

# Minority Enfranchisement and Local Preferences for Public Goods: Evidence from the Voting Rights Act \*

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## Abstract

Previous research finds the Voting Rights Act of 1965 (VRA) increased the political power of Black communities. I analyze the broader effects of minority enfranchisement on local public finances by exploiting spatial discontinuities in the application of special provisions of the VRA. I 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. The findings suggest that declines in revenues were not mechanical responses to changes in the tax base, but were instead likely due to changing preferences for public goods.

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# 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.”<sup>1</sup> 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) and political officeholding (see Wright, 2013, pp. 188–197). 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 (Aneja and Avenancio-Leon, 2019; Facchini, Knight, and Testa, 2020).

Yet there remains limited evidence on the overall effect of the VRA on local public finances. Furthermore, very little of the literature documents backlash, or the ways in which local communities responded to the VRA that may have undermined or limited its effectiveness. In this paper, I aim to fill these gaps by examining how the VRA ultimately affected revenues, different categories of spending, and the structure of local government, which has important im-

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<sup>1</sup>The 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>.

plications 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 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, I 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 – I 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 are causally identified.

I 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 10 percentage point increase in the 1960 non-white share experienced a statistically significant relative decline of 2.8% in general revenues, including statistically significant relative declines of 4.8% in per capita taxes and of 5.1% in per capita property taxes. I also find corresponding declines in expenditures, including statistically significant declines in police and welfare spending. Importantly, while I do not observe a statistically significant decline in education spending when pooling data across years, I do find evidence of long-run declines. Among covered counties, a 10 percentage point higher non-white share leads to a 4.8% decline in education spending after about 20 years.

The level of public goods and services available to an individual depends not just on county-level spending, but also on the structure of local government. 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). Of particular relevance, 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). In such cases, we might expect higher levels of municipal incorporation in places that prefer to keep taxes low. I examine this pathway by testing the effects of Section 4 coverage on government fragmentation. I find that a 10 percentage point increase in the non-white share leads to a statistically significant relative increases in new cities of about 3.7% and new special districts by 6.9% among covered counties. These findings offers a mechanism by which covered counties could keep taxes and spending low and they further suggest that these communities were indeed motivated to find ways to preserve local control over public resources.

Probing mechanisms further, there are two reasons we might observe relative declines in per capita taxes and revenues. One possibility is that spending changes may be mechanical responses to changes in the tax base (e.g., de-

clines in income would lead to declines in income taxes without any policy changes). I 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. A second possibility is that counties implement policies that either decrease tax rates or shrink the tax base. Since I do not observe tax policy changes directly, I 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 large, long-term decline in education spending, I test for changes in public school enrollment. While estimates are noisy, the findings suggest that public school enrollment – particularly for white students – fell from 1968-1972, suggesting families in these communities may have substituted from public to private schooling in response to Section 4 coverage. Together, the results suggest that the relative declines in taxes and spending were *not* mechanical responses to decreases in the tax base. Rather, the findings support the possibility that the declines were due to policy changes driven by 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). 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) by more fully documenting the effects of the enfranchisement of disadvantaged groups on the finances and structure of local governments in equilibrium.

Relatedly, this project contributes to the research on racial heterogeneity and redistribution. Existing work finds that racial heterogeneity is associ-

ated with lower spending on public goods, including public education (Cutler, Elmendorf, and Zeckhauser, 1993; Poterba, 1997; Goldin and Katz, 1999; Luttmer, 2001; Alesina, Baqir, and Easterly, 1999). This paper builds on this literature by presenting evidence for the causal effects of increasing racial heterogeneity among the electorate on public finances.

I 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 laws (Sahn, 2022), 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, which may help to inform the theory of fiscal federalism.

Finally, this paper contributes to work on the causes of government fragmentation. A prominent literature 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). This paper adds to the literature by documenting the causal effect of increasing racial heterogeneity among the electorate on government fragmentation.

I proceed 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](#) discusses the results and suggests avenues for future research.

## 2 Historical Background: The Voting Rights Act of 1965

Following the end of the Civil War, the Reconstruction government passed the Fifteenth Amendment, securing the right to vote for all men, regardless of their race. However, after the end of Reconstruction, Southern states began to develop 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 Mont-

gomery 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), 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 and which I refer to as “covered counties” throughout this paper. First, under Section 5 of the VRA, covered counties had to “preclear” any changes related to the voting process with, in practice, the Attorney General.<sup>2</sup> 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).

Before proceeding to the empirical strategy, it is important to understand

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<sup>2</sup>In initial drafts, Section 5 provided for review only by the US District Court for DC. Congress recognized that judicial review could be a heavy burden and might be unnecessary in cases where proposed changes were clearly nondiscriminatory. For this reason, Congress allowed for review from the Attorney General as a way to secure judgment more quickly. However, Congress failed to clearly delineate the responsibilities of each in the preclearance process. As a result, jurisdictions have almost exclusively submitted changes to the Attorney General, generally only filing with the US District Court for DC if they object to the Attorney General’s judgment (Roman, 1972).



coverage patterns, which motivate this paper’s research design, as well as the scope and “rollout” of the special provisions, which motivate the outcomes I look at, the mechanisms I investigate, and assist in the interpretation of results.

## 2.1 Coverage

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.<sup>3</sup>

Initially, Attorney General Nicholas Katzenbach determined that 21 states had in place such a “test or device.”<sup>4</sup> 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.<sup>5</sup>

The 1970 amendments to the VRA added 1968 trigger dates, extending coverage to include any places that met these two criteria in 1968. These

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<sup>3</sup>Under 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.”

<sup>4</sup>These 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).

<sup>5</sup>See 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).

amendments resulted in coverage for a handful of additional jurisdictions.<sup>6</sup> The 1975 amendments were broader in 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.<sup>7</sup> Figure 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).<sup>8,9</sup>

## 2.2 Scope and Rollout of Special Provisions

The Johnson administration took immediate advantage of the provision for the appointment of federal examiners with some success. Three days after passage of the VRA, the Attorney General dispatched federal examiners into nine counties in the Deep South to ensure eligible Black adults could register to vote. Over the course of the next two years, examiners entered 60 counties, registering over 150,000 new Black voters (Ball, Krane, and Lauth, 1982, pp. 51–56). During this time, Black voter registration doubled in the Deep South due to the work of civil rights groups, a process undoubtedly aided by the presence of federal examiners.

The efficacy of the preclearance provision, on the other hand, was delayed

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<sup>6</sup>See 36 F.R. 5809 (1971).

<sup>7</sup>See 40 F.R. 43746 (1975), 40 F.R. 49422 (1975), 41 F.R. 784 (1976), and 41 F.R. 34329 (1976).

<sup>8</sup>*Shelby County v. Holder*, 570 U.S. 529 (2013).

<sup>9</sup>Section 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.

for two broad reasons. First, the *implementation* of the provision was slow due to a number of (primarily political) challenges.<sup>10</sup> Second, it was not immediately clear what types of changes needed to be pre-approved and, so, few potentially relevant changes were submitted for preclearance in the years immediately following passage of the VRA. However, the scope of the provision was slowly clarified in the decade following the passage of the VRA through a series of Supreme Court decisions. Ultimately, the Supreme Court determined that redistricting, annexation, polling place changes, precinct changes, changes in reregistration procedures, incorporations, and changes in any election laws were all subject to preclearance (Ball, Krane, and Lauth, 1982, pp. 66–67).<sup>11</sup> Figure 2 shows the number of changes submitted for preclearance to the Department of Justice over time, while Figure 3 further includes breakouts by type of change.

### 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

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<sup>10</sup>These challenges included a lack of capacity among the Department of Justice’s Civil Rights Division that was primarily charged with dealing with preclearance submissions, the Johnson administration’s focus on administering other provisions of the Act, and later obstruction and a lack of will on the part of the Nixon administration. A more full assessment of the implementation challenges can be found in Ball, Krane, and Lauth, 1982.

<sup>11</sup>The Supreme Court ruled that Section 5 was constitutional under the Fifteenth Amendment in *South Carolina v. Katzenbach*, 383 U.S. 301 (1966). It defined the scope of preclearance largely in *Allen v. Board of Elections*, 393 U.S. 544 (1968) and *Perkins v. Matthews*, 400 U.S. 379 (1971).

group (e.g., Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1996; Dixit and Londregan, 1998a; Dixit and Londregan, 1998b). 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 Black voter turnout (Cascio and Washington, 2014; Fresh, 2018; Ang, 2019; Aneja and Avenancio-Leon, 2019), suggesting that it succeeded in its mission to enfranchise Black voters. The theory would predict that a growing Black share of the electorate would lead to increased redistribution 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), 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).

In equilibrium, a more equitable distribution of resources is not the only possible effect of minority enfranchisement. Consider the class of models where parties compete for votes by distributing resources among clearly defined electoral groups. The theory is clear in its prediction that, as minority political power grows, politicians will increase the share of resources going towards minority groups. However, members of the majority group may update their preferences for taxation for at least 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, so public goods may not

look as appealing relative to private goods as before.<sup>12</sup> 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). Again, in such a case, public goods may no longer be as appealing for members of the majority. In either case, then, members of the majority group 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. Ultimately, the effect of minority enfranchisement on the size and distribution of the budget depends on the distribution of preferences across groups, how the size of the minority group changes after enfranchisement, and how minority enfranchisement affects the majority group.

