The Effects of Minority Enfranchisement on Public Finances and the Structure of Government^{*}

Raheem Chaudhry^{\dagger}

Last revised: April 25, 2025

Abstract

Previous research finds the Voting Rights Act of 1965 (VRA) increased the political power of Black communities. We analyze the broader effects of minority enfranchisement on local public finances by exploiting spatial discontinuities in the application of special provisions of the VRA. We find that among counties targeted by these special provisions, those with larger non-white population shares exhibited relative declines in revenues and expenditures, and relative increases in government fragmentation. An analysis of mechanisms suggests that declines in revenues were not primarily mechanical responses to changes in the tax base, but were instead likely due to changing preferences for public goods.

^{*}I would like to thank Abhay Aneja, Hilary Hoynes, Rucker Johnson, and Jesse Rothstein for helpful discussions and feedback. This paper also benefited from discussion with seminar participants at UC Berkeley, the Berkeley-Princeton Convening on Racial and Ethnic Disparities in the Labor Market, Mount Tamalpais College at San Quentin State Prison, and the Association for Public Policy Analysis and Management Fall Research Conference. The Opportunity Lab and the Law, Economics, and Politics Center at UC Berkeley provided financial support. Any errors are my own.

[†]Goldman School of Public Policy, University of California, Berkeley; rchaudhry@berkeley.edu

1 Introduction

Nearly a century after the end of the Civil War, Black Americans continued to have limited political voice, due in large part to Jim Crow laws that disenfranchised Black communities across the South. Consequently, voting rights legislation was central to the civil rights campaigns in the 1950s and 1960s, culminating in the passage of the Voting Rights Act of 1965 (VRA), which sought to fulfill the promise of the Fifteenth Amendment and eliminate racial discrimination in voting. But contemporary observers did not see the VRA as important solely because it guaranteed the right to vote to Black Americans, but also because of the potential of the franchise more broadly. As President Lyndon Johnson noted ahead of signing the Act into law, "This right to vote is the basic right without which all others are meaningless. It gives people, people as individuals, control over their own destinies."¹ He expressed the hope that the VRA would guarantee the right to vote for Black Americans, who would then "transform the vote into an instrument of justice and fulfillment."

Research shows that the VRA largely lived up to these lofty aspirations. A large literature documents the success of the VRA in increasing Black political power as measured by voter registration (Ang, 2019; Bernini, Facchini, Tabellini, and Testa, 2023) and political officeholding (Wright, 2013; Bernini, Facchini, and Testa, 2023). Some research finds that Black communities were able to translate this newfound political strength into more favorable public policy, such as a more equitable distribution of resources (Husted and Kenny, 1997; Cascio and Washington, 2014) and fairer policy outcomes more generally (Aneja and Avenancio-Leon, 2019; Facchini, Knight, and Testa, 2020).

In this paper, we aim to more comprehensively investigate how the VRA ultimately affected revenues, spending, and the structure of local government, which has important implications for the distribution of public spending and the provision of public goods.

One challenge to answering these questions is that the VRA is federal legislation and many of its provisions apply nationwide. However, there are special provisions of the Act that applied only to specific jurisdictions – usually states or counties – targeted based on a coverage formula defined in Section 4 of the Act. Until the coverage formula was ruled unconstitutional in *Shelby v. Holder (2013)*, Section 5 of the VRA required covered jurisdictions to "preclear" any changes related to voting processes with the Attorney General of the United States or the US District Court for DC, and Section 8 (formerly Section 6) allowed the Attorney General to appoint federal officials in covered jurisdictions to ensure that voter registration and voting were carried out without discrimination. However, jurisdictions were

¹The Miller Center, "August 6, 1965: Remarks on the Signing of the Voting Rights Act", https://millercenter.org/the-presidency/presidential-speeches/august-6-1965-remarks-signing-voting-rights-act.

covered because they were particularly discriminatory against minorities. This may raise concern that covered counties were different in ways that might also affect the outcomes of interest. To address this concern, we follow Aneja and Avenancio-Leon (2019), who study the effects of the VRA on labor market outcomes, and exploit spatial discontinuities in Section 4 coverage by restricting attention to a sample featuring adjoining pairs of covered counties and their never covered neighbors. If counties closer to one another are more similar in ways that affect outcomes, this sample restriction should attenuate any bias that would arise from making comparisons across the universe of counties. To eliminate lingering concern that covered and never covered counties are fundamentally different in ways that may affect outcomes – even among neighboring counties – we estimate the effect of Section 4 coverage on a range of public finance outcomes by employing a triple-differences design to compare the difference in outcomes between covered counties with higher and lower non-white shares to the corresponding difference in never covered counties. Under the assumption that differences between counties with higher and lower non-white population shares would have evolved in parallel between covered and never covered counties, these estimates can be interpreted as causal.

We find that among covered counties, those with larger non-white population shares saw an overall relative decline in revenues. Among covered counties, counties with a 1960 non-white share that was ten percentage points higher experienced a statistically significant relative decline of 3.2% in general revenues, including statistically significant declines of 4.0%in property taxes. We also find a corresponding decline of 2.6% in direct expenditures.

Observers have long recognized that communities can manipulate government boundaries to preserve local control over public resources. By forming new cities, for instance, communities "can maintain more exclusive control over taxation, service levels, and the character of the population" (Burns, 1994, p. 32). Some scholars have argued that white communities sometimes incorporated to preemptively block annexation and any corresponding increases in taxation or expansions in social services (Miller, 1981). We might expect increased government fragmentation in places where communities are motivated to preserve local control over resources. We examine this possibility by testing the effects of Section 4 coverage on the number of local governments. We find that a 10 percentage point increase in the non-white share leads to a statistically significant increase in the total sum of county governments, municipal governments, and special districts by 3.0%.

The county border pair sample has two disadvantages that make it challenging to examine heterogeneity in impacts or investigate potential mechanisms. First, the sample is relatively small and, therefore, is limited in statistical power. Moreover, within a county pair, counties tend to be observably similar. For these reasons, we proceed in our analysis by relying on a larger sample of more diverse Southern counties.

Using this sample, we study heterogeneity in impacts across two key dimensions. First, we examine variation in effects based on counties' electoral systems. Previous research shows that Black communities had the greatest success in translating the vote into improved political outcomes in jurisdictions with single-member, rather than at-large, electoral systems (Bernini, Facchini, and Testa, 2023). Consistent with this work, we find that decreases in revenues and expenditures are concentrated primarily in counties with at-large systems, those systems where racial minorities were less likely to be able to convert their newfound power to electoral success. Second, we explore heterogeneity based on a counties' non-white share. We find revenues and expenditures decline in the non-white share only in counties with relatively low non-white populations. Together, these analyses suggest that the results on public finances are concentrated in places where the voting power of non-white communities is growing, but where these communities are less able to form durable, winning political coalitions.

Probing mechanisms further, there are two reasons we might observe relative declines in taxes and revenues. One possibility is that spending changes may be mechanical responses to changes in a tax base (e.g., declines in income – due either to secular economic trends or behavioral responses to the VRA – would lead to declines in income taxes without any policy change). We find that covered counties with greater non-white shares do not seem to experience economic decline that might be correlated with declines in tax bases in ways that would meaningfully affect our main results. A second possibility is that counties implement policies that either decrease tax rates or shrink the tax base. Since we do not observe tax policy changes directly, we instead indirectly assess whether there were changes in preferences for public goods, as positive political theory would suggest that tax policy would change only if there were a shift in the underlying policy preferences of the electorate. Specifically, given the importance of education spending in local budgets, we test for changes in public school enrollment. In the larger sample of Southern counties, the findings suggest that public school enrollment fell from 1968 to 1974, which would imply that families in these communities may have substituted from public to private schooling in response to Section 4 coverage. Together, the results suggest the relative declines in taxes and spending are not primarily driven by mechanical responses to decreases in the tax base. Rather, they support the possibility that the declines were due to changing preferences for public goods.

This paper makes several contributions to the literature. Foremost, this paper adds nuance to the literature on redistributive politics. Theory suggests that enfranchising disadvantaged groups will result in a more equitable distribution of resources (e.g., Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1996). Existing empirical research has largely focused on how the VRA helped Black communities secure more equitable public policy outcomes (Husted and Kenny, 1997; Cascio and Washington, 2014; Aneja and Avenancio-Leon, 2019; Facchini, Knight, and Testa, 2020; Bernini, Facchini, and Testa, 2023). In contrast, this paper adds to recent empirical work that documents political backlash to the VRA (Kuziemko and Washington, 2018; Fresh, 2018; Ang, 2019; Eubank and Fresh, 2022; Bernini, Facchini, Tabellini, and Testa, 2023) by more fully documenting local responses to the enfranchisement of disadvantaged groups.

This project also contributes to understanding the drivers of public spending and on the structure of local government. Existing work finds that racial heterogeneity is associated with lower spending on public goods, including public education (Cutler, Elmendorf, and Zeckhauser, 1993; Poterba, 1997; Alesina, Baqir, and Easterly, 1999; Goldin and Katz, 1999; Luttmer, 2001). Separately, there is research that finds that the number of political jurisdictions is related to trade-offs between size and heterogeneity in income or race (Alesina and Spolaore, 1997; Bolton and Roland, 1997; Alesina, Baqir, and Hoxby, 2004); other researchers show that the supply of new jurisdictions is related to the presence of special interest groups that would benefit from local control over resources (Burns, 1994). Relatedly, previous work has found that places covered by the special provisions of the VRA – where changes to city boundaries had to be precleared under Section 5 – were more likely to annex outlying Black communities (Durst, 2018), but have been less likely to since *Shelby* (Durst, 2019). This paper builds on both of these literatures by presenting evidence for why racial heterogeneity matters for public finances and government fragmentation.

We also contribute to a more comprehensive understanding of fiscal federalism and, relatedly, the tools local communities have to promote or undermine practices that target inequality. The literature on fiscal federalism largely focuses on determining the assignment of various functions of government to different levels of government. In general, the literature argues for the local provision of goods and services, but for redistribution to be carried out at higher levels of government (for a synthesis of this literature, see Oates, 1999). One limitation of this literature is that it does not account for frictions in determining the local provision of public goods (e.g., disenfranchisement of minority groups) or how local communities' preferences change with a changing electorate (e.g., due to migration or enfranchisement). For instance, during the Great Migration, Black families left the Jim Crow South for superior economic and political opportunity in the North and West. But a growing literature documents backlash: In response to greater Black in-migration from the South, white families fled across jurisdictional borders (Boustan, 2010), local governments adjusted spending in ways that ultimately made destination communities worse for upward mobility (Derenoncourt, 2022), and communities responded by implementing more exclusionary zoning (Sahn, 2022; Cui, 2024), which are often credited with upholding racial and income segregation. This paper documents additional ways that local communities may respond to a changing electorate, ultimately presenting a more full picture of how communities maintain local control over resources.

The rest of this paper proceeds as follows. Section 2 provides historical background on the VRA. Section 3 details the theoretical motivation for the paper. Section 4 introduces the research design and presents descriptive statistics. Section 5 covers the main results on the effects of Section 4 coverage on revenues, expenditures, and government fragmentation. Section 6 offers suggestive evidence on why and how local communities decreased revenues and spending following Section 4 coverage. Section 7 concludes.

2 Historical Background: The Voting Rights Act of 1965

Following the end of the Civil War, the Reconstruction government passed the Fifteenth Amendment, guaranteeing the right to vote for all men, regardless of their race. However, soon after the end of Reconstruction, Southern states began to pass laws meant to disenfranchise Black communities. These Jim Crow laws were wide ranging and included, for example, literacy tests and poll taxes that technically applied to all prospective voters, but in practice were used as tools to disenfranchise Black Americans.

Consequently, the right to vote was at the center of the civil rights movements of the 1950s and 1960s. As early as 1957, Martin Luther King, Jr. articulated the connection between voting rights and economic inequality, arguing that voting was necessary to secure "all other rights, school integration, adequate housing, job opportunities, [and] integrated public transportation" (Jackson, 2007, p. 87). After nearly a decade of fighting for voting rights, civil rights activists organized the now-infamous marches from Selma to Montgomery for voting rights in March 1965. Following the horror of Bloody Sunday, where Alabama state troopers brutally assaulted activists crossing the Edmund Pettus Bridge, President Lyndon Johnson called on Congress to pass strong voting rights legislation. By the end of the summer, President Johnson had signed into law the Voting Rights Act of 1965 (VRA), described as perhaps "the most radical piece of civil rights legislation since Reconstruction" (Tribe, 1978, p. 263).

The VRA contains a number of general provisions that apply nationwide. At the heart of these general provisions is Section 2, which prohibits any "voting qualification or prerequisite to voting, or standard, practice or procedure" that interferes with "the right of any citizen of the United States to vote on account of race or color."

Additionally, the VRA contains a number of special provisions that applied only to a subset of jurisdictions (largely in the South), determined according to rules laid out in Section 4 of the Act. First, under Section 5 of the VRA, counties covered under Section 4 – which I refer to as "covered counties" hereafter – had to "preclear" any changes related to the voting process with, in practice, the Attorney General. Second, under Section 8 (originally Section 6) of the Act, the Attorney General could appoint federal examiners in covered jurisdictions to prepare lists of eligible voters to help ensure they could be registered to vote. Finally, Section 8 further allowed the Attorney General to appoint additional "persons" to districts where federal examiners had been appointed to monitor elections and ensure eligible voters were allowed to vote (throughout the paper, I follow some scholars of the VRA and refer to these individuals as "observers" to distinguish them from the federal examiners).

