The Effect of Minority Enfranchisement on Preferences for Public Goods: Evidence from the Voting Rights Act of 1965 *

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Last revised: July 24, 2022

Abstract

Previous research finds the Voting Rights Act of 1965 (VRA) was instrumental in increasing Black political enfranchisement and, consequently, helping Black communities secure more desirable policy outcomes. In this paper, I analyze the broader effects minority enfranchisement on local public finances by exploiting spatial discontinuities in the application of special provisions of the VRA, generated by a coverage formula defined in Section 4 of the Act. In response to Section 4 coverage, counties with larger non-white population shares exhibited relative declines in revenues and expenditures. The findings suggest these declines were not mechanical responses to secular changes in the tax base, but were instead likely generated by changing preferences for public goods. Further exploration shows that counties responded to coverage by increasing government fragmentation, providing one potential mechanism by which communities may have kept taxes low. Responses to the VRA illustrate how local communities may respond to and potentially undermine efforts to address racial inequality.

*I would like to thank Abhay Aneja, Hilary Hoynes, Rucker Johnson, and Jesse Rothstein for helpful discussion and invaluable feedback. This paper also benefitted from discussion with seminar participants at UC Berkeley. The Opportunity Lab and the Law, Economics, and Politics Center at UC Berkeley provided generous financial assistance. Any errors are my own.

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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."[1] It was his 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 and political officeholding. 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 and improved provision of public services.[2]

However, 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 local communities' preferences for taxation, redistribution, and public goods. One challenge to answering this question 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 counties – targeted based on a coverage formula defined in Section 4 of the VRA (hereafter, I refer to these jurisdictions as "covered counties" or "Section 4 counties"). 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 for the appointment of federal officials in covered jurisdictions to ensure that voter registration and voting were carried out

[1] 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.

[2] **wright˙sharing˙2013**.

without discrimination. However, jurisdictions covered under Section 4 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˙effect˙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 within 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 employ a triple-differences design to estimate the effect of Section 4 coverage on a range of public finance outcomes. 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 Section 4 counties, those with larger non-white population shares saw an overall relative *decline* in revenues. For a 10 percentage point increase in the 1960 non-white share, covered counties experienced a relative decline of 2.8% in general revenues, including a 4.9% decline in per capita taxes and a 5.2% decline in per capita property taxes. The effects of Section 4 coverage on different categories of expenditures are noisier; however, point estimates are typically negative, consistent with revenue decreases. Most importantly, I find that a 10 percentage point increase in the non-white share is associated with a 4.4% decline in per capita education spending, which would account for most of the observed decline in revenues, though this estimate is not statistically significant.

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., declines 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 (though statistically insignificant) decline in education spending, I test for changes in public school enrollment. For a 10 percentage point increase in the non-white share, the share of school-age children enrolled in public school declined by a statistically insignificant 3.7% in covered counties from 1968-1972, suggest-

ing 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 point to the possibility that the declines were due to policy changes, driven by changing preferences for public goods.

Motivated by these findings, I look for potential policy changes that might explain the relative declines in taxation and expenditures. Unfortunately, I do not observe tax rates at the county level over time, the most direct explanation. Instead, I turn to alternative explanations. The historical record is replete with instances where unincorporated areas in the South incorporated to block annexation from larger neighboring cities in order to keep taxes low and preserve local control over resources. I find that counties with greater non-white shares responded to Section 4 coverage by increasing government fragmentation. A 10 percentage point increase in the Black share is associated with a statistically significant relative increase in new cities of about 3.3% and new special districts by a statistically insignificant 5.8%. These findings suggest a mechanism by which covered counties kept taxes and spending low and, furthermore, reveal that these communities were indeed motivated to find ways to preserve local control over public resources.

This paper makes several contributions to the literature. Foremost, this paper adds nuance to the literature on redistributive politics. Positive political theory on redistribution suggests that enfranchising disadvantaged groups will result in a more equitable distribution of resources (**cox˙electoral˙1986**; **lindbeck˙balanced-budget˙1987**; **dixit˙determinants˙1996**). Existing empirical research has largely focused on how the VRA helped Black communities secure more equitable public policy outcomes (**husted˙effect˙1997**; **cascio˙valuing˙2014**; **aneja˙effect˙2019**; **facchini˙franchise˙2020**). This paper adds to recent empirical work that documents political backlash to the VRA (**kuziemko˙why˙2018**; **fresh˙effect˙2018**; **ang˙40-year-old˙2019**; **eubank˙enfranchisement˙2022**) by more fully documenting the effects of the enfranchisement of disadvantaged groups on the finances and structure of local governments in equilibrium.