Some literature does provide evidence for such “backlash” to the VRA. Research 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. 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 responded by differentially increasing 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

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<sup>12</sup>In the context of the Jim Crow South, the mechanism by which redistribution might increase in favor of newly enfranchised communities is somewhat unique. In the South, white school boards often allocated a disproportionate share of resources to white schools. With increasing Black political power following civil rights legislation, officials were less likely to siphon away resources from Black communities, thereby increasing the share of resources available to Black students.

for changes in the levels of taxation and spending in places that were subject to additional provisions of the VRA.

## 4 Research Design

### 4.1 Empirical Strategy

The goal of this paper is to examine how Section 4 coverage affected revenues, expenditures, and the structure of local government.<sup>13</sup> 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 time-invariant, unobserved shocks that were correlated with both coverage and with local public policy outcomes.

The coverage rule itself raises concern for the existence of unobserved 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. Estimates of Section 4 coverage would be biased if there were features of counties that either led to or derived from this discriminatory policy and that were correlated with the outcomes of interest.

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 (such as state

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<sup>13</sup>Throughout 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.

legislation or geographically-concentrated economic shocks) and that 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, I follow Aneja and Avenancio-Leon, 2019 who study the labor market impacts of Section 4. They restrict attention to a contiguous border county pair sample, an approach which takes advantage of the spatial discontinuity in Section 4 coverage. This sample comprises 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 (the construction of this sample is described in detail in [subsection 4.2](#)).

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 4.4](#) for further discussion). Second, focusing on a border discontinuity 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 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, if we restrict attention to a pair of neighboring counties, it is plausible that civil rights campaigns affected communities immediately to either side of the border similarly.

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, I 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 public policy outcomes I estimate a triple-differences model in an event study framework:

$$y_{cpt} = \alpha + \sum_{k \neq -1} \theta_k \text{VRA}_{cpt}^k + \sum_{k \neq -1} \beta_k \text{VRA}_{cpt}^k \times \% \text{ Non-white}_c^{1960} + X'_{cpt} \gamma + \mu_c + \tau_{pt} + \epsilon_{cpt} \quad (1)$$

where  $c$  indexes county,  $p$  a county pair, and  $t$  a year.  $\text{VRA}_{cpt}^k \equiv \mathbb{1}\{t = e_c + k\}$  where  $e_c$  is the year county  $c$  is first covered by Section 4 and  $k$  is years since coverage.  $\% \text{ Non-white}_c^{1960}$  is the share of the population of county  $c$  that was not white in 1960.<sup>14</sup>  $X'_{cpt}$  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, and median age – interacted by year. Following Dube, Lester, and Reich, 2016, I also include county fixed effects  $\mu_c$  and pair-specific time effects  $\tau_{pt}$ , 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).

Under the version of the parallel trends assumption imposed above, the

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<sup>14</sup>Unlike previous related studies, I interact treatment with the share non-white instead of the share Black. Among counties in the 1965 cohort, the share non-white and the share Black are nearly identical, so results are very similar regardless of which variable I interact with. However, estimates from regressions featuring later cohorts are sensitive to the choice of interaction. Counties covered later were covered because they discriminated against members of minority language (mostly Spanish-speaking) groups. These counties also had sizable populations of individuals that were not white or Black. To estimate results in these counties, it is important to take into account the potential electoral power of not just Black communities, but these other racial or ethnic minorities as well.



effect of the Section 4 coverage on the relative change in outcomes between counties with higher and lower non-white shares  $\beta_k$  is causally identified.

The event study design is useful because it allows us to inspect for pre-trends and dynamic treatment effects. To estimate the overall effect of Section 4 coverage, I instead estimate

$$y_{cpt} = \alpha + \theta \text{VRA}_{cpt} + \beta \text{VRA}_{cpt} \times \% \text{ Non-white}_c^{1960} + X'_{cpt} \gamma + \mu_c + \tau_{pt} + \epsilon_{cpt} \quad (2)$$

where now VRA is an indicator for whether county  $c$  was covered at time  $t$ .

For inference, I follow Aneja and Avenancio-Leon, 2019 and cluster standard errors at the county level since coverage can theoretically vary within state. This approach has the additional benefit of accounting for the fact that counties can appear in multiple border pairs.

Since treatment is strongly correlated within states, I present robustness checks where I cluster estimates for the main results at the state, instead of the county, level in Online Appendix A. Because there are few states in the sample, I conduct inference implementing the wild bootstrap routine from Cameron, Gelbach, and Miller, 2008.

## 4.2 Sample Construction

To construct the contiguous border pair sample, I identify all pairs of adjacent counties in the United States where one county was ever covered under Section 4 and where the other was never covered under Section 4. Figure 4 displays the full list of counties in the sample by coverage status.

Of 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. I refer to all counties in a pair where the covered county was covered in

1965 (1975) as the 1965 (1975) cohort.<sup>15</sup>

For the main results, I focus on the 1965 cohort. 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 the main focus of this paper is on how local communities responded to Black enfranchisement, it seems appropriate to focus attention on the cohort covered by the original 1965 law. However, I do present estimates for the full sample (that is, featuring all cohorts) in Online Appendix A.

An individual county may appear in the sample multiple times if it is in multiple pairs. Overall, there are 474 counties in the main sample (i.e., the 1965 cohort), representing 227 unique counties. The main sample includes 237 counties that were ever covered, representing 120 unique counties.

### 4.3 Data

Below, I briefly summarize the main data sources used in the analysis. The data sources and related details are discussed more fully in Online Appendix B.

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.

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<sup>15</sup>Section 4 of the 1965 law established a coverage rule to determine which counties would be potentially subject to the special provisions. However, the law left it up to the Attorney General to identify jurisdictions that had a device in place restricting the right to vote and to the Director of the Census to determine jurisdictions where less than 50 percent of the voting age population voted or was eligible to vote. This determination process took time and, consequently, some counties covered under the 1965 law were not determined to be covered by Section 4 until 1966. For similar reasons, some counties covered under the 1975 amendments were not covered until 1976. For simplicity, I nevertheless refer to these as the 1965 and 1975 cohorts.

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. I 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, I use information from various sources to test for potential mechanisms. Data on county population (including breakouts by race and for school-age children) 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.

I 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, I aggregate all data to the county level.

Missing data present one complication to analysis. In general, there are few missing values for any of the variables used in the main analysis. When there are missing values, those observations are dropped from the analysis. For a few of the public finance outcomes (spending on education, health, and welfare), there are occasionally values of zero. When this is the case, we also drop those observations from the analysis. I explain this decision in Online Appendix B and present results using alternative strategies for handling the zero values in Figure A.3.

## 4.4 Descriptive Statistics

[Table 1](#) presents baseline characteristics of counties in the sample by coverage status for different samples.

Panel A displays characteristics for counties first covered in 1965 that border at least one county that was never covered, and those never covered neigh-

bors. This panel shows that covered counties and their neighbors were very similar in 1960. Differences are generally statistically insignificant and, where differences do exist, they are small. Crucially, covered counties and their counterparts are balanced on the share of the population that is non-white or Black. Panel B expands the sample to include all counties in the contiguous border pair sample, regardless of when counties were first covered. Covered and never covered counties are still generally balanced on most covariates, though covered counties tend to have smaller populations and have larger non-white populations.

Table A.1 presents additional justification for the research design. Panel A includes all counties in states with at least one county in the contiguous border pair sample from Panel A in [Table 1](#). Even in this sample, which features counties that are still relatively geographically close, there are large differences between the covered and never covered counties. For instance, roughly one-third of residents in covered counties are racial minorities, compared to just 13% in never covered counties. Finally, Panel B of this table reports characteristics for all contiguous US counties. Differences between covered and never covered counties are now even larger. For example, over one-quarter of individuals in covered counties are racial minorities, compared to less than five percent in never covered counties.

## 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 finance 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 white Americans, changing their preferences for taxation, redistribution, or public goods. In this section, I explore how Section 4 coverage affected levels of revenues, different

categories of expenditures, and the structure of local government.

## 5.1 Revenues

To estimate the effect of Section 4 coverage on public finances, I estimate [Equation 1](#) using OLS. Prior to estimation, all outcome variables are normalized for population and log transformed.

[Figure 5](#) 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. 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, suggesting revenues fall in response to Section 4 coverage.

I estimate [Equation 2](#) to recover a single, pooled estimate of the effect of Section 4 on revenues. [Table 2](#) presents estimates from regressions of outcomes on the interaction between coverage and non-white county share. This table has two pairs of columns. The first pair of columns contains estimates with county and year fixed effects, an approach that mimics a standard difference-in-differences design. The second pair of columns instead include county and pair-year fixed effects, an approach that leverages the border pair design. Each pair of columns contains two additional columns: one for a regression without additional controls and one with additional controls. Additional controls include 1960 county characteristics, including family median income, share with a high school degree, employment-to-population ratio, median age, and median years of education, each interacted with year. Estimates are similar across all four columns, though standard errors tend to be tighter in columns 3 and 4, which include pair-year fixed effects. The preferred estimates can be found in column 4, with county and pair-year fixed effects and controls.

Column 4 of [Table 2](#) shows that relative revenues per capita fell by .0028 log points for every one percentage point increase in the non-white share. Given that the average county in the main sample has a non-white share of roughly 24%, the difference in revenues per capita between a covered county with the

mean non-white share and an all-white covered county is estimated to have fallen by about 6.6% relative to the difference between two such never covered counties. Additionally, total taxes fell by 11.3% at the mean and property taxes by 11.9%. While the coefficients on federal and state transfers are not statistically significant at the .05 level, point estimates suggest a decrease of about 6.8% and 7.0% at the mean, respectively.