Section 4 of the VRA establishes a formula to determine which jurisdictions are covered by the special provisions of the VRA. Under Section 4(b), coverage applies to any state or political subdivision where (1) the Attorney General maintained that there was in place a test or device commonly used to discriminate against racial minorities in the electoral process on November 1, 1964, and (2) the Director of the Census determined that less than 50% of the voting age population was registered to vote on November 1, 1964, or voted in the 1964 election.²

Initially, Attorney General Nicholas Katzenbach determined that 21 states had in place such a "test or device."³ Director of the Census Bureau A. Ross Eckler determined that in seven of the 21 states (Alabama, Alaska, Georgia, Louisiana, Mississippi, South Carolina, and Virginia), less than 50% of the voting age population voted in the 1964 election. Furthermore, he determined that certain counties – predominantly in North Carolina – also had turnout below 50% and were therefore also subject to the coverage formula.⁴

The 1970 amendments to the VRA added 1968 trigger dates, extending coverage to include any places that met these two criteria in 1968. These amendments resulted in coverage for a handful of additional jurisdictions.⁵ The 1975 amendments were broader in

²Under Section 4(c), a "test or device" includes "any requirement that a person as a prerequisite for voting or registration for voting (1) demonstrate the ability to read, write, understand, or interpret any matter, (2) demonstrate any educational achievement or his knowledge of any particular subject, (3) possess good moral character, or (4) prove his qualifications by the voucher of registered voters or members of any other class."

³These states included Alabama, Alaska, Arizona, California, Connecticut, Delaware, Georgia, Hawaii, Idaho, Louisiana, Maine, Massachusetts, Mississippi, New Hampshire, New York, North Carolina, Oregon, South Carolina, Virginia, Washington, and Wyoming. See 30 F.R. 9897 (1965).

⁴See 30 F.R. 9897 (1965), 30 F.R. 14505 (1965), 31 F.R. 19 (1966), 31 F.R. 982 (1966), 31 F.R. 3317 (1966), and 31 F.R. 5081 (1966).

 $^{{}^{5}}See 36$ F.R. 5809 (1971).

scope: they added 1972 trigger dates and, furthermore, expanded coverage to places where voting materials were only offered in English but where a single-language minority group made up more than 5% of the population. This provision extended coverage to Alaska, Arizona, and Texas in their entirety, as well as parts of California, Florida, Michigan, New York, North Carolina, and South Dakota.⁶ Figure A.1 shows which counties were covered by the special provisions by date of coverage.

Importantly, with few exceptions, coverage was an absorbing state: once covered, counties typically remained covered until the Supreme Court ruled the coverage formula unconstitutional in *Shelby County v. Holder* (2013).^{7,8}

3 Theoretical Framework

In the redistributive politics literature, there are two broad, canonical classes of electoral models where parties compete for votes in an effort to secure electoral office. In the first, individuals seek to maximize their utility over consumption and leisure, differing only in their income. This heterogeneity generates varying preferences for taxation and (lump-sum) redistribution. When preferences are single-peaked, parties campaign on the preferences of the median voter (e.g., Romer, 1975; Roberts, 1977; Meltzer and Richard, 1981). In the second, there are clearly defined electoral groups who prefer one political party but that may be induced to shift their vote in exchange for redistribution to their group (e.g., Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1996; Dixit and Londregan, 1998). In either set of models, enfranchising a disadvantaged group would predict weakly greater redistribution toward members of that group.

Recent empirical work shows that the VRA had large, positive effects on voter registration and turnout in places with larger Black communities (Cascio and Washington, 2014; Fresh, 2018; Ang, 2019; Aneja and Avenancio-Leon, 2019; Bernini, Facchini, Tabellini, and Testa, 2023), suggesting that it succeeded in its mission to enfranchise Black voters. Theory would predict that a growing Black share of the electorate would lead to increased redistribution

⁶See 40 F.R. 43746 (1975), 40 F.R. 49422 (1975), 41 F.R. 784 (1976), and 41 F.R. 34329 (1976).

⁷Shelby County v. Holder, 570 U.S. 529 (2013).

⁸Section 4 does include a "bailout" provision that allows jurisdictions to become exempt from coverage under certain conditions. Since the passage of the VRA, a few dozen jurisdictions have successfully bailed out of coverage. Importantly for the research design here, only a small number of these jurisdictions were bailed out in the 30 years following passage of the VRA, only one of these, Wake County, North Carolina, was in the South. Because Wake County became exempt shortly after the passage of the VRA (in 1967), I treat it as a never covered county. See https://www.justice.gov/crt/section-4-voting-rights-act#bailout for a list of bailed out jurisdictions.

toward Black communities. Empirical work offers some support for this prediction: Research does find, for instance, that the elimination of literacy tests and poll taxes led to increased spending on the poor (Husted and Kenny, 1997) and that, among places with literacy tests, counties with larger Black populations were able to secure a more equitable distribution of state transfers (Cascio and Washington, 2014). More generally, the literature finds that Black communities were able to fight for more favorable public policies even beyond taxation and redistribution. Black workers were able to secure a greater share of public sector jobs (Aneja and Avenancio-Leon, 2019), Black Americans were arrested at lower rates in places with elected law enforcement officials (Facchini, Knight, and Testa, 2020), there were increases in Black elected officials and capital spending (Bernini, Facchini, and Testa, 2023), and the historical record is replete with cases where governments improved the provision of public services in Black communities following passage of the civil rights legislation of the 1960s (see Wright, 2013, Ch. 6 for a summary of the literature).

Extending the franchise to a group guarantees neither a more equitable distribution of resources nor an absolute increase in resources to the newly enfranchised group. Under the framework outlined above, resources would only be distributed more equitably if the newly enfranchised increased their share of the electorate. Previous research finds that, in places where the political 'threat' from Black populations was greatest, white communities were more likely to "countermobilize" and increase voter registration (Bernini, Facchini, Tabellini, and Testa, 2023). Research also finds that local support for Democrats fell following Democrats' support for civil rights legislation (Kuziemko and Washington, 2018) and passage of the VRA (Ang, 2019; Fresh, 2018), possibly by shifting support or increasing turnout among racially conservative white Southerners. If white voters respond to Black enfranchisement by increasing turnout, then politicians may not be as motivated to attune themselves to the needs of the newly enfranchised Black communities.

Determining whether the absolute level of resources devoted to newly enfranchised groups increases requires understanding not only how enfranchisement effects the distribution of resources, but also how it affects their total level. The total level of resources available for redistribution may change following enfranchisement. This may be because enfranchisement causes changes in a tax base, which mechanically affects revenues. For example, if home prices fall following enfranchisement, property taxes would fall mechanically. Alternatively, members of the pre-existing electorate may update their preferences for taxation following enfranchisement for two reasons. First, as existing resources are redistributed towards minority communities, members of the majority group secure fewer benefits for any fixed level of taxation. Second, members of the majority group may have disutility over minorities' consumption of public goods. For example, many white Americans ceased to use public spaces such as parks or swimming pools following their desegregation, which ultimately led to disinvestment in public spaces throughout the country (McGhee, 2021). In either case, public goods may no longer be as appealing for members of the majority and, consequently, they might prefer less taxation overall. In any model where the level of public goods and the budget constraint are functions of the tax rate (E.g., Lindbeck and Weibull, 1987), politicians may respond by campaigning on lower tax rates, and therefore a smaller budget. While there is limited work on how changes in local political preferences affected local preferences for taxation and redistribution, there is some evidence of backlash across other dimensions: Research finds, for example, that places covered by Section 4 of the VRA saw increased Black incarceration (Eubank and Fresh, 2022).

Because the theory leads to ambiguous predictions of the overall effect of minority enfranchisement, I proceed to develop a strategy to empirically test for responses among places that were subject to additional provisions of the VRA.

4 Research Design

4.1 Sample Construction

The goal of this paper is to examine how Section 4 coverage affected revenues, expenditures, and the structure of local government.⁹ The primary challenge for causal identification of the effects of Section 4 coverage is that any empirical analysis that compares outcomes for covered jurisdictions to never covered ones will be biased if there exist unobserved confounders that are correlated with both coverage and with local public policy outcomes.

The coverage rule itself raises the possibility of the existence of such confounders. Jurisdictions that were covered by Section 4 were targeted for coverage *because* they engaged in particularly discriminatory behavior: In order to be covered by Section 4, a jurisdiction needed to have a device in place that restricted the right to vote and where less than 50% of the voting age population was registered to vote or actually voted in a presidential election.

The empirical coverage patterns present additional complications. While the coverage rule is written so that coverage can be targeted to individual towns or counties, in practice initial coverage of Section 4 applied to entire states, with the notable exception of North Carolina. So, if there were phenomena that disproportionately affected these covered states

⁹Throughout this paper, I interpret findings as the effect of Section 4 coverage, rather than the effect of the three special provisions (preclearance, appointment of federal examiners, appointment of federal observers). This is because not all special provisions were actively used in all covered counties: the appointments of federal examiners and observers were at the discretion of the Attorney General. So, estimates in this paper can be thought of as the effects of Section 4 coverage or, equivalently, the combined effects of preclearance and the *threat* of the appointment of federal examiners or observers.

(such as state legislation or geographically-concentrated economic shocks) and which affected the outcomes of interest – perhaps likely in this context where many treated counties are geographically concentrated in the Deep South or neighboring states – then it may be difficult to disentangle the effects of Section 4 coverage from these phenomena.

To overcome these challenges, we follow Aneja and Avenancio-Leon (2019), who study the labor market impacts of Section 4, and leverage the spatial discontinuity in Section 4 coverage by restricting our focus to a sample comprising all counties ever covered by Section 4 that border at least one county never covered by Section 4, as well as those never covered neighbors. Focusing on contiguous border pairs attenuates concerns about bias in two key ways. First, neighboring counties are more similar than counties further apart, which suggests they may serve as better comparisons for one another (see subsection 4.5 for further discussion). Second, focusing on adjoining counties may alleviate concern about the confounding effects of geographically-concentrated shocks. While such shocks might disproportionately affect one broad, geographic area more than another, they are more likely to affect communities within a narrow bandwidth of the spatial discontinuity similarly. For example, during the 1950s and 1960s, major civil rights campaigns disproportionately targeted the Deep South. Any analysis comparing all covered counties to never covered ones may not be able to distinguish between the effects of Section 4 coverage from the civil rights movement more generally. However, it is plausible that civil rights campaigns affected neighboring communities similarly, so focusing on adjacent counties may ameliorate concerns about bias in a larger sample.

To construct the contiguous border pair (CBP) sample, we first identify all counties first covered under Section 4 immediately after passage of the VRA.¹⁰ We then identify all counties that border the covered counties *and* that were never covered by Section 4 themselves. Figure 1 maps the final sample by coverage status.

An individual county may appear in the sample multiple times if it is in multiple pairs. Overall, there are 506 counties in the main sample, representing 237 unique counties. The main sample includes 253 counties that were ever covered (128 unique counties) and 253 never covered counties (109 unique counties).

¹⁰Of counties ever covered under Section 4, nearly all were covered either at the time the VRA was originally passed in 1965 or following the 1975 amendments, though a small number were covered following amendments passed in 1970. Counties covered in 1965 were targeted for coverage on the basis of racial discrimination in voting procedures, whereas counties covered later were mostly targeted because they had sizable non-English speaking populations but they only provided election material in English. Since a main motivation of this paper is to understand how local communities responded to Black enfranchisement, we focus attention on the cohort covered by the original 1965 law.

4.2 Empirical Strategy

If we assume that outcomes would have evolved in parallel between covered and never covered counties in the absence of Section 4 coverage, it would be natural to estimate the effects of Section 4 coverage among this restricted sample using a difference-in-differences design. However, it might be too much to invoke this assumption given the spatial patterns of coverage, even if we restrict attention to adjacent counties.

Instead, we follow the existing literature and impose a weaker assumption that, absent Section 4 coverage, the difference in outcomes between covered counties with higher and lower non-white shares would have evolved in parallel to the corresponding difference in never covered counties.

Then, to estimate the effects of the VRA on a range of outcomes we estimate the following triple-differences model using OLS:

$$y_{cpt} = \alpha + \theta \text{VRA}_{cpt} + \beta \left(\text{VRA}_{cpt} \times \% \text{ Non-white}_{c}^{1960} \right) + X'_{cpt} \gamma + \mu_{c} + \tau_{pt} + \epsilon_{cpt}$$
(1)

where c indexes county, p a county pair, and t a year. VRA_{cpt} is an indicator for whether county c was covered at time t. % Non-white¹⁹⁶⁰_c is the share of the population of county c that was not white in 1960.¹¹ X'_{cpt} includes fixed effects for county characteristics measured in 1960 – share non-white, median family income, share with a high school diploma, the employment-to-population ratio, median years of education, median age, and share urban – interacted by year. Following Dube, Lester, and Reich (2016), we also include county fixed effects μ_c and pair-specific time effects τ_{pt} , which control for any pair-specific features that vary over time. These features could include time-specific shocks to the pair (e.g., a one-time shock to the local labor market) or characteristics that vary among the pair over time (e.g., demographic changes).

Standard errors are clustered at the county level, which accounts for the fact that counties can appear in multiple border pairs.

