	(**2**)	(**3**)	(**4**))
VRA × Black	0.118	0.117	0.044	0.041
	(0.062)	(0.050)	(0.007)	(0.01)
Sample Threshold (based on 1964)	10%	7%	10%	7%
County-level Controls	X	X	X	X
N	721000	663000	721000	663000

Notes: This table presents regression coefficients from 4 separate regressions, one per column. The sample is now expanded to all working-age indvididuals (men and women) in the NC county-pair sample. Each column reports estimates from ordinary least squares regressions relating the VRA to (relative) wages (Columns 1 and 2) or public sctor employment (Columns 3 and 4). 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. 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 C.15: The Effect of the VRA on Population Characteristics (Individual-level Regressions), 1960–1980

Outcome:	(1) Education	(2) Experience	(3) Race = Black	(4) Earnings Index
VRA × Black	0.11	-0.33	-0.001	0.004
	(0.11)	(0.23)	(0.01)	(0.01)
N	673000	673000	673000	673000
County-level Controls	X	X	X	X

Notes: This table reports estimates of OLS regressions relating whehter a county ever became covered by the VRA to a given worker characteristic (testing for effects of the VRA on characteristics of the worker population). The dependent variable in each column is a characteristic in a given year. All regressions include county baseline controls, county 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; each is measured at the 1960 level 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 C.16: The Effect of the VRA on Black Population Characteristics (Individual-level Regressions), 1960–1980

Outcome:	(1) Education	(2) Experience	(3) Age	(4) Earnings Index
VRA	-0.004	0.111	-0.564	-0.004
	(0.504)	(0.11)	(0.515)	(0.01)
N	115000	115000	115000	115000
County-level Controls	X	X	X	X

Notes: This table reports estimates of OLS regressions relating whehter a county ever became covered by the VRA to a given worker characteristic (testing for effects of the VRA on characteristics of the worker population). The dependent variable in each column is a characteristic in a given year. All regressions include county baseline controls, county 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; each is measured at the 1960 level 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 C.17: 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 C.18: 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 C.19: Effect of the VRA on Worker Commuting

	(1)	(2)
Outcome:	1(Commute to Different State)	Log(Wage)
VRA × Black	0.01	0.11
	(0.01)	(0.3)
VRA × Commuter		0.03
		(0.02)
Black × Commuter		0.004
		(0.01)
VRA × Black × Commuter		0.01
		(0.03)
N	571000	571000
County-level Controls	X	X

Notes: This table reports estimates of OLS regressions examining the impact of both the VRA and the use of federal election examiners on black (relative) labor market outcomes. The dependent variable in each column is either the log hourly wage or an indicator for whether a worker was employed in the government sector. All regressions include county baseline controls, county 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; each is measured at the 1960 level 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 C.20: 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 C.21: 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 C.22: 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 C.23: 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 C.24: Occupational Growth in the Public Sector

Top 10 Occupations	Bottom 10 Occupations
Statisticians	Athletes, sports instructors, and officials
Personal service occupations, nec	Social scientists, n.e.c.
Garbage and recyclable material collectors	Psychologists
Welfare service aides	Engineering instructors
Health aides, except nursing	Art makers
Data entry keyers	Teachers , n.e.c.
Office machine operators, n.e.c.	Chemistry instructors
Broadcast equipment operators	Office supervisors
File clerks	Civil engineers
Repairers of industrial electrical equipment	Foresters and conservation scientists

Source: Decennial Censuses, 1960 & 1980

Table C.25: Testing Complementarity between Political Power and Civil Rights Act

	(1)	(2)	(3)	(4)
	Outcome: Log(Wage)			
VRA × Black	0.24	0.04	−0.065	−0.062
	(0.11)	(0.04)	(0.054)	(0.109)
VRA × Frac. Small Est.	0.03			
	(0.03)			
VRA × Black × Frac. Small Est.	-0.27			
	(0.16)			
VRA × Frac. Large Est.		0.16		
		(0.07)		
VRA × Black × Frac. Large Est.		0.20		
		(0.24)		
TitleVIIExposure × Black			5.302	4.908
			(3.17)	(0.2.34)
VRA × Black × TitleVIIExposure			0.351	0.28
			(0.157)	(0.365)
County-level Controls	X	X	X	X
Occupation Controls				X
N	518000	518000	10500	10500

Notes: REVISE NOTES. This table presents regression coefficients from four separate regressions, one per column. An observation is an individual Census respondent in a given Census year. The sample is limited to workers in the private sector (excluding government workers). 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 fraction of establishments that are either below 20 employes ("Frac. Small Est.") or above 100 employees ("Frac. Large Est."). All regressions control for individual education, years worked, and squared(years worked), and include county-race and county pair-year-race fixed effects. County-year fixed effects are omitted, and VRA indicates how wages change for white workers in VRA-affected counties. 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 County Data Books.

Table C.26: 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 C.27: Impact of the VRA on Public Assistance Access, 1962 - 1977

	Outcome Variable: **1**(Public Assistance Receipt)			
VRA	.11	.08	-.09	-.19
	(.16)	(.16)	(.21)	(.20)
VRA × Black Pop. Share			1.04	1.69
			(.57)	(.66)
N	690	690	690	690

Notes: This table presents regression coefficients from 4 separate regressions, one per column. An observation is a county-year. The dependent variable is the per capita number of public assistance recipients in a given county (measured twice - in 1964 and 1980). 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, Consolidated File, 1944–1977.

Table C.28: 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 C.29: Effects of the VRA on Relative Wages, by Local Government Executive Branch Structure

Outcome:	(1) Log(Wage)	(2) Public Employment
Black × VRA × Mayor-Council Govt.	0.128	0.046
	(0.047)	(0.025)
Black × VRA	−0.024	0.02
	(0.032)	(0.024)
N	375000	375000

Notes: This table presents regression coefficients from 2 separate 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 C.30: Effects of the VRA on Relative Wages, by City Council Structure

Outcome:	(1) Log(Wage)	(2) Public Employment
Black × VRA × Ward Elections	0.134	0.001
	(0.047)	(0.02)
Black × VRA	−0.024	0.004
	(0.032)	(0.024)
N	375000	375000

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 C.31: 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.

Table C.32: 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