For robustness, I repeat the above exercise using the full sample. The graphs in Figure A.1 are very similar to the corresponding graphs for the main sample, except pre-existing trends are closer to zero. Findings from the pooled regression presented in Table A.2 are consistent with the results for the main sample, though point estimates are somewhat attenuated.

Table A.3 reproduces estimates and p-values from column 4, but adds an additional column for p-values derived from clustering at the state level, calculated using the wild bootstrap routine from Cameron, Gelbach, and Miller, 2008. Effects on general revenues are no longer significant, while effects on taxes remain significant at the .1 level and those on property taxes remain significant at the .05 level.

## 5.2 Expenditures

Given that revenues are falling, we would expect to see a concomitant fall in expenditures. I repeat the same estimation strategy to examine how Section 4 coverage affected various expenditure categories.

Table 3 presents results from the pooled regression of Section 4 on log expenditures per capita. There are no statistically significant effects of Section 4 coverage for spending on education or health. However, there are declines in other types of spending. Section 4 coverage leads to a 12.6% decline in police spending and a 31.9% decline in welfare spending at the mean.

While the pooled model shows no effect of Section 4 coverage on education spending, inspection of the event study graphs in Figure 6 shows that education spending did fall over time. In 1987 (22 years after the reform), education spending was 11.4% lower in covered counties with the mean non-white share

compared to an all-white county.

I also look at the effects of Section 4 coverage on spending related to housing and corrections. However, since these data are only available from 1967 onwards, we cannot inspect for pre-existing trends, so I omit these results from the main paper. These results, presented in Figure A.2, show that coverage had large, imprecisely estimated negative effects on housing spending, but no effects on corrections spending.

Recall that when analyzing the effects of coverage on education, health, or welfare expenditures, I drop observations where the outcome measure has a value of zero. One reason values might be zero is if total spending on that outcome was less than \$500 (see Online Appendix B). I re-run results imputing a value of \$500 if the outcome has a recorded value of \$0. Event study graphs are presented in Figure A.3 and pooled estimates can be found in Table A.4. Under this alternative specification, effects on education are similar, effects on welfare are attenuated (and appear to precede coverage), and we now see large, negative declines in health and hospital spending.

Analysis using the full sample yields results similar to the main analysis. Event study graphs in Figure A.4 show that, after pooling the full sample, pre-existing trends tend to be close to zero for all expenditure categories where data are available. The findings again suggest that Section 4 led to long-run declines in education spending, policing, and welfare, though the effects are somewhat attenuated relative to effects on the 1965 cohort alone.

Table A.6 compares p-values from clustering at the county versus the state level. Effects for policing and welfare spending are no longer statistically significant.

### **5.3 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 that might raise taxes (e.g., Miller, 1981; Burns, 1994; Kruse, 2007). Understanding how Section 4 coverage affected government structure, then, is important for understanding why spending fell, how it was kept low, and may have implications for the distribution of resources within counties.

I assess the effects of Section 4 coverage on various measures of fragmentation by estimating Equation 1 using Poisson regression. Visual inspection of Figure 7 reveals that 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. However, following Section 4 coverage, covered counties with greater non-white shares experience an increase in the per capita number of governments overall, municipal governments, and special districts.

Table 4 presents the corresponding estimates from the pooled regression estimated using Equation 1. There is a statistically significant relative increase in the per capita number of municipalities, equal to a 9.2% increase for a covered county with the mean non-white share relative to an all-white county. Furthermore, there is a statistically significant increase in the per capita number of special districts, equal to 17.7% at the mean.<sup>16</sup>

These findings suggest that communities responded to Section 4 coverage by seeking ways to maintain local control over fiscal decisionmaking, including by forming new cities and special districts. This may also help explain why revenues and spending fall in covered counties. These results are consistent with other episodes in the historical record where, in covered counties with larger non-white shares, unincorporated areas became more likely to incorporate, preventing annexation from larger cities that might impose higher taxes (Kruse, 2007, pp. 247–248). And conversely, unincorporated areas in never covered counties create fewer cities, which might be the result of unincor-

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<sup>16</sup>In 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.



porated areas being less likely to incorporate, which could make these areas targets for annexation from larger cities and potentially higher taxes.

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 3](#)).

In sum, the increase in fragmentation provides further evidence that local communities sought ways to maintain local control over fiscal decisionmaking following Section 4 coverage and suggests a mechanism by which they secured relatively lower taxes and spending.

## 6 Mechanisms

So far, I have established that aggregate revenues and expenditures declined in covered counties with greater non-white shares. I 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 and providing one possible mechanism by which covered counties kept taxes low. In this section, I more systematically 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 per capita revenues raised. I examine each explanation in turn.

### 6.1 Are declines just mechanical responses to other trends?

Communities could experience a secular decline in a tax base, 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.

Given the overall importance of property taxes for local revenues (in the main sample, property taxes make up 90% of total taxes and 27% of general revenues on average at baseline) and the large declines in per capita property taxes, I first test for potential changes in the property tax base. [Table 5](#) presents results from pooled regressions of two outcomes on Section 4 coverage – the share of housing units that are owner occupied and the median value of a single-family, owner-occupied home – over the period 1950-1990. Among covered counties, a 10 percentage point increase in the non-white share leads to a 2.4% increase in the owner-occupied share, but there is an equal 2.4% decline in median home values. 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. However, visual inspection of [Figure 8](#) shows that, through 1980, there were large gains in homeownership with modest, statistically insignificant declines in median home values. During this same period, we already observe large declines in per capita property taxes, suggesting it is unlikely that relative declines in per capita property tax collection are due to declines in the property tax base, at least in the first couple of decades following passage of the VRA.

Next, I 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 be used to summarize the economic health of a population or region more generally, which may be correlated with 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 individuals living below the poverty line. For all of these reasons, a change in, say, average incomes, may be correlated with changes in various tax bases or other revenue sources. However, [Table 6](#) finds no evidence that covered counties with greater non-

white shares experience any meaningful changes in per capita income or median family incomes.

Finally, I test for changes in population racial demographics. Given the relative disadvantage of Black Americans across a number of dimensions during this time period, changes in racial demographics may be correlated with changes in tax bases in ways not captured by the various aforementioned economic measures. For example, if white flight is associated with a county becoming more disadvantaged, then we may see a decline in various tax bases even if we do not observe incomes or housing values falling at the mean or median. [Table 7](#) shows that covered counties with larger non-white shares experience small, statistically insignificant population declines. However, covered counties with larger non-white shares experience small *increases* in the white share, suggesting there is no evidence of white flight from the county on net.

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, though these findings are not statistically significant. 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 find no evidence that the economic health of counties is falling in ways that might affect various tax bases. Therefore, the findings suggest that it is unlikely that the reduction in spending is a mechanical response to underlying trends.

## **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). I do not observe local tax schedules, so I 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, I 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 and from the Census on the school-age population. Because not all school districts were sampled in the OCR surveys, I have data to conduct analysis using about three-quarters of the main sample (see Online Appendix B for additional details).

Table 8 shows that from 1968 to 1972, covered counties see declines in public school enrollment, despite no change in the school-age population. Among covered counties, there is a large decline in white enrollment of 15.5% at the mean, though this is not statistically significant at the .05 level. We also see a smaller, statistically insignificant decline in Black enrollment of 5.9% when evaluated at the mean. Given that the school-age population is stable, 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 may imply 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 the reductions in spending may reflect changing preferences of families and communities in response to Section 4 coverage. That is, following Section 4 coverage, individuals in counties with greater non-white shares seem to prefer less taxation, less redistribution, and a greater preference for private relative to public goods.

## 7 Discussion and 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. I find that places with greater non-white shares saw relative declines in revenues and taxation. I also find negative effects on some key expenditure categories, particularly in the long run.

I further show that counties with higher non-white shares responded to Section 4 by increasing the level of local government fragmentation, consistent with local communities being more likely to incorporate, a strategy communities have historically used to block annexation, keep taxes low, and to preserve local control over resources more generally.

Exploration of mechanisms suggests it is unlikely that these changes were simply mechanical responses to declines in various tax bases. The findings rule out meaningful declines in measures that might be associated with declines in tax bases, such as property values or incomes.

Instead, the analysis in this paper provides some evidence that families in covered counties may have responded by shifting their preferences from public to private goods. I find that the share of, particularly white, children in public schools declines in covered counties with greater non-white shares, though these estimates are not statistically significant at the .05 level. If families do substitute toward private goods, they may also prefer less taxation, since they no longer receive the same benefits per dollar taxed. If that is the case, it may explain why we see a decline in revenues and expenditures in covered counties with larger non-white shares.

It is important to note that the results presented here do not imply that the VRA was somehow bad for Black communities. This paper cannot offer a full evaluation of the VRA for three reasons. First, under the assumptions and empirical strategy used in this paper, I can only compare changes between the differences in outcomes in covered counties with higher and lower non-white shares relative to corresponding changes in never covered counties and, even then, can only make comparisons between a small sample of county pairs. Consequently, I do not identify the effects of the VRA more broadly. Second, I document only changes in the level of resources at the county level – I cannot say anything about the distribution of resources between white and Black communities within a county. If the pie becomes distributed more evenly, then it is possible that the VRA increased the level of resources available to Black Americans even if the size of the pie shrunk. However, I cannot assess this possibility here. Finally, the ultimate measure of the VRA lies not in its success in enfranchising Black Americans – and perhaps not even in securing a more equitable distribution of resources for Black communities – but in the ability of Black Americans to, in the words of President Johnson, “transform the vote into...fulfillment.” An assessment of the VRA in producing “fulfillment” for Black Americans is far beyond the scope of this paper. Given the limitations of this paper, future work might examine how Black enfranchisement affects the distribution of resources within counties and, perhaps most importantly, whether political enfranchisement can ultimately improve the economic fortunes of Black Americans, their children, and their communities in the long run.