¹¹Unlike previous related studies, we interact treatment with the share non-white instead of the share Black. Among counties in the main sample, the share non-white and the share Black are nearly identical, so results are very similar regardless of which variable we interact with. However, we also conduct estimation using an alternative sample that include counties with sizable Hispanic populations. To estimate results in these counties, it is important to take into account the potential electoral power of not just Black communities, but other racial or ethnic minorities as well.

4.3 Alternative Sample

Throughout the paper, we also conduct analysis on an alternative sample of all Southern counties, defined as counties in states that were formerly part of the Confederacy (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia).

We use this alternative sample for two reasons. First, re-estimating the main results on this expanded sample helps us assess the extent to which estimates for the main sample apply to a sample of larger, more diverse counties. Second, we use this sample to analyze heterogeneity in impacts across a range of dimensions. The county border pair sample is small and, therefore, is limited in statistical power. Moreover, within a county pair, counties tend to be observably similar. This alternative specification overcomes both of these challenges.

We estimate impacts of Section 4 coverage on outcomes by adapting the triple-differences approach above in two key ways. First, we classify a county as covered only if it was covered under the initial passage of the VRA in 1965, rather than if the county was ever covered. Since there are counties in this expanded sample that were covered in the 1975 amendment to the VRA, we restrict our analysis to outcomes measured before 1975. Second, we replace the pair-year fixed effects in Equation 1 with state-year fixed effects. Otherwise, our approach is the same.

4.4 Data

Below, we briefly summarize the main data sources used in the analysis.

Data on government finances and organization come from the Census of Governments (CoG). The CoG is a census that has been conducted by the Census Bureau every five years since 1957. These data are collected for all individual state and local governments in the United States, including those of states, counties, municipalities, townships, special districts, and independent school districts.

Much of these data have been digitized and can be accessed electronically. For 1972 onwards, complete CoG data are available through the Census Bureau. For 1962 and 1967, data on counts of local governments are hosted on the Interuniversity Consortium for Political and Social Research (ICPSR). Data for earlier years are not as readily accessible. We digitized data on government finances for 1957-1967 and data on government organization in 1957 from scans of historical CoG reports available through the Census Bureau.

Additionally, we use information from various sources to test for potential mechanisms. Data on county population and income come from the Census Bureau. Data on homeownership and home values comes from Census' *County Data Book* series. Data on public school enrollment comes from the Office of Civil Rights school district surveys, decoded by Sarah Reber and Ben Denckla.

We use data on 1960 county demographic and economic characteristics from Census' *County and City Data Book* series as baseline controls in the main analyses.

Since data are available at different geographies across sources and years, we aggregate all data to the county level.

For analysis of the main sample, we focus on outcomes up to 20 years following the enactment of the VRA. For the sample of all Southern counties, we focus on outcomes measured before the passage of the 1975 amendment to the VRA, since it subjected many of the control counties in this sample to the special provisions.

When analyzing individual outcomes, we occasionally exclude a small number of counties where the outcome data are missing or appear unreliable. We discuss this and other issues related to the data in more detail in Appendix B.

4.5 Descriptive Statistics

We present descriptive statistics for our samples in Table 1. Panel A displays characteristics for counties first covered in 1965 that border at least one county that was never covered, and those never covered neighbors. Covered counties tend to have lower populations (though this difference is not significant), but are otherwise very similar to their never covered neighbors in 1960. Crucially, covered counties and their counterparts are balanced on the share of the population that is non-white or Black.

Panel B contains descriptive statistics for the sample of all Southern counties. For this sample, there are significant differences in pre-treatment levels for nearly all baseline characteristics. Of particular relevance, covered counties have much higher shares of racial minorities than never covered counties, highlighting the importance of analyzing differences in trends – instead of levels – over time.

5 Main Results

Previous research shows that different provisions of the VRA were successful in increasing voter registration and turnout (Cascio and Washington, 2014; Ang, 2019; Aneja and Avenancio-Leon, 2019). By increasing Black political power, the VRA could have implications for local public finances more broadly. For example, Black communities might have developed sufficient electoral strength in some places to elect leaders who would implement their preferred policies. Alternatively, the Act could have triggered backlash among the preexisting electorate, changing their preferences for taxation, redistribution, or public goods. In this section, we explore how Section 4 coverage affected levels of revenues, different categories of expenditures, and the structure of local government.

5.1 Revenues and Expenditures

We first analyze the effects of Section 4 coverage on public finances. Prior to estimation, we normalize all outcome variables by population and apply a log transformation.

We start by examining effects on revenues using an event study framework, adapting Equation 1 by replacing the key interaction term with a series of terms that interact year, the treatment indicator, and the non-white share. This approach allows us to inspect for the presence of pre-trends and for dynamics in treatment effects.

Figure 2 plots event study coefficients on the interaction between an indicator for Section 4 coverage, an indicator for year, and the non-white county share in 1960. Across all outcomes, these coefficients are not statistically different from zero before passage of the VRA. After passage of the VRA, coefficients are negative and generally statistically significant for all outcomes except for charges. Moreover, effects appear to grow over time for most outcomes.

We summarize impacts in Table 2. In this table, we estimate impacts of coverage on various revenue categories using Equation 1. In each cell, we display coefficients on the interaction between an indicator for whether a county was ever covered, an indicator for the post-1965 period, and the non-white county share. Each column corresponds to a different revenue category. For now, we focus on the first panel, which includes analysis of our contiguous border pair sample. Within that first panel, we present results from our main analysis in the row labeled "Pooled". Estimates in this row are associated with regressions which include all data from 1957-1987.

We find that, among covered counties, a one percentage point increase in the non-white share leads to a .0033 decline in log general revenues per capita relative to an equivalent increase among never covered counties. Given that the average county in the main sample has a non-white share of roughly 25%, we estimate the difference in revenues per capita between a covered county with the mean non-white share and an all-white covered county falls by about 7.8% relative to the difference between two such never covered counties. Looking at different revenue categories, a one percentage point increase in the non-white share leads to declines in log property taxes per capita of .0041 (9.6% at the mean), log federal transfers per capita of .0037 (8.6%) and log state transfers per capita by .0039 (9.2%).

Because there may be dynamic treatment effects, the main analysis may not capture long-term effects. For this reason, we also produce long difference estimates by estimating Equation 1 using only data from the period immediately preceding passage of the law (1962) and the last year of data in our analysis period (1987). For general revenues and property taxes, both sets of estimates are similar. However, long difference estimates for federal and state transfers are more negative than for the full sample.

We repeat this analysis using our sample of Southern counties. To conduct this analysis, we adapt Equation 1 by replacing pair-year with state-year fixed effects. We also restrict our data to exclude any observations observed after the 1975 amendments to the VRA. We present event study impacts in Figure A.4. Despite the shorter panel, we can still see some of the patterns as in the analysis of the border pair sample. We summarize results in Panel B. For the full sample, we find attenuated, statistically insignificant impacts of Section 4 coverage on revenues. In a short panel and in a setting where treatment effects grow over time, however, the pooled impacts may not capture long-run effects. If we instead focus on the long differences, we do find statistically significant declines in general revenues and property taxes. Point estimates for all other revenue sources are negative, but statistically insignificant.

The results here are estimated using models including the full set of controls. In Table A.1, we display estimates from models excluding baseline controls interacted by year. Across all outcomes, results are very similar.

Given that revenues are falling, we might expect to see a corresponding fall in expenditures. We repeat the same analytical procedure to examine how Section 4 coverage affected various expenditure categories.

We again start by investigating event study estimates in Figure 3. For most outcomes, pre-trends are not significantly different from zero. Highway and police spending, however, begin to fall before passage of the VRA, though both are stable in the years immediately following passage of the VRA. In the years following the reform, point estimates for all spending categories fall, and these declines tend to grow over time.

Table 3 presents results from the pooled regression on the effects of Section 4 coverage on log expenditures per capita. Overall, we find that for a one percentage point increase in the non-white share, there is a statistically significant .0026 decline in log total direct expenditures per capita. This fall corresponds to a 6.3% decline in counties with the mean non-white share relative to counties with an all-white population.

Next, we examine impacts on individual spending categories. For a one percentage point increase in the non-white share, we find that log per capita spending fell by a statistically significant .0073 (16.4% at the mean) for highways, .0062 (14.1%) for policing, and .01 (21.9%) for welfare.

As before, estimates over the full sample may not capture long-term effects. When focus-

ing on the long differences, we find large declines for other spending as well. In particular, log spending per capita on education falls by .0046 (10.7% at the mean).

When we estimate effects on the sample of Southern counties, we find no statistically significant impacts using the full sample. However, when we focus on the long differences, we find declines in direct expenditures, education spending, and highway spending that are similar to long difference estimates for the border pair sample. Estimates for other outcomes are not significant.

In Table A.2, we re-estimate all models excluding baseline controls interacted by year. As with revenues, results are insensitive to the inclusion of controls.

5.2 Structure of Local Government

The structure of local government plays a major role in how resources are distributed within counties. Previous work has argued that local communities manipulate government boundaries (e.g., by creating new cities or special districts) to maintain local control over resources and to block annexation from neighboring cities (e.g., Miller, 1981; Burns, 1994; Kruse, 2007).

We repeat the analytical procedure above to estimate the effect on various measures of fragmentation, including the total number of local governments (counties, municipalities, and special districts), the number of municipalities, and the number of special districts.¹² As before, all outcomes are log transformed and normalized for population.

Visual inspection of event study plots in Figure 4 reveals there are no pre-existing trends in the difference in fragmentation between covered counties with higher or lower non-white shares relative to never covered counties, as measured by any of our outcomes. However, following Section 4 coverage, covered counties with greater non-white shares experience an increase in the per capita number of governments overall. Impacts increase in the decade or so following coverage, but appear to plateau thereafter.

We present our main results in Table 4. Overall, we find that a one percentage point increase in the non-white share leads to a statistically significant .0030 increase in the log number of local governments per capita, an increase of 7.6% when evaluated at the mean. We also find that there is a statistically insignificant increase of .0017 in the log number of cities per capita (4.3% at the mean), and an increase of .004 in the log number of special

¹²In many states in the sample, including Florida, Maryland, North Carolina, Tennessee, and Virginia, school districts generally overlap with counties or independent cities. Consequently, there is little temporal variation in school district fragmentation that can be exploited in these states. For this reason, this paper does not examine the effect on school district fragmentation. Therefore, we also exclude school districts when calculating the total number of local governments. We also exclude townships from the count of total governments since townships are not common in the South.

districts per capita (10.3%), significant at the .1 level.

Point estimates associated with the long difference specification are slightly attenuated and are no longer statistically significant. The pooled estimates for Southern counties, displayed in Panel B, are also slightly attenuated, but are significant. Estimates excluding baseline controls, shared in Table A.3 are also similar to the main results.

These results are consistent with other episodes in the historical record. For example, as public spaces desegregated and Black communities gained political power during the Civil Rights era in Atlanta, white families moved away from the central city, often to un-incorporated areas on its periphery. These communities would then incorporate to prevent annexation from Atlanta, effectively preventing their tax dollars from financing public goods and services that support larger, more diverse communities (Kruse, 2007, pp. 247–248).

One challenge to this interpretation is that the Supreme Court ruled that incorporations are subject to preclearance. However, this determination was not made until *Perkins v. Matthews (1971)*. Additionally, incorporations made up roughly .2% of all preclearance submissions from 1970-1979, suggesting that many jurisdictions continued not to submit incorporations for preclearance, even after *Perkins* (see Figure A.3).

5.3 Heterogeneity

In this section, we examine heterogeneity in impacts on two key dimensions: the electoral system and the racial make-up of a population. However, there are two key challenges in examining heterogeneity using the main sample. First, the relatively small sample size limits statistical power. Second, because counties within a border pair tend to be observably similar, there is limited variation to exploit in the dimensions we explore. To address these limitations, we instead examine heterogeneity using the larger sample of all Southern counties.

First, we examine whether there is heterogeneity in impacts based on a jurisdiction's electoral system on a few key outcomes. Prior research suggests that Black voters were more successful in exerting political influence in jurisdictions with single-member districts (Bernini, Facchini, and Testa, 2023). In Panels A and B of Table 5, we estimate Equation 1 on counties with and without single-member districts, respectively. Among counties with single-member districts, we find that higher non-white shares are associated with slightly smaller revenues and expenditures, but these effects are not statistically significant. On the contrary, we find that in places without single-member districts – those with at-large systems – a one percentage point increase in the non-white share leads to a statistically significant decline of log general revenues per capita of .0026. For direct expenditures, we again find no

evidence of significant impacts in counties with single-member districts. For counties with at-large electoral systems, we do not find statistically significant impacts for the full panel, but we find evidence of significant negative effects on expenditures when examining long differences.

Results on government fragmentation, as measured by the log count of local governments (the sum of counties, municipalities, and special districts) per capita are more mixed. While point estimates are positive across counties with either type of electoral systems, estimates are larger and significant only in counties with single-member districts.

Next, we explore whether there is heterogeneity based on the racial make-up of a county. In places where racial minorities make up a large share of the population, non-white communities may be more likely to develop winning political coalitions after passage of the VRA. On the other hand, in places where the share of racial minorities is small, the enfranchisement of racial minorities may be perceived as a threat to the previously enfranchised community, but they may not be able to successfully advocate for or implement their preferred policies.