This paper also contributes 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˙essay˙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˙was˙2010**), local governments adjusted spending in ways that ultimately made destination communities worse for upward mobility (**derenoncourt˙can˙2022**), and communities responded by implementing more exclusionary zoning laws (**sahn˙racial˙2021**), which are often credited with upholding racial and income segregation. Similarly, previous research shows that attempts to reform school finance to make it more equitable may have led local communities to adopt caps on property taxes (**fischel˙did˙1989**) and more stringent land-use policy (**krimmel˙reclaiming˙2022**). 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. Is this paragraph too much of a stretch?

This work also contributes to two additional strands of the literature. First, this paper speaks to research on racial heterogeneity and redistribution, which finds that racial heterogeneity is associated with lower spending on public goods, including public education (**cutler˙demographic˙1993**; **poterba˙demographic˙1997**; **goldin˙human˙1999**; **luttmer˙group˙2001**; **alesina˙public˙1999**). Second, this paper contributes to work on the causes of government fragmentation, which finds that the number of political jurisdictions is related to trade-offs between size and heterogeneity in income or race (**alesina˙number˙1997**; **bolton˙breakup˙1997**; **alesina˙political˙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˙formation˙1994**). This paper extends both of these literatures by presenting evidence on the *causal* effects of increasing racial heterogeneity among the electorate on public finances and government fragmentation, respectively.

I proceed as follows. Section 2 provides historical background on the VRA. **??** details the theoretical motivation for the paper. **??** introduces the research design and presents descriptive statistics. **??** covers the main results on the effects of Section 4 coverage on revenues and expenditures. **??** offers suggestive evidence on why and how local communities decreased revenues and spending following Section 4 coverage. **??** briefly 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˙civil˙2007**). After nearly a decade of fighting for voting rights, civil rights activists organized the now-infamous marches from Selma to Montgomery for voting rights in March 1965. Following the horror of Bloody Sunday, where Alabama state troopers brutally assaulted activists crossing the Edmund Pettus Bridge, President Lyndon Johnson called on Congress to pass strong voting rights legislation. By the end of the summer, President Johnson had signed into law the Voting Rights Act of 1965 (VRA), perhaps "the most radical piece of civil rights legislation since Reconstruction" (**tribe˙american˙1978**).

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 special provisions that applied only to a subset of jurisdictions (largely in the South), 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.[3] 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

[3]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˙section˙1972**).

"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 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 (for this reason, I sometimes refer to covered counties as "Section 4 counties" throughout this paper). 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.[4]

Initially, Attorney General Nicholas Katzenbach determined that 21 states had in place such a "test or device."[5] 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.[6]

The 1970 amendments to the VRA added 1968 trigger dates, extending coverage to include any places that met these two criteria in 1968. These amendments resulted in coverage for a handful of additional jurisdictions.[7] The 1975 amendments were broader in scope: they added 1972 trigger dates and, furthermore, expanded coverage to places where

[4]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."

[5]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).

[6]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).

[7]See 36 F.R. 5809 (1971).

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.[8] **??** 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)*.[9,10]

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˙compromised˙1982**). 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 for two broad reasons. First, the *implementation* of the provision was slow due to a number of (primarily political) challenges.[11] 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

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

[9] Shelby County v. Holder, 570 U.S. 529 (2013).

[10] 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, o nly 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.

[11] 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˙compromised˙1982**.

(**ball˙compromised˙1982**).[12] **??** shows the number of changes submitted for preclearance to the Department of Justice over time, while **??** further includes breakouts by type of change.

3 Theory: Voter Enfranchisement and Preferences for Redistribution

In the study of the positive political theory of redistribution, 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 (**romer˙individual˙1975**; **roberts˙voting˙1977**; **meltzer˙rational˙1981**). In the second, there are clearly defined electoral groups who prefer one political party but that may be induced to shift their vote in exchange for redistribution to their group (**cox˙electoral˙1986**; **lindbeck˙balanced-budget˙1987**; **dixit˙determinants˙1996**; **dixit˙fiscal˙1998**; **dixit˙ideology˙1998**). In either set of models, enfranchising a disadvantaged group would predict weakly greater redistribution toward members of that group.