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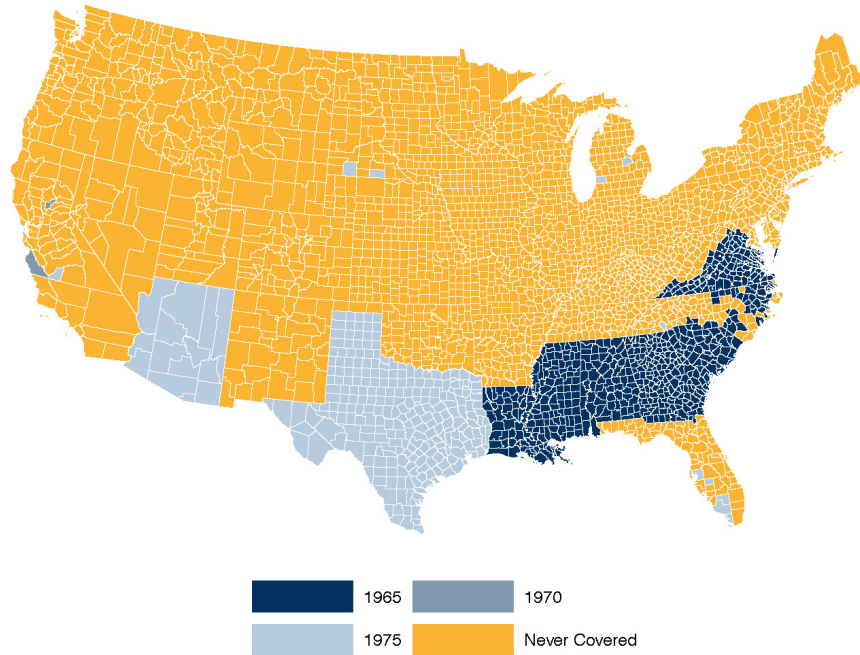


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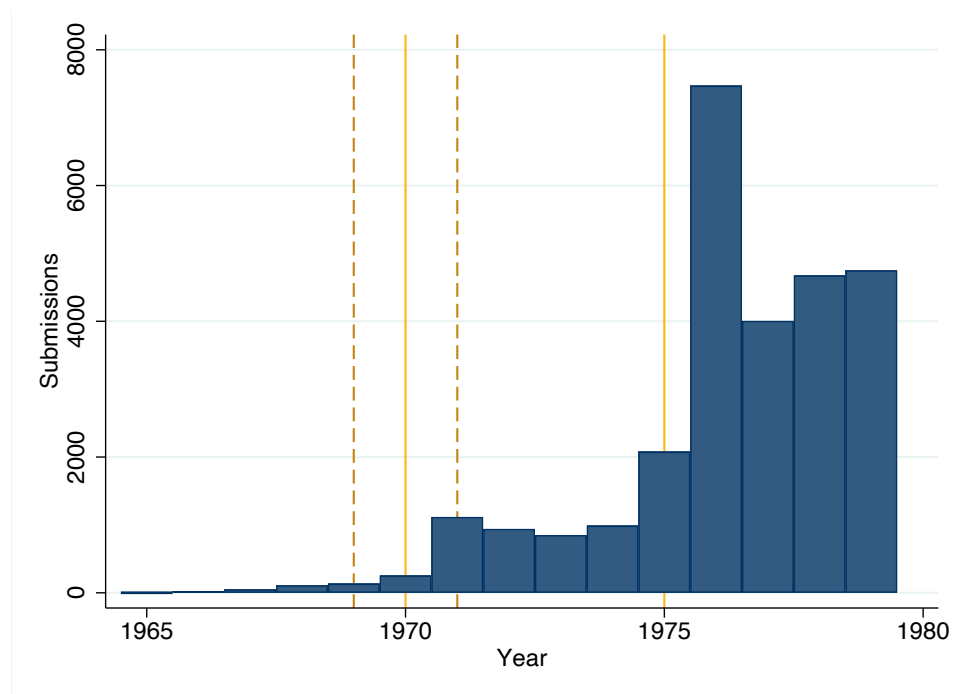
## 8 Figures and Tables

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



Source: Department of Justice

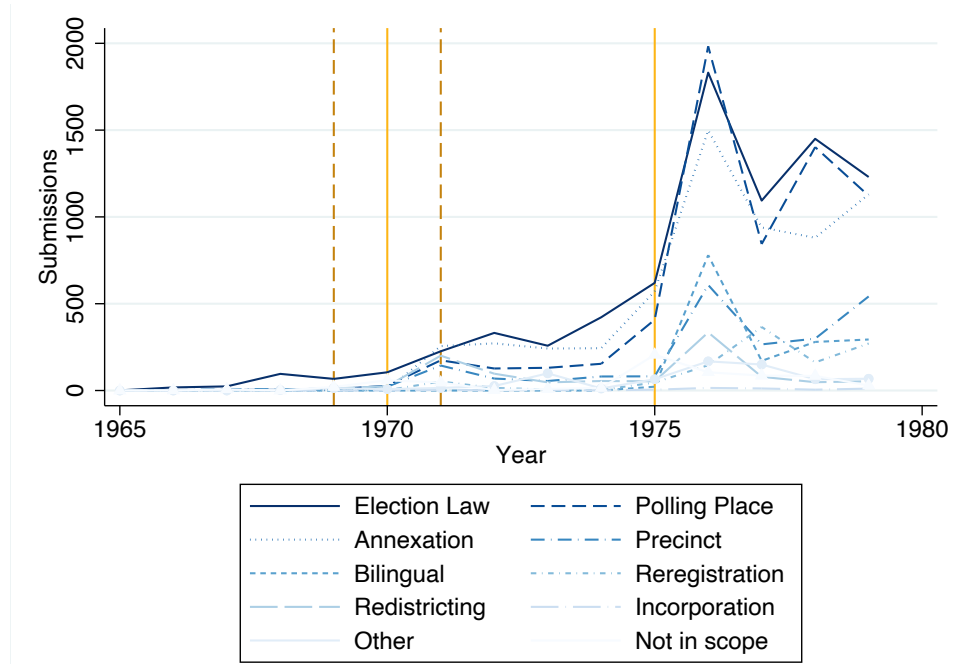
Figure 2: Number of Changes Submitted Under Section 5 (1965-1979)



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 3: Number of Changes Submitted Under Section 5, by Type of Change (1965-1979)



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 4: Counties Covered Under Section 4 and Their Neighbors