We report results for communities where the share of racial minorities greater or less than 30% in Panels C and D of Table 5, respectively. In places with non-white shares greater than 30%, impacts on overall revenues and expenditures are not statistically significant and, in fact, point estimates tend to be slightly positive. For counties with non-white shares less than or equal to 30%, a one percentage point increase in the non-white share leads to a statistically significant .0028 decline in log general revenues per capita. We find no impacts on expenditures for the full sample, but we do find the long difference for log expenditures per capita falls by a significant .0048. For government fragmentation, impacts are not significant when we break out by the pre-reform minority share, but point estimates are positive in places where racial minorities made up more than 30% of the population, and negative otherwise.

Overall, this analysis suggests that revenues and expenditures fall more in places with larger non-white populations relative to places with smaller ones, but only in those communities where racial minorities are less able to translate their newfound enfranchisement into winning political coalitions. The results further suggest that places where minority populations are more able to leverage their numbers, local communities may respond by fragmenting to preserve local control over resources.

6 Mechanisms

So far, we have established that revenues and expenditures declined in covered counties with greater non-white shares. We have also shown that fragmentation increased in response to Section 4 coverage, raising the possibility that individuals in covered counties facing greater electoral threat from newly enfranchised Black voters may have searched for ways to increase local control over resources. In this section, we explore why revenues and spending fell.

In particular, we might observe a relative decline in per capita revenues either as a mechanical response to underlying trends, or because individual policy preferences change, which may ultimately lead to policy changes that reduce revenues raised. We examine each explanation in turn.

6.1 Are declines just mechanical responses to other trends?

Even without legislative changes, communities could experience declines in local tax bases – due either to behavioral responses to the VRA or to broader secular trends – which would mechanically reduce taxes collected. To test for this possibility, I examine changes in a number of economic measures which could plausibly be correlated with changes in various tax bases. We estimate the impact of the reform on various potential mediators using the same empirical strategy and present results in Table 6. Each column represents a different potential mediator. Otherwise, the table is structured identically to those associated with the main results.

First, we test for changes in income. Changes in income might affect tax rates in myriad ways. Income is often taxed directly, though income taxes are not a major source of local revenue. Income is also strongly correlated with consumption, and consumption taxes *are* important sources of local revenue. Measures of income can also used to summarize the economic health of a population or region more generally, which may be correlated the size of a tax base. Finally, many transfers from higher levels of government are based on a county's economic health (or lack thereof). For example, some federal transfers are reserved for places with high shares of low-income individuals. For all of these reasons, a change in, say, average incomes, may be correlated with changes in various tax bases or other revenue sources. We find that, for a one percentage point increase in the non-white share, there are small, statistically insignificant effects on log per capita income or log median family income.

Given the overall importance of property taxes for local revenues¹³ and the large declines in per capita property taxes, we next test for potential changes in the property tax base. Among covered counties, a one percentage point increase in the non-white share leads to a statistically significant .0023 increase in the log owner-occupied share (5.7% at the mean). At the same time, we estimate a .0019 decline in log median home values (4.5% at the mean),

¹³In the main sample, property taxes make up roughly one-quarter of total taxes and about one-fifth of general revenues on average at baseline.

significant at the .1 level. Focusing only on these point estimates, it is unclear whether the net effect of these changes would be to increase or decrease the property tax base.

Finally, we examine whether the VRA had any impacts on county populations. Population size might affect per capita outcomes if changes in population do not lead to constant changes in revenues, spending, or number of governments per person. Additionally, changes in population may be indicative of changes in the broader fortunes of a county. If outmigration is associated with a county becoming more disadvantaged, for example, we might expect to see a decline in various tax bases, even if we do not observe incomes or housing values falling at the mean or median. I find there is no statistically significant effect of coverage on county population. The point estimate implies a small change – a decline of .6% for counties with the mean non-white share, relative to all-white counties – and we can rule out large increases or decreases.

The story is somewhat different among the sample of Southern counties, however. Impacts are larger (that is, more negative) and are significant for per capita incomes, median family incomes, and population among this sample. Effects on other outcomes are similar.

Though there are only modest changes in these potential mediators, we nevertheless attempt to more systematically test the extent to which changes in these variables may be driving the main results. In Table 7, we re-estimate the impacts of the main results on key outcomes, but control for different sets of potential mediators. In the first set of rows, we re-estimate the main estimating equations but add controls for log per capita income and log family income. In the second, we include controls for log owner occupied share and log median housing value. And in the third, we control for log population. Across nearly all samples, outcomes, and specifications, the potential mediators attenuate impacts, but only marginally. Including potential mediators only meaningfully impacts effects on government fragmentation in the Southern counties sample, where estimates are cut by roughly half.

There is one more piece of indirect evidence that may shed light on the how the economic health of counties changed after VRA passage. Recall that I previously found that covered counties with greater non-white shares experienced declines in federal and state revenues. Since transfers from higher levels of government are often based on need, it is possible that covered counties are becoming, if anything, somewhat *less* disadvantaged relative to their never covered neighbors during this period in ways not captured by the above measures.

Taken together, these investigations suggest there is limited evidence that counties are changing in ways that meaningfully drive the declines in revenues and expenditures we observe.

6.2 Are individual preferences for public goods changing?

An alternative explanation is that communities update their policy preferences in response to Section 4 coverage and implement policies that reduce per capita revenues. For example, communities could either choose to collect fewer taxes on the existing tax base (e.g., by decreasing tax rates) or to reduce the tax base (e.g., excluding incomes below a certain level from taxation). We do not observe local tax schedules, so we cannot test for these pathways directly. However, it may be possible to test for changing preferences in taxation indirectly. One reason individuals may demand lower taxes is because their preferences for public goods change: if individuals shift from public to private goods, they may perceive they do not benefit as much from public investments and, therefore, demand lower taxes.

Since education is often central in families' decisionmaking and makes up a large share of local budgets, we test for this channel by examining whether families substitute from public to private schooling using data from the Office of Civil Rights (OCR) on public school enrollment between 1968 and 1980. To study how public school enrollment changed in the years following passage of the VRA, we implement the same long difference approach we have used to analyze the main results. However, since we have no data prior to passage of the VRA, we examine impacts relative to 1968, three years after passage of the VRA. To conduct the analysis, we require a county – or pair of counties, for the border pair sample – to have observations for 1968 and for the last period in the panel (1980 for the border pair sample, and 1974 for the Southern counties). For the border pair sample, this means we lose about half our counties. For the sample of Southern counties, we lose about one-third.

We present results in Table 8. Each column represents a different outcome, each log transformed and normalized for county population. We analyze impacts on the total number of pupils and by race. Since the OCR data does not always directly record information on the white population for all counties in all years, we group children into those who are Black and Hispanic, and those who are not. During this period and for our sample of counties, nearly all children who are not Black or Hispanic are likely white.

In Panel A, we report results for the border pair sample. Overall, point estimates are negative – suggesting a one percentage point increase in the non-white share is associated with reductions in the number of public school students – but are statistically insignificant for all students and for students who are not Black or Hispanic. However, we do find negative, statistically significant impacts on the number of Black or Hispanic students. For all of these estimates, standard errors tend to be large, a consequence of the smaller sample available for this analysis.

Given the limited statistical power of the border pair sample, we focus primarily on the results using the larger sample of Southern counties, which we report in panel B. We find that, among covered counties, a one percentage point increase in the non-white share leads to a statistically significant .0097 decline in the log number of pupils per capita by 1974. Given that the mean non-white share among Southern counties is about 24%, this implies a decrease of 21.3% students per capita when evaluated at the mean relative to an all-white county. We find that this effect is largely concentrated among white students: from 1968 to 1974, a one percentage point increase in the non-white share leads to a .0172 decline in the log per capita number of pupils who are not Black or Hispanic, which corresponds to a 34.6% decline when evaluated at the mean.

Given that we do not find negative impacts on population, and that school enrollment and high school completion are rising during this period both overall and for racial minorities (Snyder, 1993, pp. 6–9), these results strongly suggest that families are choosing to send children to private school instead. Because data are only available starting in 1968 – three years *after* passage of the VRA – these findings are likely conservative.

The results presented here suggest that, following Section 4 coverage, families in communities with larger non-white shares responded to Section 4 coverage by substituting from public goods to private ones. If families' relative preferences for public goods are declining, we might expect that they would prefer less taxation and redistribution overall.

7 Conclusion

The Voting Rights Act of 1965 sought to fulfill the promise of the Fifteenth Amendment and secure nondiscrimination in voting for racial minorities. Previous research shows that the VRA largely succeeded in achieving its first-order goal of increasing Black political participation. The literature also finds that, by increasing Black political power, the VRA helped Black communities achieve more desirable political outcomes.

This paper studies how coverage under Section 4 of the Act, which determined whether counties were subject to certain special provisions of the VRA, affected local public finances more broadly. We find that places with greater non-white shares – where minority communities had the greatest potential to take advantage of their newfound political power – saw relative declines in revenues and spending. We further show that counties with higher non-white shares responded to Section 4 by increasing local government fragmentation, consistent with local communities being motivated to preserve local control over resources.

Our exploration of mechanisms suggests it is unlikely that changes in revenues were simply mechanical responses to declines in local tax bases – whether due to behavioral responses to the VRA or broader secular trends. Instead, the analysis in this paper provides some suggestive evidence that families in covered counties may have responded by substituting from public to private goods. If families do substitute toward private goods, they may also prefer less taxation, since they no longer receive the same benefits per dollar taxed.

The ultimate measure of the VRA lies not simply in its success in enfranchising racial minorities, but also in its ability to, in the words of President Johnson, "transform the vote into an instrument of justice and fulfillment." This paper adds texture to the existing literature on the Voting Rights Act by highlighting the array of ways that local communities can respond to expansions of the franchise and to federal attempts to ameliorate racial inequality. Understanding these responses is necessary for designing policies that not only guarantee all people voice in the political process, but that ultimately ensure individuals have the ability to shape their own destinies.

References

- Alesina, Alberto, Reza Baqir, and William Easterly. "Public Goods and Ethnic Divisions". *The Quarterly Journal of Economics* 114.4 (1999), pp. 1243–1284. URL: https:// academic.oup.com/qje/article-lookup/doi/10.1162/003355399556269.
- Alesina, Alberto, Reza Baqir, and Caroline Hoxby. "Political Jurisdictions in Heterogeneous Communities". Journal of Political Economy 112.2 (2004), pp. 348–396. URL: https: //www.journals.uchicago.edu/doi/10.1086/381474.
- Alesina, Alberto and Enrico Spolaore. "On the Number and Size of Nations". *The Quarterly Journal of Economics* 112.4 (1997), pp. 1027–1056. URL: https://academic.oup.com/gje/article-lookup/doi/10.1162/003355300555411.
- Aneja, Abhay and Carlos Avenancio-Leon. "The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Rights Act". Unpublished. (2019).
- Ang, Desmond. "Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight under the Voting Rights Act". American Economic Journal: Applied Economics 11.3 (2019), pp. 1–53. URL: https://pubs.aeaweb.org/doi/10.1257/app.20170572.
- Bernini, Andrea, Giovanni Facchini, Marco Tabellini, and Cecilia Testa. Black Empowerment and White Mobilization: The Effects of the Voting Rights Act. w31425. Cambridge, MA: National Bureau of Economic Research, 2023, w31425. URL: http://www.nber.org/ papers/w31425.pdf.
- Bernini, Andrea, Giovanni Facchini, and Cecilia Testa. "Race, Representation, and Local Governments in the US South: The Effect of the Voting Rights Act". Journal of Political Economy 131.4 (2023), pp. 994–1056. URL: https://www.journals.uchicago.edu/ doi/10.1086/722092.
- Bolton, Patrick and Gerard Roland. "The Breakup of Nations: A Political Economy Analysis". The Quarterly Journal of Economics 112.4 (1997), pp. 1057–1090. URL: https: //academic.oup.com/qje/article-lookup/doi/10.1162/003355300555420.
- Boustan, Leah Platt. "Was Postwar Suburbanization "White Flight"? Evidence from the Black Migration". *Quarterly Journal of Economics* 125.1 (2010), pp. 417-443. URL: https://academic.oup.com/qje/article-lookup/doi/10.1162/qjec.2010.125.1.417.
- Bureau, U.S. Census. "County and City Data Book [United States] Consolidated File: County Data, 1947-1977: Version 2". Version v2. 2012. URL: https://www.icpsr.umich.edu/web/ICPSR/studies/7736/versions/V2.
- "Census of Governments, 1962 and 1967: Version 2". 2018. URL: https://www.icpsr. umich.edu/icpsrweb/ICPSR/studies/17/versions/V2.