Recent empirical work shows that the VRA had large, positive effects on Black voter turnout (**cascio˙valuing˙2014**; **fresh˙effect˙2018**; **ang˙40-year-old˙2019**; **aneja˙effect˙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˙effect˙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˙valuing˙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˙effect˙2019**), Black Americans were arrested at lower rates in places with elected law enforcement officials (**facchini˙franchise˙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 (**wright˙sharing˙2013**).

[12] 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).

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. 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˙sum˙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 (**lindbeck˙balanced-budget˙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˙why˙2018**) and passage of the VRA (**ang˙40-year-old˙2019**; **fresh˙effect˙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˙enfranchisement˙2022**).

Because the theory leads to ambiguous predictions of the overall effect of minority enfranchisement, I proceed to develop a strategy to empirically test for 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 local preferences for taxation, redistribution, and public goods.[13] 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 could 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 legislation or geographically-concentrated economic shocks) and that affected our 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˙effect˙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 **??**).

[13] 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.

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 **??** 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.

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:

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

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. $\%Non-white_c^{1960}$ is the share of the population of county c that was not white in 1960.[14] X_{cpt}' includes fixed effects for county characteristics measured in 1960 – share non-white, median family income, share with a high school diploma, and the employment-to-population ratio – interacted by year. Following **dube˙minimum˙2016**, I also include county fixed effects μ_c and pair-specific time effects τ_{pt}, which control for any pair-specific features that vary over time. These features could include time-specific shocks to the pair (e.g., a one-time shock to the local labor market) or characteristics that vary among the pair over time (e.g., demographic changes).

The event study design allows me to inspect for pre-trends and dynamic treatment effects. To estimate the overall effect of Section 4 coverage, I estimate

[14] 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.

$$y_{cpt} = \theta \text{VRA}_{cpt} + \beta \text{VRA}_{cpt} \times \% \text{ Non-white}_{c}^{1960} + X'_{cpt}\gamma + \mu_c + \tau_{pt} + \varepsilon_{cpt} \tag{2}$$

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

Given the spatial patterns of coverage, it might be too much to assume that outcomes would have evolved in parallel between covered and never covered counties in the absence of Section 4 coverage, even if we restrict attention to adjacent counties. Instead, I follow the existing literature and impose a weaker assumption: that, absent the Section 4 coverage, trends in outcomes between covered counties and their never covered neighbors would have evolved in parallel for counties with larger and smaller non-white population shares. Under this assumption, the effect of the Section 4 coverage on the relative change in outcomes between counties with higher and lower non-white shares β_k is causally identified.

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

Since treatment is strongly correlated within states, I also cluster estimates for the main results at the state level for robustness. Because there are few states in the sample, I conduct inference implementing the wild bootstrap routine from **cameron˙bootstrap-based˙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. **??** shows 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. I refer to all counties in a pair where the covered county was covered in 1965 (1975) as the 1965 (1975) cohort.[15]

[15]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 1975 or 1976. For simplicity, I nevertheless refer to these as the 1965 and 1975 cohorts. Finally, some counties were covered following passage of the 1970 amendments. While I include these counties when describing statistics or results for the full sample, this is a relatively small group and so I do not break out any results for this group separately.

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 a series of robustness checks in **??**.

An individual county may appear in the sample multiple times if it is in multiple pairs. Overall, there are 764 counties in the full sample, representing 375 unique counties. The full sample includes 381 counties that were ever covered under Section 4, representing 176 unique counties. The main sample has 474 counties (225 unique counties), of which 237 were ever covered (120 unique counties).

4.3 Data

4.3.1 Government Finances and the Structure of Local Government

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

Much of these data have been digitized and can be accessed electronically. For 1972 onwards, complete CoG data are available through the Census Bureau. For 1962 and 1967, data on counts of local governments are hosted on the Interuniversity Consortium for Political and Social Research (ICPSR).