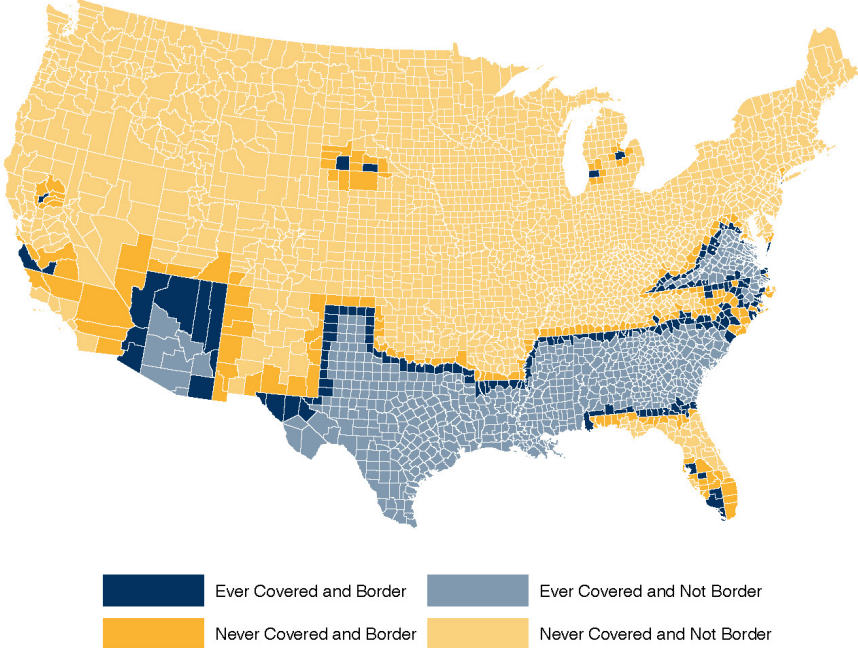
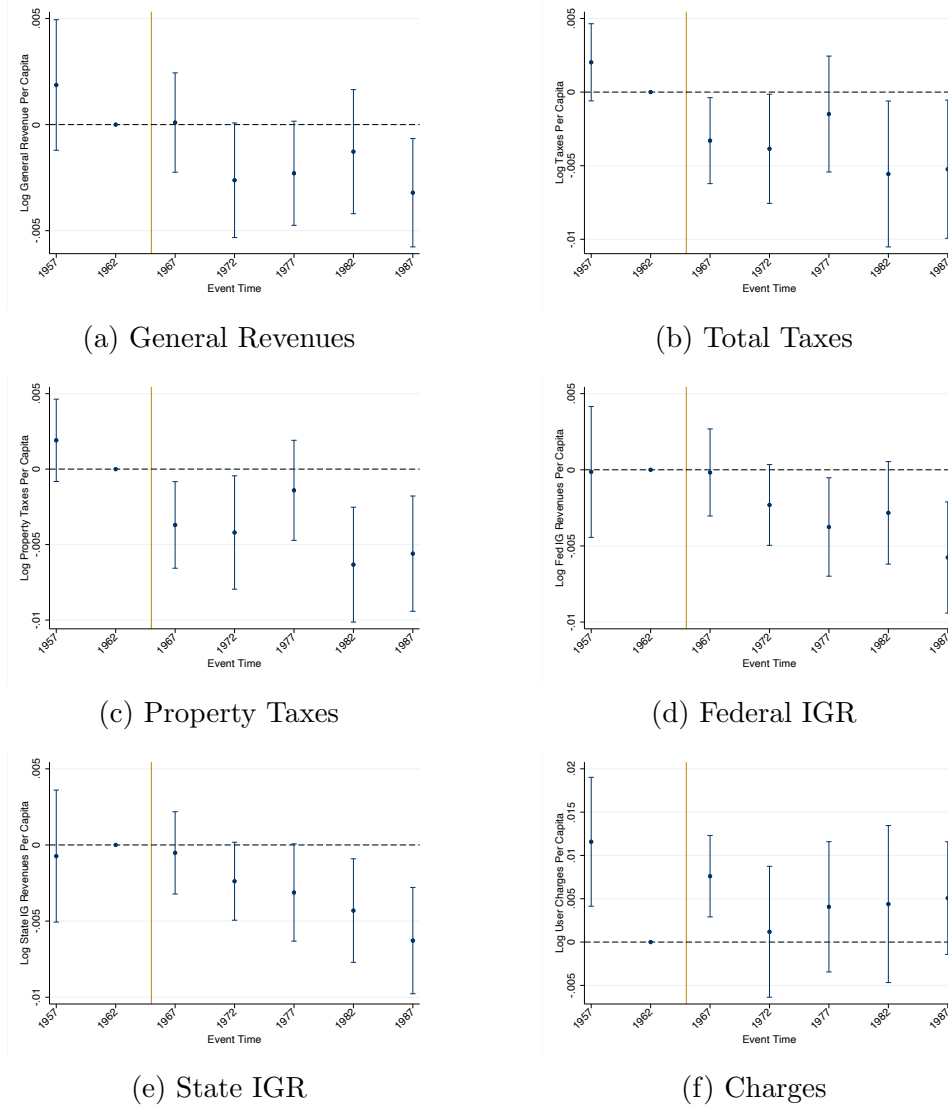
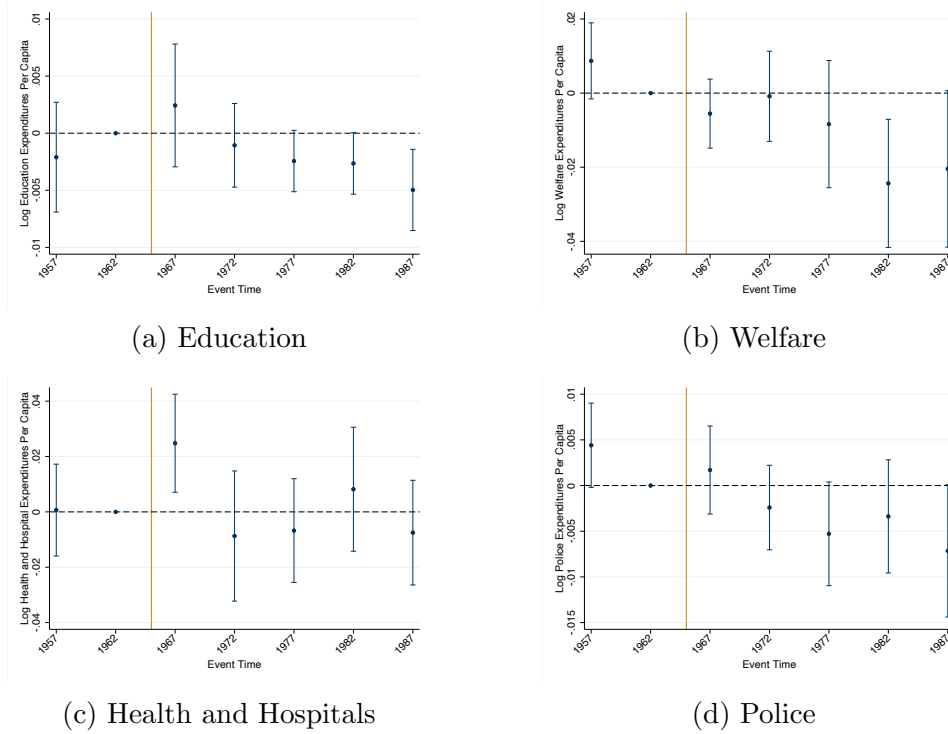


Figure 5: Effect of Section 4 Coverage on Revenues, 1965 Cohort



Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. 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.

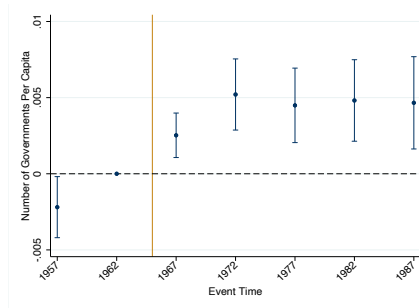
Figure 6: Effect of Section 4 Coverage on Expenditures, No Zeros, 1965 Cohort



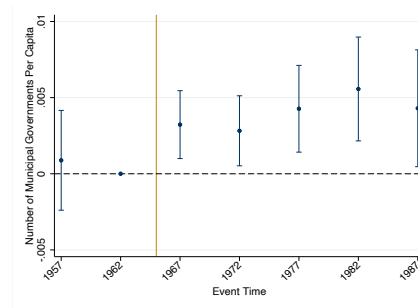
Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. 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.



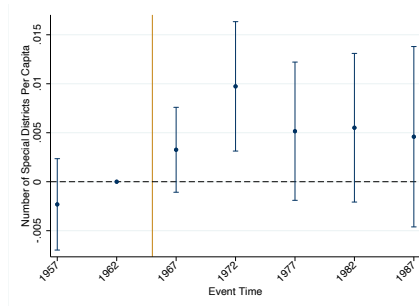
Figure 7: Effect of Section 4 Coverage on Fragmentation, 1965 Cohort



(a) All Local Governments



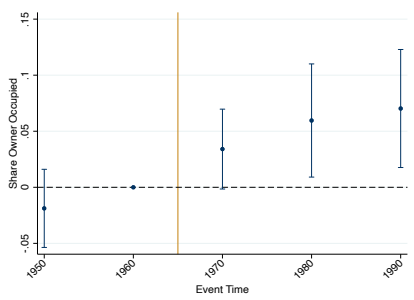
(b) Municipalities



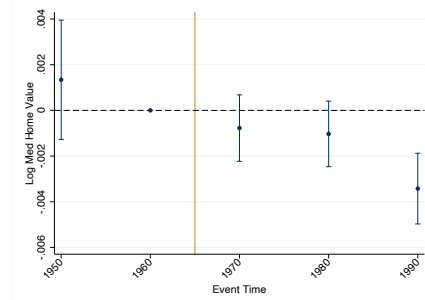
(c) Special Districts

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using Poisson regression and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita. All local governments include counties, municipalities, townships, special districts, and independent school districts. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.

Figure 8: Effect of Section 4 Coverage on Homeownership and Home Values, 1965 Cohort



(a) Owner Occupied



(b) Median Home Value

Notes: Each graph plots coefficients on interactions between an indicator for Section 4 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using Poisson regression and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.

Table 1: 1960 County Characteristics by Section 4 Coverage Status

Characteristic	Ever Covered	Never Covered	Difference	p-value
<i>A. CBP sample, 1965 cohort</i>				
Land Area (Sq Mi)	554	580	-25	0.20
Population	45,789	56,505	-10,716	0.12
Urban (%)	25.69	26.14	-0.45	0.84
Non-white (%)	25.18	23.71	1.47	0.41
Black (%)	24.96	23.49	1.46	0.40
Med Age	25.18	26.56	-1.39	0.00
Foreign Born (%)	0.41	0.44	-0.04	0.52
Spanish Heritage (%)	0.21	0.15	0.06	0.25
HS Graduate (%)	26.32	27.30	-0.98	0.26
Med Yrs Education	8.37	8.68	-0.31	0.00
# Employed	15,282	19,924	-4,642	0.09
Med Family Income	3375.59	3398.07	-22.48	0.84
Employment Rate (%)	32.03	32.04	-0.01	0.98
Observations	237	237		
<i>B. CBP sample</i>				
Land Area (Sq Mi)	1,326	1,340	-14	0.93
Population	46,655	65,332	-18,676	0.01
Urban (%)	31.06	32.94	-1.88	0.34
Non-white (%)	22.51	17.88	4.63	0.00
Black (%)	17.77	16.52	1.25	0.34
Med Age	25.86	27.65	-1.79	0.00
Foreign Born (%)	1.28	1.36	-0.08	0.62
Spanish Heritage (%)	4.22	3.68	0.54	0.45
HS Graduate (%)	29.65	31.64	-1.99	0.01
Med Yrs Education	8.92	9.31	-0.39	0.00
# Employed	15,356	23,103	-7,747	0.00
Med Family Income	3758.51	3868.48	-109.97	0.24
Employment Rate (%)	32.24	32.56	-0.32	0.39
Observations	381	383		

Notes: The table compares mean county characteristics for sample counties ever covered by Section 4 to those never covered by Section 4 using a simple two-sample t-test. Panel A includes counties from the contiguous border pair sample, restricting to just pairs where the covered county was first covered in 1965 or 1966. Panel B expands to include all cohorts.