- Burns, Nancy. The formation of American local governments: private values in public institutions. New York: Oxford University Press, 1994. 194 pp.
- Cascio, Elizabeth U. and Ebonya Washington. "Valuing the Vote: The Redistribution of Voting Rights and State Funds following the Voting Rights Act of 1965 *". The Quarterly Journal of Economics 129.1 (2014), pp. 379–433. URL: https://academic.oup.com/ qje/article/129/1/379/1897098.
- Cox, Gary W. and Mathew D. McCubbins. "Electoral Politics as a Redistributive Game". The Journal of Politics 48.2 (1986), pp. 370-389. URL: https://www.journals. uchicago.edu/doi/10.2307/2131098.
- Cui, Tianfang. "Did Race Fence Off The American City? The Great Migration and the Evolution of Exclusionary Zoning". *Working Paper* (2024). URL: https://www.tom-cui.com/assets/pdfs/LotsEZ_Latest.pdf.
- Cutler, David, Douglas Elmendorf, and Richard Zeckhauser. "Demographic characteristics and the public bundle". *Public Finance* 48 (1993), pp. 178–198.
- Derenoncourt, Ellora. "Can You Move to Opportunity? Evidence from the Great Migration". *American Economic Review* 112.2 (2022), pp. 369–408. URL: https://pubs.aeaweb. org/doi/10.1257/aer.20200002.
- Dixit, Avinash and John Londregan. "The Determinants of Success of Special Interests in Redistributive Politics". *The Journal of Politics* 58.4 (1996), pp. 1132–1155. URL: https://www.journals.uchicago.edu/doi/10.2307/2960152.
- "Fiscal federalism and redistributive politics". Journal of Public Economics 68.2 (1998), pp. 153–180. URL: https://linkinghub.elsevier.com/retrieve/pii/S0047272797000972.
- "Ideology, Tactics, and Efficiency in Redistributive Politics". The Quarterly Journal of Economics 113.2 (1998), pp. 497–529. URL: https://academic.oup.com/qje/articlelookup/doi/10.1162/003355398555667.
- Dube, Arindrajit, T. William Lester, and Michael Reich. "Minimum Wage Shocks, Employment Flows, and Labor Market Frictions". *Journal of Labor Economics* 34.3 (2016), pp. 663–704. URL: https://www.journals.uchicago.edu/doi/10.1086/685449.
- Durst, Noah J. "Racial Gerrymandering of Municipal Borders: Direct Democracy, Participatory Democracy, and Voting Rights in the United States". Annals of the American Association of Geographers 108.4 (2018), pp. 938–954. URL: https://www.tandfonline. com/doi/full/10.1080/24694452.2017.1403880.
- "Race and Municipal Annexation After the Voting Rights Act". Journal of the American Planning Association 85.1 (2019), pp. 49-59. URL: https://www.tandfonline.com/ doi/full/10.1080/01944363.2018.1556113.

- Eubank, Nicholas and Adriane Fresh. "Enfranchisement and Incarceration after the 1965 Voting Rights Act". American Political Science Review (2022), pp. 1–16. URL: https:// www.cambridge.org/core/product/identifier/S0003055421001337/type/journal_ article.
- Facchini, Giovanni, Brian Knight, and Cecilia Testa. The Franchise, Policing, and Race: Evidence from Arrests Data and the Voting Rights Act. w27463. Cambridge, MA: National Bureau of Economic Research, 2020, w27463. URL: http://www.nber.org/papers/ w27463.pdf.
- Fresh, Adriane. "The Effect of the Voting Rights Act on Enfranchisement: Evidence from North Carolina". The Journal of Politics 80.2 (2018), pp. 713-718. URL: https://www.journals.uchicago.edu/doi/10.1086/697592.
- Goldin, Claudia and Lawrence Katz. "Human Capital and Social Capital: The Rise of Secondary Schooling in America, 1910-1940". The Journal of Interdisciplinary History 29.4 (1999), pp. 683–723.
- Haines, Michael R. "Historical, Demographic, Economic, and Social Data: The United States, 1790-2002: Version 3". Version v3. 2010. URL: https://www.icpsr.umich.edu/web/ ICPSR/studies/2896/versions/V3.
- Husted, Thomas A. and Lawrence W. Kenny. "The Effect of the Expansion of the Voting Franchise on the Size of Government". *Journal of Political Economy* 105.1 (1997), pp. 54– 82. URL: https://www.journals.uchicago.edu/doi/10.1086/262065.
- Jackson, Thomas F. From civil rights to human rights: Martin Luther King, Jr., and the struggle for economic justice. Politics and culture in modern America. Philadelphia, Pa: University of Pennsylvania Press, 2007. 459 pp.
- Kruse, Kevin Michael. White flight: Atlanta and the making of modern conservatism. 3. print., 1. paperback print. Politics and society in twentieth-century America. Princeton, N.J. Oxford: Princeton Univ. Press, 2007. 325 pp.
- Kuziemko, Ilyana and Ebonya Washington. "Why Did the Democrats Lose the South? Bringing New Data to an Old Debate". American Economic Review 108.10 (2018), pp. 2830– 2867. URL: https://pubs.aeaweb.org/doi/10.1257/aer.20161413.
- Lindbeck, Assar and Jorgen Weibull. "Balanced-budget redistribution as the outcome of political competition". *Public Choice* 52.3 (1987), pp. 273-297. URL: http://link. springer.com/10.1007/BF00116710.
- Luttmer, Erzo F. P. "Group Loyalty and the Taste for Redistribution". Journal of Political Economy 109.3 (2001), pp. 500-528. URL: https://www.journals.uchicago.edu/doi/ 10.1086/321019.

- McGhee, Heather C. The sum of us: what racism costs everyone and how we can prosper together. First edition. New York: One World, 2021. 1 p.
- Meltzer, Allan H. and Scott F. Richard. "A Rational Theory of the Size of Government". Journal of Political Economy 89.5 (1981), pp. 914-927. URL: https://www.journals. uchicago.edu/doi/10.1086/261013.
- Miller, Gary J. Cities by contract: the politics of municipal incorporation. Cambridge, Mass: MIT Press, 1981. 242 pp.
- Oates, Wallace E. "An Essay on Fiscal Federalism". Journal of Economic Literature 37.3 (1999), pp. 1120–1149. URL: https://pubs.aeaweb.org/doi/10.1257/jel.37.3.1120.
- Poterba, James M. "Demographic structure and the political economy of public education". *Journal of Policy Analysis and Management* 16.1 (1997), pp. 48-66. URL: https://onlinelibrary.wiley.com/doi/10.1002/(SICI)1520-6688(199724)16:1%3C48:: AID-PAM3%3E3.0.C0;2-I.
- Reber, Sarah and Ben Denckla. "Office of Civil Rights School Survey Data". 2006. URL: https://web.archive.org/web/20150109135107/http://l1.ccpr.ucla.edu/OCR/ ocr.htm.
- Roberts, Kevin W.S. "Voting over income tax schedules". *Journal of Public Economics* 8.3 (1977), pp. 329–340. URL: https://linkinghub.elsevier.com/retrieve/pii/0047272777900056.
- Romer, Thomas. "Individual welfare, majority voting, and the properties of a linear income tax". *Journal of Public Economics* 4.2 (1975), pp. 163–185. URL: https://linkinghub.elsevier.com/retrieve/pii/004727277590016X.
- Sahn, Alexander. Racial Diversity and Exclusionary Zoning: Evidence from the Great Migration. Unpublished. 2022. URL: https://drive.google.com/file/d/1Dd0KKH7oS1mxNVAOblRTj-rmnXOwIOD/view?usp=share_link.
- Snyder, Thomas. 120 Years of American Education: A Statistical Portrait. U.S. Department of Education, Office of Educational Research and Improvement, 1993.
- Tribe, Laurence. American Constitutional Law. Foundation Press, 1978.
- U.S. Census Bureau. "Population, Supplementary Reports: Negro Population, by County 1960 and 1950". 1966. URL: https://www.census.gov/library/publications/1966/ dec/population-pc-s1-52.html.
- "Census of Governments, Individual Finances 1967-2012". 2015. URL: https://www. census.gov/programs-surveys/gov-finances/data/historical-data.html.
- "County Intercensal Tables: 1970-1979". 2016. URL: https://www.census.gov/data/ tables/time-series/demo/popest/pre-1980-county.html.

- U.S. Census Bureau. "Historical County Level Poverty Estimates Tool". URL: https:// www.census.gov/library/visualizations/time-series/demo/census-povertytool.html.
- Wright, Gavin. Sharing the prize: the economics of the civil rights revolution in the American South. Cambridge, Mass: Belknap Press of Harvard University Press, 2013. 353 pp.

8 Figures and Tables



Figure 1: Counties Covered Under Section 4 and Their Neighbors

Source: Department of Justice



Figure 2: Effect of Section 4 on Revenues, CBP Sample

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.



Figure 3: Effect of Section 4 on Expenditures, CBP Sample

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.



Figure 4: Effect of Section 4 on Fragmentation, CBP Sample

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.

Characteristic	Ever Covered	Never Covered	Difference
A. CBP Sample			
Land Area (Sq Mi)	533	570	-37
Population	$47,\!347$	62,932	$-15,\!585$
Urban (%)	26.88	26.18	0.70
Non-white $(\%)$	25.19	24.09	1.11
Black (%)	24.94	23.88	1.06
Med Age	25.24	26.52	-1.27^{***}
Foreign Born $(\%)$	0.46	0.52	-0.06
Spanish Heritage $(\%)$	0.26	0.19	0.07
HS Graduate $(\%)$	27.29	27.86	-0.57
Med Yrs Education	8.48	8.75	-0.27
# Employed	$15,\!687$	22,752	-7,065
Med Family Income	3487.52	3470.91	16.60
Employed $(\%)$	32.03	32.06	-0.03
Observations	253	253	
B. Southern Counties			
Land Area (Sq Mi)	498	784	-285***
Population	35,797	42,899	-7,102
Urban (%)	28.45	32.53	-4.08**
Non-white (%)	33.41	13.21	20.20***
Black (%)	33.28	13.04	20.24^{***}
Med Age	25.12	29.43	-4.31***
Foreign Born $(\%)$	0.37	1.39	-1.03^{***}
Spanish Heritage $(\%)$	0.19	8.06	-7.87***
HS Graduate $(\%)$	26.11	29.19	-3.08***
Med Yrs Education	8.37	9.08	-0.71^{***}
# Employed	11,824	$14,\!942$	$-3,118^{*}$
Med Family Income	3298.84	3552.12	-253.28^{***}
Employed $(\%)$	32.08	32.97	-0.89***
Observations	584	578	

Table 1: 1960 County Characteristics by Section 4 Coverage Status

Notes: The table compares mean county characteristics for counties covered by Section 4 in 1965 to those that were not. Tests for statistical significance cluster standard errors at the county level.

	Gen Rev	Prop Tax	Non-Prop Tax	Fed Transfers	State Transfers	Charges
A. CBP Sample						
Pooled	-0.0033***	-0.0041^{***}	-0.0074	-0.0037**	-0.0039***	0.0018
	(0.0011)	(0.0011)	(0.0063)	(0.0015)	(0.0015)	(0.0029)
Number of Observations	$3,\!514$	3,332	3,318	3,402	$3,\!402$	$3,\!290$
Number of Counties	502	476	474	486	486	470
Long Difference	-0.0035***	-0.0042^{**}	-0.0123	-0.0060***	-0.0068***	0.0042
	(0.0014)	(0.0018)	(0.0093)	(0.0018)	(0.0018)	(0.0034)
Number of Observations	$1,\!004$	952	948	972	972	940
Number of Counties	502	476	474	486	486	470
B. Southern Counties						
Pooled	-0.0010	-0.0010	-0.0005	-0.0004	-0.0007	-0.0013
	(0.0006)	(0.0008)	(0.0017)	(0.0008)	(0.0007)	(0.0018)
Number of Observations	4,454	4,481	4,429	4,429	4,425	4,450
Number of Counties	$1,\!114$	1,121	1,108	1,108	1,107	$1,\!113$
Long Difference	-0.0019^{***}	-0.0023**	-0.0026	-0.0009	-0.0014	-0.0028
	(0.0007)	(0.0010)	(0.0024)	(0.0009)	(0.0008)	(0.0023)
Number of Observations	2,228	2,242	2,216	2,216	2,214	2,226
Number of Counties	$1,\!114$	$1,\!121$	$1,\!108$	$1,\!108$	$1,\!107$	$1,\!113$

Table 2: Effect of Section 4 on Revenues

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level.

p < .1, p < .05, p < .01

	Direct Exp	Education	Highways	Police	Welfare
A. CBP Sample					
Pooled	-0.0026**	-0.0009	-0.0073***	-0.0062***	-0.0100^{*}
	(0.0013)	(0.0015)	(0.0025)	(0.0021)	(0.0056)
Number of Observations	$3,\!458$	$3,\!318$	$3,\!094$	$3,\!416$	2,212
Number of Counties	494	474	442	488	316
	0.0000*	0 00 (0****			0.0100
Long Difference	-0.0026*	-0.0046***	-0.0079	-0.0058*	-0.0180
	(0.0015)	(0.0015)	(0.0048)	(0.0033)	(0.0114)
Number of Observations	988	948	884	976	632
Number of Counties	494	474	442	488	316
B. Southern Counties					
Pooled	-0.0010	-0.0006	-0.0024^{*}	-0.0003	0.0006
	(0.0007)	(0.0008)	(0.0014)	(0.0011)	(0.0032)
Number of Observations	4,518	4,449	4,362	4,481	$3,\!670$
Number of Counties	$1,\!130$	$1,\!113$	1,091	$1,\!121$	918
Long Difference	-0.0032***	-0.0022**	-0.0052***	-0.0001	0.0016
	(0.0010)	(0.0009)	(0.0018)	(0.0014)	(0.0048)
Number of Observations	2.260	2.226	2.182	2.242	1.836
Number of Counties	1.130	1.113	1.091	1.121	918
	-,	-,0	-,	-,- - -	010

Table 3: Effect of Section 4 on Expenditures

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level.