Data for earlier years are not as readily accessible. I digitized data on government finances for 1957-1967 and data on government organization prior to 1957 from scans of historical CoG reports available through the Census Bureau. For these earlier years, data on some outcomes can be found for county governments, aggregated to the county level, for large municipal or township governments (those with populations over 5,000 in 1957 or over 10,000 in 1962 or 1967), and for large independent school districts (those with enrollment over 300 in 1957 or 3,000 in 1962 or 1967).

Due in part to incomplete coverage for individual local governments in earlier years, I aggregate all outcomes to the county level for the main results. Once aggregated, data on most outcomes reported in this paper are available for 1957 onwards. This is important in order to be able to examine whether there were different trends between covered and never

covered counties in the period *before* passage of the VRA. However, data are not available for housing or corrections expenditures in 1957.

4.3.2 County Characteristics

Throughout this paper, I use data on county characteristics as controls in regression estimation and to explore potential mechanisms. Data on most county demographic and economic characteristics, including 1960 baseline characteristics used as controls in the estimating equations, come from the *County and City Data Books*. Data on county population (including breakouts by race and for school-age children) and income come from the Census Bureau. Data on public school enrollment comes from the Office of Civil Rights school district surveys.

Data are described in fuller detail in the **??**.

4.4 Descriptive Statistics

?? 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 neighbors. This panel shows that Section 4 counties and their neighbors were very similar in 1960. Differences are generally statistically insignificant and, where differences do exist, they are small. Crucially, Section 4 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.

?? 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 **??**. 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 Section 4 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 Section 4 counties are racial minorities, compared to less than five percent in never covered counties.

5 Effects on Revenues and Expenditures

Previous research shows that different provisions of the VRA were successful in increasing voter registration and turnout (**ang˙40-year-old˙2019**; **cascio˙valuing˙2014**; **aneja˙effect˙2019**). By increasing Black political power, the VRA could have implications for taxation and redistribution. 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 whites, changing their preferences for taxation, redistribution, and public goods. In this section, I explore how Section 4 coverage affected levels of revenues and different categories of expenditures.

5.1 Revenues

To estimate the effect of Section 4 coverage on public finances, I estimate **??** using OLS. Prior to estimation, all outcome variables are normalized for population and log transformed.

?? presents 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 **??** to recover a single, pooled estimate of the effect of Section 4 on revenues. **??** 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, and employment-to-population ratio, each interacted with year. The preferred results can be found in column 4, with county and pair-year fixed effects and controls.

Column 4 of **??** shows that relative revenues per capita fell by .0028 log points for every one percentage point increase in the non-white share. The average county in the main sample has a non-white share of roughly 24%. Among covered counties, the difference in revenues per capita between a county with the mean non-white share and an all-white county fell by about 7.1% relative to the difference between two such never covered counties. Additionally, total taxes fell by 12.4% at the mean and property taxes by 13.3%. While the coefficient on

federal and state transfers are not statistically significant at the .05 level, point estimates suggest a decrease of about 7.6% and 7.3% at the mean, respectively.

For robustness, I repeat the above exercise using the full sample. The graphs in **??** are very similar to the analogous graphs for the main sample, except pre-existing trends are now much closer to zero. Findings from the pooled regression presented in **??** are consistent with the results for the main sample, though point estimates are somewhat attenuated.

The results for the main sample are largely robust to clustering at the state, instead of the county, level. **??** reproduces estimates and p-values from models estimated clustering standard errors at the county level. The last column instead clusters at the state level, using the wild bootstrap routine from **cameron˙bootstrap-based˙2008** to estimate p-values. While effects on general revenues are no longer significant at the .05 level, effects on taxes and property taxes are significant.

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.

?? displays event study estimates of the effect of Section 4 on log expenditures per capita. Note that data on housing and corrections are not available prior to 1962, so I am unable to assess whether there are pre-existing trends for these variables prior to the passage of the VRA.

?? presents results from the pooled regression of Section 4 on log expenditures per capita. Point estimates are negative for education, health care, policing, housing, and corrections, consistent with the observed decline in revenues. Most of the spending cuts seem to fall on education: Education spending declines by .0044 log points, which corresponds to an 11.1% decline at the mean, though this estimate is not statistically significant. Given that education expenditures make up, on average, 61% of revenues among the main sample at baseline, an 11.1% decline in education spending would account for most of the observed decline in revenues. For the remaining expenditure categories, visual inspection of the event study graphs in **??** reveal that estimates tend to be noisy and imprecisely estimated for many outcomes. For example, health care outcomes seem to be on a sharp upward trajectory before passage and fall back to pre-VRA levels shortly after passage. Additionally, policing expenditures appear to be falling modestly prior to passage of the VRA.