Table 2: Effect of Section 4 on Revenues, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
General Revenue	-0.0024 (0.0014)	-0.0028 (0.0012)	-0.0026 (0.0010)	-0.0028 (0.0010)
Total Taxes	-0.0043 (0.0019)	-0.0047 (0.0017)	-0.0044 (0.0016)	-0.0049 (0.0015)
Property Taxes	-0.0040 (0.0018)	-0.0045 (0.0015)	-0.0046 (0.0015)	-0.0052 (0.0013)
Federal IGR	-0.0024 (0.0020)	-0.0028 (0.0019)	-0.0029 (0.0016)	-0.0029 (0.0016)
State IGR	-0.0023 (0.0019)	-0.0025 (0.0018)	-0.0026 (0.0015)	-0.0030 (0.0016)
Charges	-0.0013 (0.0038)	-0.0013 (0.0038)	-0.0017 (0.0031)	-0.0013 (0.0030)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	3318	3311	3318	3304

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table 3: Effect of Section 4 on Expenditures, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
Education	0.0000 (0.0019)	-0.0007 (0.0018)	-0.0003 (0.0017)	-0.0007 (0.0017)
Welfare	-0.0152 (0.0050)	-0.0145 (0.0051)	-0.0170 (0.0055)	-0.0157 (0.0054)
Health and Hospitals	0.0009 (0.0104)	0.0011 (0.0097)	0.0030 (0.0085)	0.0014 (0.0076)
Police	-0.0059 (0.0027)	-0.0063 (0.0024)	-0.0057 (0.0025)	-0.0055 (0.0022)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	3318	3311	3318	3304

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table 4: Effect of Section 4 on Fragmentation, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
# Local Govt	0.0048 (0.0021)	0.0049 (0.0017)	0.0050 (0.0012)	0.0054 (0.0012)
# Municipalities	0.0018 (0.0020)	0.0022 (0.0018)	0.0029 (0.0012)	0.0036 (0.0010)
# Special Districts	0.0089 (0.0049)	0.0090 (0.0047)	0.0056 (0.0034)	0.0067 (0.0033)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	3318	3311	3318	3304

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. Model is estimated using Poisson regression and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita. All local governments include counties, municipalities, townships, special districts, and independent school districts. Standard errors are clustered at the county level and are presented in parentheses.

Table 5: Effect of Section 4 on Housing, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
Owner Occupied (%)	0.0028 (0.0012)	0.0026 (0.0010)	0.0027 (0.0007)	0.0024 (0.0006)
Med Home Value	-0.0015 (0.0014)	-0.0016 (0.0015)	-0.0019 (0.0009)	-0.0024 (0.0009)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	2361	2357	2352	2344

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table 6: Effect of Section 4 on Income, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
Med Family Income	0.0000 (0.0013)	-0.0001 (0.0010)	0.0001 (0.0009)	-0.0007 (0.0006)
Per Capita Income	-0.0005 (0.0012)	-0.0008 (0.0009)	-0.0003 (0.0007)	-0.0010 (0.0005)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	1887	1884	1886	1880

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed. Standard errors are clustered at the county level and are presented in parentheses.



Table 7: Effect of Section 4 on Population, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
Population	-0.0007 (0.0016)	-0.0003 (0.0011)	-0.0009 (0.0008)	-0.0011 (0.0008)
White (%)	0.0010 (0.0007)	0.0009 (0.0006)	0.0008 (0.0004)	0.0007 (0.0003)
Black (%)	-0.0046 (0.0032)	-0.0048 (0.0034)	-0.0040 (0.0023)	-0.0039 (0.0023)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	4116	4107	4112	4094

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table 8: Effect of Section 4 on School Enrollment, 1965 Cohort

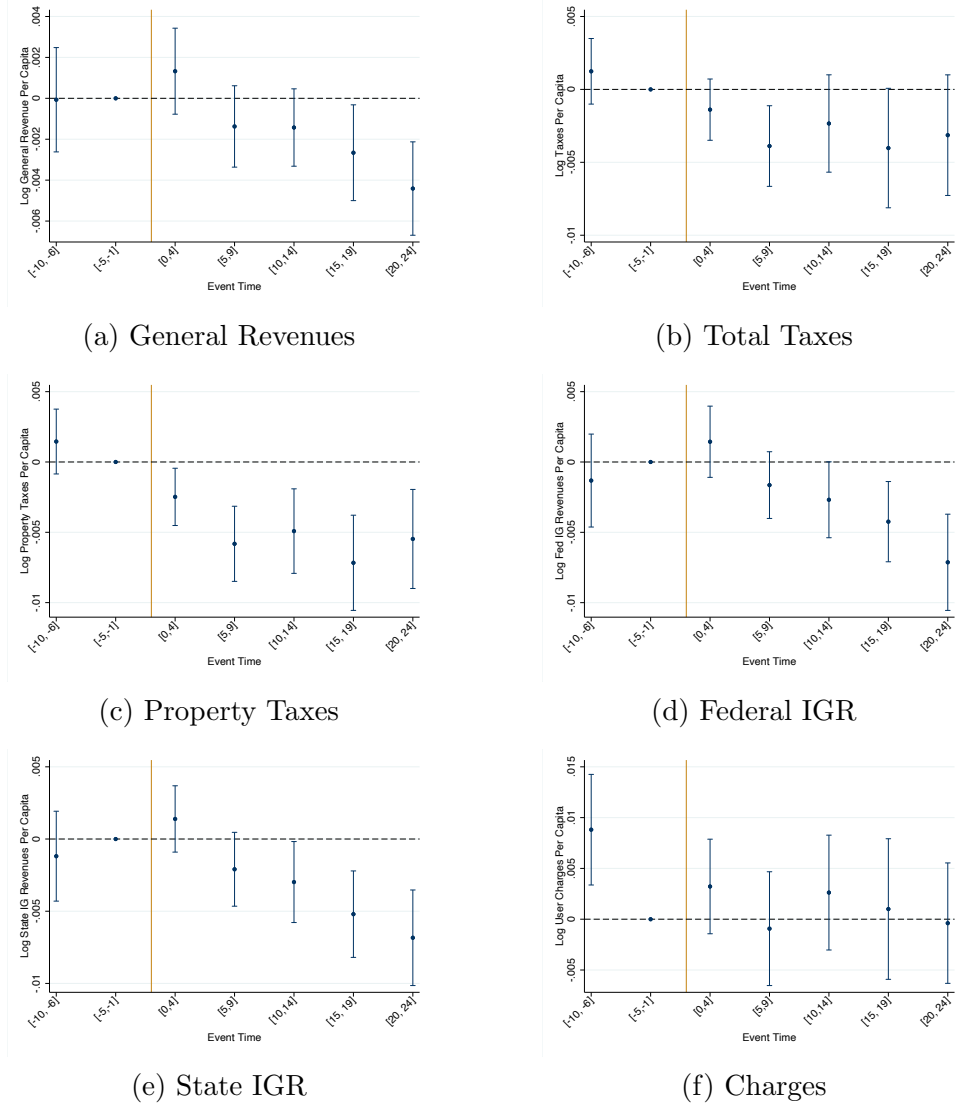
Outcome	(1)	(2)	(3)	(4)
<i>A. School-Age Population</i>				
White	0.0003 (0.0007)	0.0002 (0.0007)	-0.0000 (0.0005)	-0.0000 (0.0005)
Black	0.0003 (0.0009)	0.0003 (0.0008)	0.0002 (0.0007)	0.0002 (0.0006)
<i>B. Pupils</i>				
White	-0.0072 (0.0059)	-0.0067 (0.0048)	-0.0070 (0.0046)	-0.0069 (0.0039)
Black	-0.0028 (0.0022)	-0.0022 (0.0019)	-0.0029 (0.0018)	-0.0025 (0.0019)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	1146	1146	1146	1146

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed. Standard errors are clustered at the county level and are presented in parentheses.

## Online Appendix

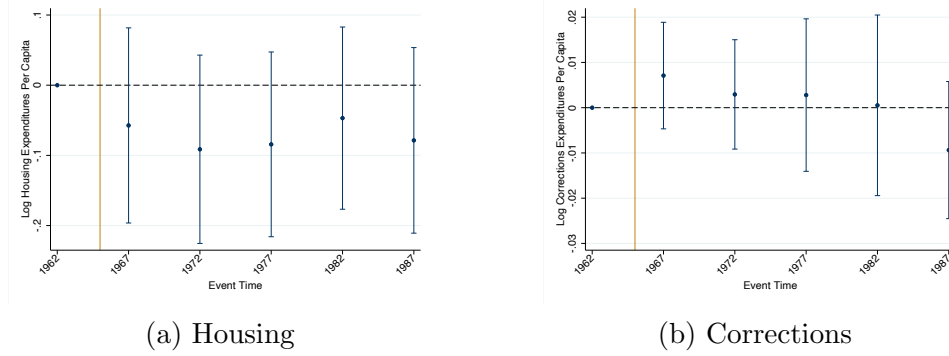
### A Additional Figures and Tables

Figure A.1: Effect of Section 5 on Revenues, All Cohorts



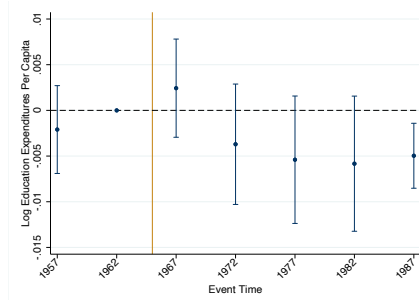
Notes: Each graph plots coefficients on interactions between an indicator for Section 5 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents relative time county was first covered by VRA.