	# Govt	# Cities	# Spec Dist
A. CBP Sample			
Pooled	0.0030^{**}	0.0017	0.0040^{*}
	(0.0013)	(0.0010)	(0.0022)
Number of Observations	$3,\!542$	$3,\!542$	$3,\!542$
Number of Counties	506	506	506
Long Difference	0.0023	0.0015	0.0032
	(0.0021)	(0.0017)	(0.0036)
Number of Observations	1,012	1,012	1,012
Number of Counties	506	506	506
B. Southern Counties			
Pooled	0.0022^{***}	0.0015^{**}	0.0028^{**}
	(0.0007)	(0.0007)	(0.0014)
Number of Observations	4,535	4,536	4,535
Number of Counties	$1,\!135$	$1,\!135$	$1,\!135$
I D'A	0 001 7**	0.0004	0.0001**
Long Difference	0.0017***	0.0004	0.0031***
	(0.0008)	(0.0008)	(0.0016)
Number of Observations	$2,\!270$	$2,\!270$	$2,\!270$
Number of Counties	$1,\!135$	$1,\!135$	$1,\!135$

 Table 4: Effect of Section 4 on Government Fragmentation

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level. *p<.1, **p<.05, ***p<.01

A. SMD -0.0004 -0.0007 0.0018*** Pooled -0.0004 (0.0009) (0.0009) Number of Observations 2,754 2,762 2,780 Number of Counties 689 691 696 Long Difference -0.0010 -0.0019 0.0007 (0.0009) (0.0013) (0.0010) Number of Observations 1,378 1,382 1,392 Number of Counties 689 691 696 B. No SMD -0.0026** -0.0012 0.0012 Pooled -0.0026** -0.0012 0.0012 Number of Observations 1,700 1,756 1,755 Number of Counties 425 439 439 Long Difference -0.0032*** -0.0039** 0.0012 (0.0012) (0.0017) (0.0014) Number of Counties 425 Number of Observations 850 878 878 Number of Observations 1,540 1,580 1,592 Number of Counties 385 395 398 Long Difference 0.0008 0.		Gen Rev	Direct Exp	# Govt
Pooled -0.0004 -0.0007 0.0018** Number of Observations 2,754 2,762 2,780 Number of Counties 689 691 696 Long Difference -0.0010 -0.0019 0.0007 (0.0009) (0.0013) (0.0010) Number of Observations 1,378 1,382 1,392 Number of Counties 689 691 696 B. No SMD -0.0026** -0.0012 0.0012 Pooled -0.0028** -0.0012 0.0012 Number of Observations 1,700 1,756 1,755 Number of Counties 425 439 439 Long Difference -0.0032*** -0.0039** 0.0012 (0.0012) (0.0017) (0.0014) Number of Observations 850 878 878 Number of Observations 850 878 878 Number of Observations 1,540 1,580 1,592 Number of Observations 1,540 1,580 1,592 Number of Observations 770 790 796	A. SMD			
Number of Observations Number of Counties (0.0008) (0.0010) (0.0009) Number of Counties $2,754$ $2,762$ $2,780$ Sumber of Counties 689 691 696 Long Difference -0.0010 -0.0019 0.0007 Number of Observations $1,378$ $1,382$ $1,392$ Number of Counties 689 691 696 B. No SMD -0.0026^{**} -0.0012 0.0012 Pooled -0.0026^{**} -0.0012 0.0012 Number of Observations $1,700$ $1,756$ $1,755$ Number of Counties 425 439 439 Long Difference -0.0032^{***} -0.0039^{**} 0.0012 Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority $\% > 30\%$ $Pooled$ 0.0013 0.0038^* 0.0020 Number of Observations $1,540$ $1,580$ $1,592$ Number of Observations 385 395 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0012 (0.0014) (0.0017) (0.0012) Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} <td>Pooled</td> <td>-0.0004</td> <td>-0.0007</td> <td>0.0018**</td>	Pooled	-0.0004	-0.0007	0.0018**
Number of Observations Number of Counties $2,754$ 689 $2,762$ 691 $2,780$ 696 Long Difference 		(0.0008)	(0.0010)	(0.0009)
Number of Counties689691696Long Difference-0.0010-0.00190.0007 (0.0009) (0.0013) (0.0010) Number of Observations $1,378$ $1,382$ $1,392$ Number of Counties 689 691 696 B. No SMD-0.0026**-0.0012 0.0012 Pooled -0.0026^{**} -0.0012 0.0012 Number of Observations $1,700$ $1,756$ $1,755$ Number of Observations $1,700$ $1,756$ $1,755$ Number of Observations 425 439 439 Long Difference -0.0032^{***} -0.0039^{**} 0.0012 (0.0012) (0.0017) (0.0014) (0.0014) Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority $\% > 30\%$ 0.0013 0.0038^* 0.0020 Pooled 0.0013 0.0038^* 0.0020 Number of Observations $1,540$ $1,580$ $1,592$ Number of Observations 770 790 796 Number of Observations 770 790 796 Number of Observations $2,914$ $2,938$ $2,943$ Number of Observations $2,914$ $2,938$ $2,943$ Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations 729 <	Number of Observations	2.754	2.762	2.780
Long Difference-0.0010 (0.0009)-0.0019 (0.0013)0.0007 (0.0013)Number of Observations Number of Counties1,378 6891,382 6911,392 696B. No SMD Pooled-0.0026** (0.0010)-0.0012 (0.0013)0.0012 (0.0012)Number of Observations Number of Counties1,700 4251,756 4391,755 439Long Difference (0.0012)-0.0032*** (0.0012)-0.0039** (0.0017)0.0012 (0.0017)Number of Observations Number of Counties850 425878 439878 439C. Minority $\% > 30\%$ Pooled0.0013 (0.0019)0.0020 (0.0019)0.0020 (0.0019)Number of Observations Number of Counties1,540 385 3951,580 3981,592 398Long Difference (0.0020)0.0008 (0.0025)0.0010 (0.0023)0.0012 (0.0023)Number of Observations Number of Counties770 385 395398D. Minority $\% \le 30\%$ Pooled-0.0028** (0.0014)-0.0012 (0.0017)-0.0012 (0.0013)Number of Observations Number of Counties729 735 737-0.0048** 735 737-0.0016 (0.0023)Number of Observations Number of Counties2,914 2,938 2,9432,943 737Long Difference (0.0016)-0.0052*** (0.0023)-0.0016 (0.0023)Number of Observations Number of Observations1,458 729 735 737-0.0016 737	Number of Counties	689	691	696
$\begin{array}{c c c c c c c c c c c c c c c c c c c $				
Long Limitation(0.0009)(0.0013)(0.0010)Number of Observations1,3781,3821,392Number of Counties689691696B. No SMD(0.0010)(0.0013)(0.0012)Pooled-0.0026**-0.00120.0012(0.0010)(0.0013)(0.0012)Number of Observations1,7001,7561,755Number of Counties425439439Long Difference-0.0032***-0.0039**0.0012(0.0012)(0.0017)(0.0014)Number of Observations850878878Number of Observations850878878878Number of Counties425439439439C. Minority $\% > 30\%$ Pooled0.00130.0038*0.0020Pooled0.00130.0038*0.0020(0.0019)Number of Observations1,5401,5801,592Number of Counties385395398Long Difference0.00080.00290.0010(0.0020)(0.0025)(0.0023)Number of Observations770790796Number of Observations2,9142,9382,943Number of Counties729735737Long Difference-0.0052***-0.0048**-0.0016(0.0016)(0.0023)(0.0021)Number of Observations1,4581,4701,474Number of Observations729735737Long Difference-0.00	Long Difference	-0.0010	-0.0019	0.0007
Number of Observations Number of Counties1,378 6891,382 6911,392 696B. No SMD Pooled -0.0026^{**} (0.0010) -0.0012 (0.0013) 0.0012 (0.0012)Number of Observations Number of Counties $1,700$ 425 $1,756$ 439 $1,755$ 439Long Difference Number of Observations Observations -0.0032^{***} (0.0012) -0.0012 (0.0017) 0.0012 (0.0014)Number of Observations Number of Counties 425 425 439 439 439 C. Minority % > 30% Pooled 0.0013 (0.0019) 0.0038^* (0.0020) (0.0020) 0.0019 (0.0019)Number of Observations Number of Counties 385 395 395 398Long Difference (0.0020) 0.0008 (0.0022) (0.0023) 0.0010 (0.0025) (0.0023)Number of Observations Number of Counties 770 385 395 790 398D. Minority % $\leq 30\%$ Pooled -0.0028^{**} (0.0014) (0.0017) (0.0017) (0.0018) -0.0012 (0.0014) (0.0017) (0.0018)Number of Observations Number of Counties 729 735 737 -0.0048^{**} - 0.0016 (0.0023) (0.0021) Number of Observations 729Number of Observations Number of Observations Number of Observations 729 -0.0048^{**} - -0.0016 (0.0023) (0.0021) Number of Observations 729Number of Observations Number of Observations Number of Observations 729 -0.0048^{**} - -0.0016 (0.0021) Number of Observations 729Number of Observations Number of Observations Number of O		(0.0009)	(0.0013)	(0.0010)
Number of Counties 689 691 696 B. No SMD -0.0026** -0.0012 0.0012 Pooled -0.0026** -0.0012 0.0012 Number of Observations 1,700 1,756 1,755 Number of Counties 425 439 439 Long Difference -0.0032*** -0.0039** 0.0012 (0.0012) (0.0017) (0.0014) Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority % > 30% - - - 0.0013 0.0038* 0.0020 Pooled 0.0013 0.0020) (0.0019) 1.580 1.592 Number of Observations 1,540 1,580 1.592 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0023) Number of Observations 770 790 796 398 D. Minority % ≤ 30% - - -0.0028** -0.0003 -0.0	Number of Observations	1.378	1.382	1.392
B. No SMD Pooled -0.0026^{**} -0.0012 0.0012 Number of Observations $1,700$ $1,756$ $1,755$ Number of Counties 425 439 439 Long Difference -0.0032^{***} -0.0039^{**} 0.0012 Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority $\% > 30\%$ 0.0013 0.0038^* 0.0020 Pooled 0.0013 0.0038^* 0.0020 Number of Observations $1,540$ $1,580$ $1,592$ Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority $\% \leq 30\%$ -0.0028^{**} -0.0003 -0.0012 Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***	Number of Counties	689	691	696
B. No SMD -0.0026** -0.0012 0.0012 Pooled -0.0026** -0.0012 0.0012 Number of Observations 1,700 1,756 1,755 Number of Counties 425 439 439 Long Difference -0.0032*** -0.0039** 0.0012 (0.0012) (0.0017) (0.0014) Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority % > 30% Pooled 0.0013 0.0038* 0.0020 Pooled 0.0013 0.0020 (0.0019) (0.0020) (0.0019) Number of Observations 1,540 1,580 1,592 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) (0.0023) Number of Observations 770 790 796 Number of Observations 770 790 796 Number of Observations 2,914 2,938 2,943 Number of Observations 2,914 2,938 2,943				
Pooled -0.0026^{++} -0.0012 0.0012 Number of Observations $1,700$ $1,756$ $1,755$ Number of Counties 425 439 439 Long Difference -0.0032^{***} -0.0039^{**} 0.0012 (0.0012) (0.0017) (0.0014) Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority % > 30% 0.0013 0.0038^{*} 0.0020 Pooled 0.0013 0.0038^{*} 0.0020 Number of Observations $1,540$ $1,580$ $1,592$ Number of Observations $1,540$ $1,580$ $1,592$ Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Observations $2,914$ $2,938$ $2,943$ Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ $1,474$ Number of Observations $1,458$ $1,470$ $1,474$ Number of Observations 750	B. No SMD	0.00000**	0.0010	0.0010
(0.0010) (0.0013) (0.0012) Number of Observations $1,700$ $1,756$ $1,755$ Number of Counties 425 439 439 Long Difference -0.0032^{***} -0.0039^{**} 0.0012 (0.0012) (0.0017) (0.0014) Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority % > 30% 425 439 439 Pooled 0.0013 0.0038^* 0.0020 (0.0019) (0.0020) (0.0019) Number of Observations $1,540$ $1,580$ $1,540$ $1,580$ $1,592$ Number of Counties 385 395 398 Long Difference 0.0008 0.0029 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 385 395 398 D. Minority % $\leq 30\%$ Pooled -0.0028^{**} -0.0003 -0.0012 (0.0014) (0.0017) (0.0014) (0.0017) (0.0018) Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ Number of Observations $1,458$ $1,470$ Number of Observations $1,458$ $1,470$ <t< td=""><td>Pooled</td><td>-0.0026***</td><td>-0.0012</td><td>0.0012</td></t<>	Pooled	-0.0026***	-0.0012	0.0012
Number of Observations1,7001,7561,755Number of Counties425439439Long Difference -0.0032^{***} -0.0039^{**} 0.0012 (0.0012)(0.0017)(0.0014)Number of Observations850878878Number of Counties425439439C. Minority % > 30% 425 439439Pooled0.00130.0038*0.0020(0.0019)(0.0020)(0.0019)Number of Observations1,5401,5801,5401,5801,592Number of Counties385395398Long Difference0.00080.0029(0.0020)(0.0025)(0.0023)Number of Observations770790796Number of Counties385395D. Minority % \leq 30% -0.0028^{**} -0.0003 -0.0012 Pooled -0.0028^{**} -0.0003 -0.0012 Number of Observations729735737Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016)(0.0023)(0.0021)Number of Observations1,4581,4701,474Number of Observations1,4581,4701,474		(0.0010)	(0.0013)	(0.0012)
Number of Counties 425 439 439 Long Difference -0.0032^{***} -0.0039^{**} 0.0012 Number of Observations 850 878 878 Number of Counties 425 439 439 C. Minority % > 30% 425 439 439 Pooled 0.0013 0.0038^* 0.0020 Number of Observations $1,540$ $1,580$ $1,592$ Number of Observations $1,540$ $1,580$ $1,592$ Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority % $\leq 30\%$ -0.0028^{**} -0.0003 -0.0012 Pooled -0.0028^{**} -0.0003 -0.0012 Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 Number of Observations $1,458$ $1,470$ $1,474$ Number of Observations $1,458$ $1,470$ $1,474$	Number of Observations	1,700	1,756	1,755
Long Difference -0.0032^{***} -0.0039^{**} 0.0012 Number of Observations850878878Number of Counties425439439C. Minority % > 30%0.00130.0038*0.0020Pooled0.00130.0038*0.0020(0.0019)(0.0020)(0.0019)Number of Observations1,5401,580Number of Counties385395System395398Long Difference0.00080.0029(0.0020)(0.0023)(0.0023)Number of Observations770790Number of Counties385395Jone of Counties385395Jumber of Observations770790Number of Observations770790Number of Counties385395Jone of Counties385395Jumber of Observations2,9142,938Long Difference -0.0052^{***} -0.0048^{**} Number of Observations729735Number of Observations1,4581,470Number of Observatio	Number of Counties	425	439	439
Long Difference-0.0032***-0.0039**0.0012 (0.0012) (0.0017) (0.0014) Number of Observations850878878Number of Counties425439439C. Minority % > 30%0.00130.0038*0.0020Pooled 0.0013 0.0038*0.0020 (0.0019) (0.0020) (0.0019) Number of Observations1,5401,580Number of Counties385395System of Counties385395Number of Observations770790Number of Observations770790Number of Counties385395D. Minority % \leq 30%-0.0028**-0.0003Pooled -0.0028^{**} -0.0003Number of Observations2,9142,9382,943735737Long Difference -0.0052^{***} -0.0048**Number of Observations1,4581,470Number of Observations729735Number of Observations729735Number of Observations1,4581,470Number of Observations1,4581,470Number of Observations1,4581,470Number of Observations1,4581,474Number of Observations1,4581,470Number of Observations1,4581,470Number of Observations1,4581,470Number of Observations1,4581,470Number of Observations1,4581,470 <td< td=""><td>I D:#</td><td>0 0020***</td><td>0.0020**</td><td>0.0019</td></td<>	I D:#	0 0020***	0.0020**	0.0019
Number of Observations Number of Counties 850 878 878 Number of Counties 425 439 439 C. Minority % > 30% Pooled 0.0013 0.0038^* 0.0020 Number of Observations $1,540$ $1,580$ $1,592$ Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority % $\leq 30\%$ Pooled -0.0028^{**} -0.0003 -0.0012 Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ $1,474$ Number of Observations $1,458$ $1,470$ $1,474$	Long Difference	-0.0032	-0.0039	(0.0012)
Number of Observations850878878Number of Counties425439439C. Minority $\% > 30\%$ Pooled0.00130.0038*0.0020 (0.0019)Number of Observations1,5401,5801,592 398Number of Counties385395398Long Difference0.00080.00290.0010 (0.0020)Number of Observations770790796 Number of CountiesNumber of Observations770790796 Number of CountiesD. Minority $\% \le 30\%$ Pooled-0.0028**-0.0003-0.0012 (0.0014)Number of Observations2,9142,9382,943 Number of CountiesNumber of Observations729735737Long Difference-0.0052***-0.0048**-0.0016 (0.0023)Number of Observations1,4581,4701,474 Number of ObservationsNumber of Observations1,4581,4701,474	Normhan af Obarratiana	(0.0012)	(0.0017)	(0.0014)
Number of Counties 425 439 439 C. Minority $\% > 30\%$ 0.0013 0.0038* 0.0020 Pooled 0.0019 (0.0020) (0.0019) Number of Observations 1,540 1,580 1,592 Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority $\% \le 30\%$ -0.0028** -0.0003 -0.0012 Number of Observations 2,914 2,938 2,943 Number of Counties 729 735 737 Long Difference -0.0052*** -0.0048** -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations 1,458 1,470 1,474	Number of Observations	800	878	878
C. Minority % > 30% Pooled 0.0013 0.0038* 0.0020 Number of Observations 1,540 1,580 1,592 Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority % ≤ 30% -0.0028** -0.0003 -0.0012 Pooled -0.0028** -0.0003 -0.0012 Number of Observations 2,914 2,938 2,943 Number of Counties 729 735 737 Long Difference -0.0052*** -0.0048** -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations 1,458 1,470 1,474	Number of Counties	425	439	439
Pooled 0.0013 (0.0019) 0.0038^* (0.0020) 0.0020 (0.0019) Number of Observations $1,540$ 385 $1,580$ 395 $1,592$ 398 Long Difference 0.0008 (0.0020) 0.0029 (0.0025) 0.0010 (0.0023) Number of Observations Number of Counties 770 385 790 395 796 398 D. Minority $\% \leq 30\%$ Pooled -0.0028^{**} (0.0014) -0.0012 (0.0017) 0.0012 (0.0018) Number of Observations Number of Counties 729 735 737 Long Difference -0.0052^{***} 729 -0.0048^{**} 735 -0.0016 (0.0021) Number of Observations Number of Observations $1,458$ $1,470$ $1,474$	C. Minority $\% > 30\%$			
$\begin{array}{cccccccccccccccccccccccccccccccccccc$	Pooled	0.0013	0.0038^{*}	0.0020
Number of Observations $1,540$ $1,580$ $1,592$ Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority $\% \leq 30\%$ (0.0014) (0.0017) (0.0018) Number of Observations $2,914$ $2,938$ $2,943$ Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ $1,474$		(0.0019)	(0.0020)	(0.0019)
Number of Counties 385 395 398 Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority % $\leq 30\%$ $0.0014)$ (0.0017) (0.0018) Number of Observations $2,914$ $2,938$ $2,943$ Number of Observations 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ $1,474$	Number of Observations	1,540	1,580	1,592
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$	Number of Counties	385	395	398
Long Difference 0.0008 0.0029 0.0010 (0.0020) (0.0025) (0.0023) Number of Observations 770 790 796 Number of Counties 385 395 398 D. Minority % $\leq 30\%$ -0.0028^{**} -0.0003 -0.0012 Pooled -0.0028^{**} $-0.0017)$ (0.0018) Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ $1,474$				
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$	Long Difference	0.0008	0.0029	0.0010
Number of Observations770790796Number of Counties385395398D. Minority $\% \leq 30\%$ -0.0028**-0.0003-0.0012Pooled-0.0028**-0.0003-0.0012(0.0014)(0.0017)(0.0018)Number of Observations2,9142,9382,943Number of Counties729735737Long Difference-0.0052***-0.0048**-0.0016(0.0016)(0.0023)(0.0021)Number of Observations1,4581,4701,474		(0.0020)	(0.0025)	(0.0023)
Number of Counties 385 395 398 D. Minority % $\leq 30\%$ -0.0028**-0.0003-0.0012Pooled-0.0028**-0.0003-0.0012(0.0014)(0.0017)(0.0018)Number of Observations2,9142,9382,943Number of Counties729735737Long Difference-0.0052***-0.0048**-0.0016(0.0016)(0.0023)(0.0021)Number of Observations1,4581,4701,474	Number of Observations	770	790	796
$ \begin{array}{c ccccccccccccccccccccccccccccccccccc$	Number of Counties	385	395	398
D. Minority $70 \le 3070^{\circ}$ Pooled -0.0028^{**} -0.0003 -0.0012 Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 Number of Observations $1,458$ $1,470$ $1,474$	D Minority $% < 30%$			
Folicit -0.0023 -0.0003 -0.0012 (0.0014)(0.0017)(0.0018)Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016)(0.0023)(0.0021)Number of Observations $1,458$ $1,470$ $1,474$	D. Minority $\gamma_0 \ge 30\gamma_0$ Pooled	0 0028**	0.0003	0.0012
Number of Observations $2,914$ $2,938$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016)(0.0023)(0.0021)Number of Observations $1,458$ $1,470$ $1,474$	1 00164	(0.0028)	(0.0003)	(0.0012)
Number of Observations $2,914$ $2,936$ $2,943$ Number of Counties 729 735 737 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016) (0.0023) (0.0021) Number of Observations $1,458$ $1,470$ $1,474$	Number of Observations	(0.0014)	(0.0017)	(0.0010)
Number of Counties 123 135 137 Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016)(0.0023)(0.0021)Number of Observations 1.458 1.470 1.474 Number of Observations 725 727	Number of Counties	2,314 720	2,350 735	2,940 737
Long Difference -0.0052^{***} -0.0048^{**} -0.0016 (0.0016)(0.0023)(0.0021)Number of Observations1,4581,4701,474Number of Counting720725727	Number of Counties	129	755	101
Long Difference -0.0052 -0.0046 -0.0010 (0.0016) (0.0023) (0.0021) Number of Observations 1,458 1,470 1,474	Long Difference	-0 0052***	-0 00/8**	-0.0016
Number of Observations $1,458$ $1,470$ $1,474$	Toug Durcience	(0.0002)	(0.0040)	(0.0010)
$\begin{array}{cccccccccccccccccccccccccccccccccccc$	Number of Observations	1 458	1 470	1 474
Number of Counties 729 735 737	Number of Counties	729	735	737