Figure A.2: Effect of Section 5 on Additional Expenditures, No Zeroes, 1965 Cohort

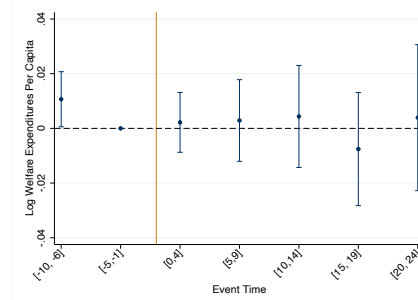


Notes: Each graph plots coefficients on interactions between an indicator for Section 5 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents relative time county was first covered by VRA.

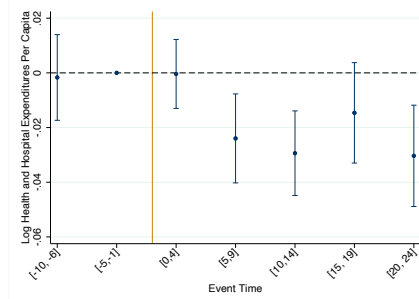
Figure A.3: Effect of Section 5 on Expenditures, Impute \$500 for \$0, 1965 Cohort



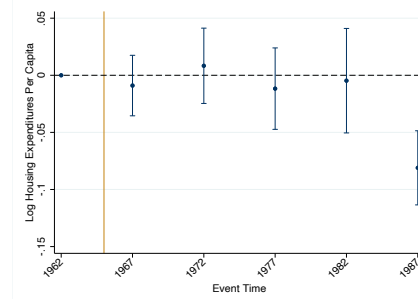
(a) Education



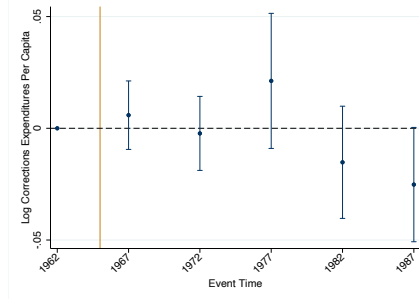
(b) Welfare



(c) Health and Hospitals



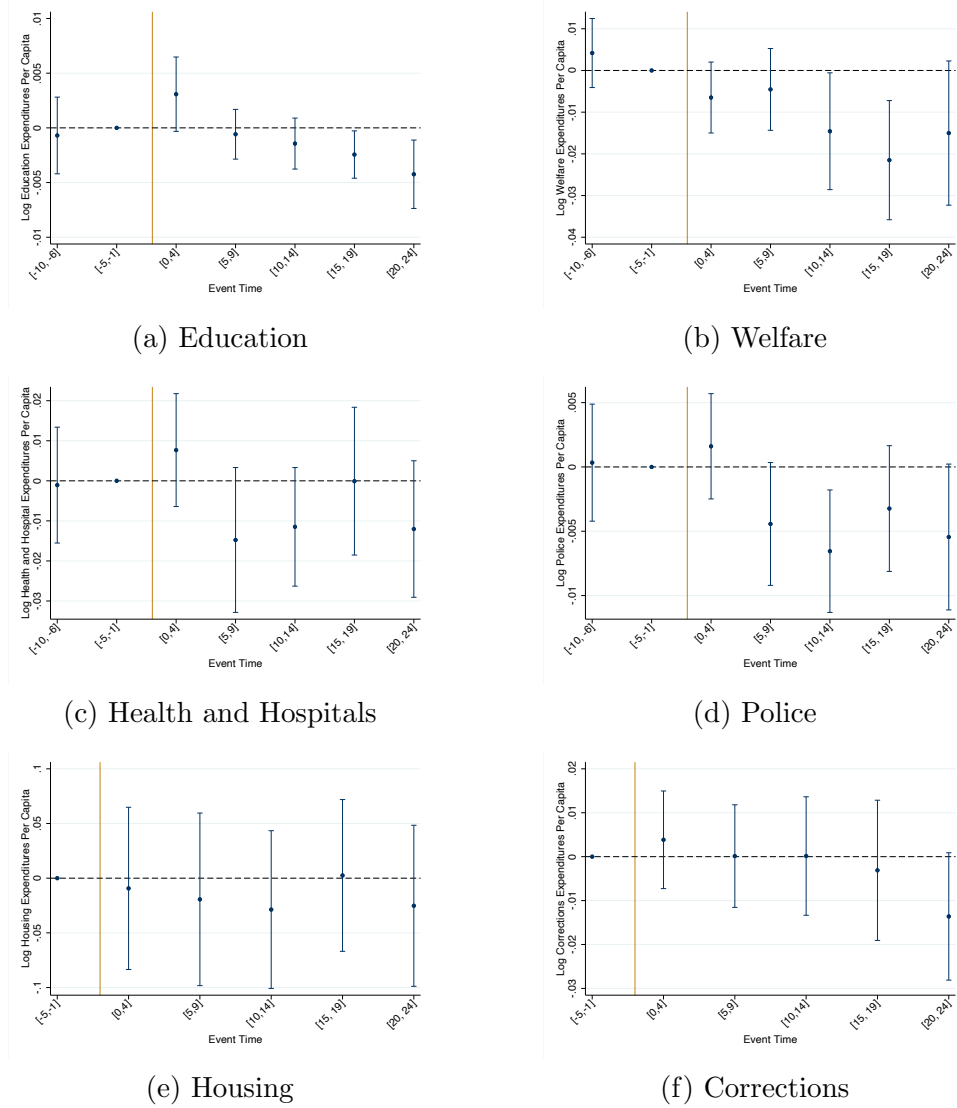
(d) Housing



(e) Corrections

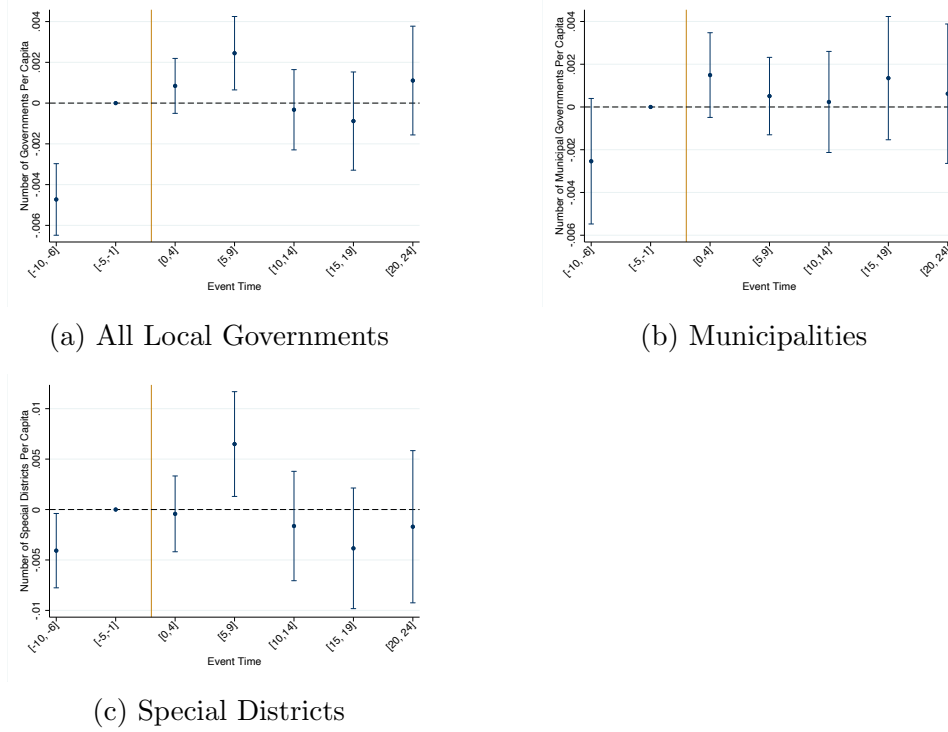
Notes: Each graph plots coefficients on interactions between an indicator for Section 5 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. 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.4: Effect of Section 5 on Expenditures, No Zeroes, All Cohorts



Notes: Each graph plots coefficients on interactions between an indicator for Section 5 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level. Vertical line represents relative time county was first covered by VRA.

Figure A.5: Effect of Section 5 on Fragmentation, All Cohorts



Notes: Each graph plots coefficients on interactions between an indicator for Section 5 coverage, an event time indicator, and non-white county share in 1960. Model is estimated using Poisson regression and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita. All local governments include counties, municipalities, townships, special districts, and independent school districts. Standard errors are clustered at the county level. Vertical line represents relative time county was first covered by VRA.



Table A.1: 1960 County Characteristics by Section 4 Coverage Status

Characteristic	Ever Covered	Never Covered	Difference	p-value
<i>A. Contiguous states, 1965 cohort</i>				
Land Area (Sq Mi)	498	480	18	0.25
Population	35,770	39,505	-3,736	0.40
Urban (%)	28.70	23.61	5.10	0.00
Non-white (%)	33.06	12.63	20.43	0.00
Black (%)	32.99	12.55	20.45	0.00
Med Age	25.27	28.46	-3.19	0.00
Foreign Born (%)	0.37	0.55	-0.17	0.00
Spanish Heritage (%)	0.19	0.13	0.07	0.06
HS Graduate (%)	26.16	25.02	1.13	0.04
Med Yrs Education	8.38	8.65	-0.27	0.00
# Employed	11,821	13,452	-1,631	0.32
Med Family Income	3316.40	3303.13	13.27	0.85
Employment Rate (%)	32.12	31.19	0.94	0.00
Observations	576	483		
<i>B. US counties</i>				
Land Area (Sq Mi)	794	1,021	-228	0.00
Population	37,954	61,687	-23,733	0.00
Urban (%)	32.61	31.87	0.73	0.52
Non-white (%)	25.79	4.88	20.90	0.00
Black (%)	25.27	3.92	21.35	0.00
Med Age	26.74	30.06	-3.31	0.00
Foreign Born (%)	1.04	2.35	-1.31	0.00
Spanish Heritage (%)	5.95	1.70	4.25	0.00
HS Graduate (%)	28.55	36.99	-8.44	0.00
Med Yrs Education	8.76	9.98	-1.21	0.00
# Employed	12,715	22,400	-9,685	0.00
Med Family Income	3542.44	4427.24	-884.80	0.00
Employment Rate (%)	32.65	33.97	-1.32	0.00
Observations	857	2240		

Notes: The table compares mean county characteristics for sample counties ever covered by Section 4 to those never covered by Section 4 using a simple two-sample t-test. This table includes only places in the contiguous United States and does not include duplicate counties. Panel A includes all counties that were covered in 1965 or 1966, as well as all counties in states that neighbored any state with covered counties. Panel B includes all counties in the United States.

Table A.2: Effect of Section 4 on Revenues, All Cohorts

Outcome	(1)	(2)	(3)	(4)
General Revenue	0.0011 (0.0019)	0.0004 (0.0020)	-0.0015 (0.0010)	-0.0013 (0.0009)
Total Taxes	-0.0041 (0.0024)	-0.0050 (0.0015)	-0.0031 (0.0013)	-0.0036 (0.0012)
Property Taxes	-0.0059 (0.0026)	-0.0062 (0.0015)	-0.0058 (0.0013)	-0.0058 (0.0011)
Federal IGR	0.0023 (0.0027)	0.0017 (0.0026)	-0.0016 (0.0013)	-0.0016 (0.0013)
State IGR	0.0016 (0.0018)	0.0006 (0.0018)	-0.0014 (0.0013)	-0.0019 (0.0013)
Charges	-0.0009 (0.0026)	-0.0019 (0.0024)	-0.0037 (0.0026)	-0.0033 (0.0024)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	5334	5313	5334	5292

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table A.3: Comparison of Clustering on County or State, Revenues, 1965 Cohort

Outcome	Estimate	p-values	
		County	State
General Revenue	-0.0028	0.0075	0.2750
Taxes	-0.0049	0.0009	0.0550
Property Taxes	-0.0052	0.0001	0.0350
Fed IG	-0.0029	0.0727	0.4775
State IG	-0.0030	0.0615	0.4800

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed and normalized for population. The “county” column presents p-values from models estimated clustering standard errors at the county level. The “state” column presents p-values from models estimated clustering standard errors at the state level and generated using the wild bootstrap method from Cameron, Gelbach, and Miller, [2008](#).

Table A.4: Effect of Section 4 on Expenditures, Imputing \$500 for \$0, 1965 Cohort

Outcome	(1)	(2)	(3)	(4)
Education	-0.0018 (0.0027)	-0.0024 (0.0026)	-0.0021 (0.0028)	-0.0024 (0.0027)
Welfare	-0.0125 (0.0062)	-0.0123 (0.0062)	-0.0136 (0.0075)	-0.0122 (0.0073)
Health and Hospitals	-0.0074 (0.0105)	-0.0069 (0.0096)	-0.0073 (0.0080)	-0.0078 (0.0074)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	3318	3311	3318	3304

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table A.5: Effect of Section 4 on Expenditures, All Cohorts

Outcome	(1)	(2)	(3)	(4)
Education	0.0022 (0.0021)	0.0015 (0.0023)	-0.0006 (0.0013)	-0.0003 (0.0013)
Welfare	-0.0074 (0.0043)	-0.0059 (0.0040)	-0.0144 (0.0043)	-0.0136 (0.0042)
Health and Hospitals	0.0007 (0.0082)	-0.0047 (0.0076)	-0.0033 (0.0075)	-0.0051 (0.0066)
Police	-0.0013 (0.0015)	-0.0040 (0.0017)	-0.0027 (0.0015)	-0.0037 (0.0016)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	5323	5310	5312	5286

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita and log transformed. Standard errors are clustered at the county level and are presented in parentheses.

Table A.6: Comparison of Clustering on County or State, Expenditures, 1965 Cohort

Outcome	Estimate	p-values	
		County	State
Education	-0.0007	0.6886	0.8075
Police	-0.0055	0.0111	0.1775
Health and Hospital	0.0014	0.8522	0.9000
Welfare	-0.0157	0.0038	0.1775

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. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed and normalized for population. The “county” column presents p-values from models estimated clustering standard errors at the county level. The “state” column presents p-values from models estimated clustering standard errors at the state level and generated using the wild bootstrap method from Cameron, Gelbach, and Miller, 2008.

Table A.7: Effect of Section 4 on Fragmentation, All Cohorts

Outcome	(1)	(2)	(3)	(4)
# Local Govt	-0.0012 (0.0018)	-0.0021 (0.0018)	0.0029 (0.0010)	0.0030 (0.0008)
# Municipalities	-0.0005 (0.0017)	-0.0001 (0.0016)	0.0011 (0.0011)	0.0020 (0.0009)
# Special Districts	0.0003 (0.0033)	-0.0008 (0.0033)	0.0034 (0.0025)	0.0027 (0.0023)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Pair-Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
N	5334	5313	5334	5292

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. Model is estimated using Poisson regression and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are expressed per capita. All local governments include counties, municipalities, townships, special districts, and independent school districts. Standard errors are clustered at the county level and are presented in parentheses.

## 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](#) by the Inter-university Consortium for Political and Social Research (ICPSR).

For counts of local governments in 1957 and local public finances from 1957-1967, I handcode data from scans of historical Census of Governments reports, which are no longer available on Census Bureau's Census of Governments page but I 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. 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.

The outcomes reported in the main figures and tables are major revenue sources and expenditure categories for which there are data from 1957 onwards. There are additionally data available on corrections and housing expenditures, but these data are only available from 1962 onwards and therefore we cannot



examine pre-existing trends. Nevertheless, I report results for these spending categories in [Online Appendix A](#).

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

## **Data on 1960 County Characteristics**

I present descriptive statistics on county characteristics in 1960. I 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 [hosted](#) by ICPSR.

## **Data on Other County Characteristics**

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

Data on population by race comes from the Census Bureau. Data for 1950 and 1960 come from the decennial census and can be found [here](#). Data for the 1970s come from the County Intercensal Tables and can be found [here](#).

Data on income also come from Census and can be found in the Historical Income Tables located [here](#).

Finally, data on home values and owner-occupied shares come from the County Data Books from 1952-1994. These data were compiled by Michael Haines and are [hosted](#) by the ICPSR.

## **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. I 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. In total, there is data associated with about three-quarters of the counties in this paper's main sample.

I focus on the three surveys conducted from 1968-1972. Thereafter, the sample methodology changes and, consequently, the sample is no longer consistent over time.

## Missing Data

There are very few missing values for observations in our analysis for nearly all outcomes I examine in this paper.

Rather, for data on public finances and government organization (counts of local governments), it is somewhat more common that some observations have values equal to zero. I handle zeroes differently when analyzing public finances and government organization. I discuss each of these decisions in turn.

For most public finance outcomes, zeroes are very rare. For example, there are no zeroes for the revenue measures analyzed in this paper or for expenditures on police. However, there are zeroes for other expenditure outcomes,

including three presented in the main paper (spending on education, health, and welfare).

For the main results presented in the paper, I drop observations where the outcome variable has a value of \$0. However, I test alternative strategies for handling zeroes in [Online Appendix A](#). I discuss this decision below.

In 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. In the main analysis, I impute \$500 wherever there are zeroes. However, in [Figure A.3](#) and [Table A.4](#), I present results from regressions where I instead drop observations with zeroes. Results on education spending are robust to the choice of how to deal with zeroes, but results on welfare and health spending are sensitive to this choice.

For the main results, why do I drop zeroes instead of imputing some small value, given that the historical reports suggest that zeroes are values less than \$500?

For education spending and spending on health and hospitals in all years except 1962-1967, there are only a small number of observations with zero values for these outcomes. If we inspect counties that ever have zero values for these outcomes, these zeroes appear to be mistakes or, effectively, missing values. For example, a county might have a zero value for education spending, sandwiched between years where they are spending millions on education. It is unlikely the county decided to defund education for one year.

Using the same approach (manually checking time series for counties with zero values in outcomes in some years), for welfare, housing, and corrections, it is likely the truth is somewhere in between – some zeroes are “true” zeroes (that is, they spend less than some small amount on these categories), while others appear to effectively be missing data.

Because I cannot always confidently distinguish between true zeroes and missing zeroes, I run the analysis in two ways, (1) by dropping observations with zeroes and (2) by instead imputing \$500 where outcomes are set to \$0 in the data. Since education spending makes up the majority of local spending, and because zeroes appear to be true zeroes for this outcome, I choose to drop

zeroes in the main specification reported in the paper. However, results using the imputation method can be found in [Online Appendix A](#).

For analysis of government organization (i.e., counts of local governments), I keep zeroes in the analysis for several reasons. First, zeroes are more sensible in this context. E.g., it is possible that there are no special districts or independent school districts within a county boundary. Second, it's possible to indirectly verify whether these are "true" zeroes. In general, if a county has no, say, independent school districts in one year, it tends not to have any across all years.