Table 5: Effect of Section 4 on Key Outcomes, by Subgroup, Southern Counties

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls, and estimated separately for each subgroup. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level. *p< .1, **p< .05, ***p< .01

	Per Capita Income	Med Family Income	Owner Occupied (%)	Med Home Val	Population
A. CBP Sample					
Pooled	-0.0008	-0.0007	0.0023^{***}	-0.0019^{*}	-0.0003
	(0.0005)	(0.0006)	(0.0006)	(0.0010)	(0.0011)
Number of Observations	2,000	2,000	2,518	2,518	2,530
Number of Counties	500	500	506	506	506
Long Difference	-0.0011	-0.0013	0.0019^{**}	-0.0026***	-0.0005
	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0012)
Number of Observations	1,000	$1,\!000$	1,004	$1,\!004$	1,012
Number of Counties	500	500	504	504	506
B. Southern Counties					
Pooled	-0.0014***	-0.0025***	0.0012^{***}	-0.0016**	-0.0021***
	(0.0004)	(0.0004)	(0.0004)	(0.0006)	(0.0007)
Number of Observations	2,268	2,268	3,364	3,362	3,402
Number of Counties	$1,\!134$	1,134	$1,\!125$	$1,\!124$	$1,\!135$
Long Difference	-0.0014^{***}	-0.0025^{***}	0.0012^{***}	-0.0017^{***}	-0.0020***
	(0.0004)	(0.0004)	(0.0003)	(0.0006)	(0.0005)
Number of Observations	2,268	2,268	2,238	2,238	$2,\!270$
Number of Counties	$1,\!134$	$1,\!134$	1,119	$1,\!119$	$1,\!135$

Table 6: Effect of Section 4 on Demographic Characteristics

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level.

	Gen Rev	Direct Exp	# Govt
A. CBP Sample			
Income	-0.0031***	-0.0024*	0.0025^{**}
	(0.0011)	(0.0013)	(0.0012)
Number of Observations	3,472	3,416	3,500
Number of Counties	496	488	500
Housing	-0.0028***	-0.0022*	0.0026**
-	(0.0011)	(0.0012)	(0.0012)
Number of Observations	3,496	3,440	3,524
Number of Counties	502	494	506
Population	-0.0032***	-0.0026**	0.0028^{**}
	(0.0011)	(0.0013)	(0.0012)
Number of Observations	$3,\!514$	$3,\!458$	$3,\!542$
Number of Counties	502	494	506
B. Southern Counties			
Income	-0.0009	-0.0008	0.0012^{*}
	(0.0006)	(0.0007)	(0.0007)
Number of Observations	4,446	4,510	4,527
Number of Counties	$1,\!112$	$1,\!128$	$1,\!133$
Housing	-0.0010	-0.0007	0.0009
	(0.0006)	(0.0007)	(0.0007)
Number of Observations	4,405	4,469	4,486
Number of Counties	1,102	1,118	$1,\!123$
Depulation	0.0010*	0.0000	0.0007
ropulation	-0.0010°	-0.0009	0.0007 (0.0006)
Number of Observations	(0.0000)	(0.0007)	(0.0000)
Number of Counties	4,404 1 114	4,010 1 1 2 0	4,000 1 125
number of Counties	1,114	1,130	1,130

Table 7: Effect of Mediators on Section 4 Impacts

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. Within each panel, the first set of rows reports results from models that control for per capita income and median family income, the second for owner occupied share and median home values, and the third for population. All additional controls are log transformed. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level.

	Total	Not Black/Hispanic	Black/Hispanic
A. CBP Sample			
Long Difference	-0.0059	-0.0040	-0.0107**
	(0.0041)	(0.0047)	(0.0044)
Number of Observations	504	504	504
Number of Counties	252	252	252
B. Southern Counties			
Long Difference	-0.0097***	-0.0172^{***}	-0.0068^{*}
	(0.0023)	(0.0035)	(0.0036)
Number of Observations	1,462	1,462	1,462
Number of Counties	731	731	731

Table 8: Effect of Section 4 on Public School Enrollment

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. *p < .1, **p < .05, ***p < .01

Appendix

A Additional Figures and Tables

Figure A.1: Counties with Subdivisions Covered Under Section 4 of the Voting Rights Act



Source: Department of Justice











Notes: Solid vertical lines represent the passage of amendments to the VRA that expanded Section 4 coverage to additional jurisdictions. Dashed vertical lines represent Supreme Court decisions that clarified the scope of the preclearance requirements.

Source: Underlying data from Department of Justice Voting Rights Section, reproduced in Ball, Krane, and Lauth (1982), pp. 244–245.



Figure A.4: Effect of Section 4 on Revenues, Southern Counties

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.



Figure A.5: Effect of Section 4 on Expenditures, Southern Counties

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.



Figure A.6: Effect of Section 4 on Fragmentation, Southern Counties

(c) Special Districts

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS and include our full set of controls. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.

	Gen Rev	Prop Tax	Non-Prop Tax	Fed Transfers	State Transfers	Charges
A. CBP Sample						
Pooled	-0.0030***	-0.0037***	-0.0082	-0.0037**	-0.0035**	0.0020
	(0.0010)	(0.0013)	(0.0062)	(0.0014)	(0.0014)	(0.0029)
Number of Observations	$3,\!514$	3,332	3,318	$3,\!402$	$3,\!402$	$3,\!290$
Number of Counties	502	476	474	486	486	470
Long Difference	-0.0033**	-0.0038^{*}	-0.0110	-0.0056***	-0.0061^{***}	0.0042
	(0.0013)	(0.0021)	(0.0092)	(0.0018)	(0.0017)	(0.0034)
Number of Observations	$1,\!004$	952	948	972	972	940
Number of Counties	502	476	474	486	486	470
B. Southern Counties						
Pooled	-0.0008	-0.0007	0.0003	-0.0005	-0.0008	-0.0015
	(0.0006)	(0.0008)	(0.0017)	(0.0008)	(0.0008)	(0.0017)
Number of Observations	4,454	4,481	4,429	4,429	4,425	4,450
Number of Counties	$1,\!114$	1,121	$1,\!108$	$1,\!108$	$1,\!107$	$1,\!113$
Long Difference	-0.0018^{***}	-0.0022^{**}	-0.0023	-0.0010	-0.0013	-0.0038^{*}
	(0.0007)	(0.0010)	(0.0024)	(0.0009)	(0.0009)	(0.0022)
Number of Observations	2,228	2,242	2,216	2,216	2,214	2,226
Number of Counties	$1,\!114$	$1,\!121$	$1,\!108$	$1,\!108$	$1,\!107$	$1,\!113$

Table A.1: Effect of Section 4 on Revenues, No Controls

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. *p < .1, **p < .05, ***p < .01

	Direct Exp	Education	Highways	Police	Welfare
A. CBP Sample					
Pooled	-0.0023^{*}	-0.0007	-0.0070***	-0.0067^{***}	-0.0096^{*}
	(0.0013)	(0.0014)	(0.0025)	(0.0024)	(0.0056)
Number of Observations	$3,\!458$	3,318	$3,\!094$	3,416	2,212
Number of Counties	494	474	442	488	316
Long Difference	-0.0020	-0.0038**	-0.0072	-0.0074^{*}	-0.0177
	(0.0017)	(0.0015)	(0.0046)	(0.0039)	(0.0114)
Number of Observations	988	948	884	976	632
Number of Counties	494	474	442	488	316
B. Southern Counties					
Pooled	-0.0006	-0.0001	-0.0018	0.0006	0.0005
	(0.0007)	(0.0008)	(0.0013)	(0.0011)	(0.0031)
Number of Observations	4,518	4,449	4,362	4,481	3,670
Number of Counties	1,130	1,113	1,091	1,121	918
	,	,	,	,	
Long Difference	-0.0034***	-0.0021**	-0.0044**	0.0009	0.0012
	(0.0010)	(0.0009)	(0.0018)	(0.0014)	(0.0047)
Number of Observations	2,260	2,226	2,182	2,242	1,836
Number of Counties	$1,\!130$	$1,\!113$	1,091	$1,\!121$	918

Table A.2: Effect of Section 4 on Expenditures, No Controls

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level.

p < .1, p < .05, p < .01

	# Govt	# Cities	# Spec Dist
A. CBP Sample			
Pooled	0.0031^{**}	0.0015	0.0044^{*}
	(0.0014)	(0.0011)	(0.0024)
Number of Observations	$3,\!542$	$3,\!542$	$3,\!542$
Number of Counties	506	506	506
Long Difference	0.0024	0.0011	0.0041
	(0.0022)	(0.0018)	(0.0038)
Number of Observations	1,012	1,012	1,012
Number of Counties	506	506	506
B. Southern Counties			
Pooled	0.0021^{***}	0.0017^{**}	0.0026**
	(0.0007)	(0.0007)	(0.0013)
Number of Observations	4,535	4,536	4,535
Number of Counties	$1,\!135$	$1,\!135$	$1,\!135$
Long Difference	0.0015^{*}	0.0005	0.0026^{*}
	(0.0008)	(0.0008)	(0.0015)
Number of Observations	$2,\!270$	$2,\!270$	$2,\!270$
Number of Counties	$1,\!135$	$1,\!135$	$1,\!135$

Table A.3: Effect of Section 4 on Government Fragmentation, No Controls

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS. Within each panel, the first set of rows report results estimated using the full panel, while the second set of rows include only data from the period before passage of the VRA and from the last year in the panel. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level.

	Per Capita Income	Med Family Income	Owner Occupied (%)	Med Home Val	Population
A. CBP Sample					
Pooled	-0.0003	0.0001	0.0025^{***}	-0.0016^{*}	-0.0005
	(0.0007)	(0.0009)	(0.0006)	(0.0008)	(0.0011)
Number of Observations	2,000	2,000	2,518	2,518	2,530
Number of Counties	500	500	506	506	506
Long Difference	-0.0007	-0.0005	0.0024^{***}	-0.0028***	-0.0008
	(0.0009)	(0.0011)	(0.0008)	(0.0008)	(0.0013)
Number of Observations	1,000	$1,\!000$	1,004	$1,\!004$	1,012
Number of Counties	500	500	504	504	506
B. Southern Counties					
Pooled	-0.0010**	-0.0015***	0.0010**	-0.0011*	-0.0029***
	(0.0005)	(0.0005)	(0.0004)	(0.0006)	(0.0008)
Number of Observations	2,268	2,268	3,364	3,362	3,402
Number of Counties	$1,\!134$	1,134	$1,\!125$	$1,\!124$	$1,\!135$
Long Difference	-0.0010**	-0.0015^{***}	0.0012^{***}	-0.0015^{**}	-0.0023***
	(0.0005)	(0.0005)	(0.0003)	(0.0006)	(0.0006)
Number of Observations	2,268	2,268	2,238	2,238	$2,\!270$
Number of Counties	$1,\!134$	$1,\!134$	1,119	$1,\!119$	$1,\!135$

Table A.4: Effect of Section 4 on Demographic Characteristics, No Controls

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. All models are estimated using OLS. Within each panel, the first set of rows reports results from models that control for per capita income and median family income, the second for owner occupied share and median home values, and the third for population. All additional controls are log transformed. All outcomes are expressed per capita and log transformed. Total number of governments includes the sum of counties, municipalities, and special districts. Standard errors are clustered at the county level.

	Total	Not Black/Hispanic	Black/Hispanic
A. CBP Sample			
Long Difference	-0.0060	-0.0040	-0.0110**
	(0.0043)	(0.0050)	(0.0043)
Number of Observations	504	504	504
Number of Counties	252	252	252
B. Southern Counties			
Long Difference	-0.0097***	-0.0173^{***}	-0.0065^{*}
	(0.0022)	(0.0035)	(0.0036)
Number of Observations	1,462	$1,\!462$	1,462
Number of Counties	731	731	731

Table A.5:	Effect o	f Section	4 on	Public	School	Enrollment
					10 0 0 0 -	

Notes: The table reports coefficients on interactions between an indicator for Section 4 coverage, a year indicator, and non-white county share in 1960. All models are estimated using OLS. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level.

B Data Appendix

Data on Local Government Finances and Organization

Data on government finances and organization come from the Census of Governments (CoG). The CoG is a census that has been conducted by the Census Bureau every five years since 1957. These data are collected for all individual state and local governments in the United States, including those of states, counties, municipalities, townships, special districts, and independent school districts.

For 1972 onwards, all data for individual governments are digitized and accessible through a Microsoft Access database found in a zipped folder found here (Note that while these data are labeled as containing data from 1967, the 1967 data feature only a sample of governments).

For 1962 and 1967, data on counts of local governments at the county level are digitized and hosted at the Inter-university Consortium for Political and Social Research (ICPSR).

For counts of local governments in 1957 and local public finances from 1957-1967, we handcode data from scans of historical Census of Governments reports, which are no longer available on Census Bureau's Census of Governments page but we will share these reports alongside the cleaned data files. For each year, there are typically reports on public finances aggregated to the county area, as well as for individual local governments, such as those for counties, municipalities, and school districts. However, the reports on individual local governments typically exclude the smallest governments. For example, the reports on municipalities exclude cities with populations less than 5,000 in 1957 and with less than 10,000 in 1962 and 1967. Similarly, the reports on school districts excludes those with enrollment lower than 300 in 1957 or lower than 3,000 in 1962 and 1967.

Because public finance data on many local (sub-county) governments are not available in earlier years, we aggregate data to the county level across all years before conducting any analysis.

Data on 1960 County Characteristics

We present descriptive statistics on county characteristics in 1960. We also include some 1960 county characteristics as baseline controls when estimating effects on all outcomes. These data come from the County Data Books. These data are available on ICPSR.

Data on Other County Characteristics

To examine mechanisms, I test for the effects of Section 4 coverage on other county characteristics.

Data on income also come from Census and can be found in the Historical Income Tables located here.

Data on home values and owner-occupied shares come from the County Data Books from 1952-1994. These data were compiled by Michael Haines are available on ICPSR.

Data on these variables are ultimately derived from the decennial census, and so are measured at the start of each decade from 1950 onwards. When analyzing effects on these outcomes, no adjustment is necessary.

However, we do run into a challenge when using these variables to test for mediators. When we test for mediators, we adapt Equation 1 by adding in controls for potential mediators. The challenge is that our primary outcomes are measured every five years from 1957 onwards (so, in years ending with a "2" or "7"), while mediators come from the decennial census (and so are defined only in years that end in a "0"). To ensure that mediators are defined in the years where our Census of Governments outcomes are measured, we linearly interpolate values for our mediators in between decennial censuses. Results are similar if we instead impute values in the years between decennial censuses based on the last observed value in the decennial census.

Data on School District Demographics

Data on school demographics come from the Office of Civil Rights (OCR) School Survey Data. Since 1968, OCR has biennially surveyed districts and schools on information related to civil rights enforcement, including information on demographics.

The original data were stored on tapes obtained by the UCLA Institute for Social Science Research and encoded in binary formats. Sarah Reber and Ben Denckla decoded these data into ASCII format. We use the version of the data they make available, which can be found here.

From 1968-1972, the surveys covered roughly 8,000 school districts and over 70,000 schools per survey. School districts were sampled so that districts with enrollment

- 300-599 were sampled with probability .25,
- 600-1,199 with probability .5,
- 1,200-2,999 with probability .75, and

• greater than 3,000 students with probability 1.

Furthermore, OCR surveyed other school districts that were of special interest (e.g., ensuring compliance with certain orders), regardless of school district size.

After 1972, the sampling methodology varied some between survey years.

Missing or Unreliable Data

There are very few missing values for observations in our analysis for nearly all outcomes we examine in this paper. Specifically, we are missing just one or two observations for each of our measures of government fragmentation. When observations are missing, we exclude the observation (a county-year) from analysis of that outcome.

For data on public finances, there are a small number of observations that have values equal to zero for some of our outcomes. It is unlikely that these are true zeroes. In some of the Census of Governments published reports, the tables with public finance spending have footnotes documenting that zeroes in cells are cases where spending was less than \$500. For this reason, we impute a small value (\$1) where there are zeroes. The results are insensitive to a different choice for the value of the imputation (e.g., imputing \$250 or \$500).

Very occasionally, we see wild year-to-year swings in a public finance outcome for a given county. We think these observations are unreliable and so seek to exclude them from our analysis. For each county, we first calculate the minimum and maximum value for each outcome. We then calculate the percent change between the maximum and minimum. For a given outcome, if a county's maximum percent change is more than three standard deviations larger than the average percent change, we drop that county from our analysis. Results are qualitatively similar if we keep these observations.

For measures of government fragmentation, we do not make any adjustments based on whether there are zeroes or if there are large changes in the number of governments. First, zeroes appear to be true zeroes in this context. Some counties, for instance have no special districts, at least in the earliest years of data. Second, even large changes in, say, the number of special districts, appear "real" in the sense that the changes are persistent: If there is a large jump in the number of special districts from one census to the next, we tend to continue to see high (or even growing) number of special districts later in the panel as well.

After implementing these data cleaning procedures, we may still have zeroes for counts of local governments (e.g., number of special districts in a county) or in the number of public school students who are members of a specific race or ethnic group. This imposes a challenge for our analysis since we divide outcomes by population and apply a log transformation before analysis. To ensure the transformed outcomes are defined, we add 1 to the raw outcomes before applying the transformations. Results are qualitatively similar if we apply alternative transformations or drop zeroes altogether.