Robustness checks over the full sample largely support these findings. Event study graphs in **??** show that, after pooling the full sample, pre-existing trends tend to be close to zero for

all expenditure categories where data are available, and point estimates tend to be less noisy. The findings again suggest that Section 4 led to declines in education spending (though the results are more modest than before and still statistically insignificant), health care, and policing (though spending recovers in the long-run). As with the main sample, housing and corrections expenditures are unchanged in the twenty years following passage of the VRA.

?? compares p-values from clustering at the county versus the state level. Effects on policing are no longer significant, but effects on health and hospital expenditures continue to be significant. Effects on other outcomes continue to be insignificant.

6 How and Why Did Revenues and Spending Fall?

So far, I have only established that revenues and expenditures declined for covered counties with greater non-white shares. In this section, I explore why revenues and spending fell and explore mechanisms by which covered counties may have kept taxes low.

6.1 Changing Preferences for Taxation, Redistribution, and Public Goods

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.

6.1.1 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. **??** 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 6.7 percentage point increase in the likelihood that a housing unit is owner occupied, but a 2.4% decline in the median home value. 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 **??** 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.

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 used to summarize the economic health of a population or region more generally, which may be correlated the size of a tax base. Finally, many transfers from higher levels of government are based on a county's economic health (or lack thereof). For example, some federal transfers are reserved for places with high shares of 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. However, **??** finds no evidence that covered counties with greater non-white shares experience any relative 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. **??** 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, though statistically insignificant, *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 Section 4 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 way not captured by the above measures.

These investigations find no evidence that counties' economic health is falling in ways that might affect various tax bases. The findings suggest that it is unlikely that the reduction

in spending is a mechanical response to underlying trends.

6.1.2 Are individual preferences for public goods changing?

The 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 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 **??** for additional details).

?? shows that from 1968 to 1972, covered counties see a statistically insignificant relative decline in public school enrollment of 3.8% for every 10 percentage point increase in their non-white share. White and Black students all are less likely to be enrolled in public school, though the decline is larger for white students: a 10 percentage point increase in the non-white share is associated with a 4.0% decline in public school enrollment for white students and a 2.7% decline for Black students, though results are not statistically significant. 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˙120˙1993**), 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.

6.2 Increasing Local Control

If individuals' preferences shift so that they tend to prefer less taxation overall, we might expect to see these shifts reflected in policy changes. The most direct way for policymakers to implement constituents' changing preferences for taxation would be to reduce taxation directly through changes to the tax schedule, which I do not observe.

However, observers have previously documented other policy levers local communities use to maintain local control over public finances and implement their preferred policies. In particular, scholars have long recognized that government formation is a tool communities can use to preserve control over what happens within the boundaries of a local jurisdiction. By forming new cities, communities "can maintain more exclusive control over taxation, service levels, and the character of the population" (**burns˙formation˙1994**). Of particular relevance, some scholars have argued that white communities have incorporated to preemptively block annexation and any corresponding increases in taxation or expansions in social services (**miller˙cities˙1981**). In such cases, we might expect higher levels of municipal incorporation in places that prefer to keep taxes low.

I assess the effects of Section 4 coverage on various measures of fragmentation by estimating **??** using Poisson regression. Visual inspection of **??** 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.

?? presents the corresponding estimates from the pooled regression estimated using **??**. For a 10 percentage point increase in the non-white share, there is a relative increase .032 log points in the per capita number of municipalities, or an 8.1% increase for a covered county with the mean non-white share relative to an all-white county. Furthermore, there is an increase of .0056 log points in the per capita number of special districts – an increase of 14.7% at the mean – though this increase is not statistically significant.[16]

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

[16]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.

to incorporate, preventing annexation from larger cities that might impose higher taxes (**kruse˙white˙2007**). On the other hand, unincorporated areas in never covered counties create fewer cities, which might be the result of unincorporated 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 **??**).

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.

7 Conclusion

The Voting Rights Act of 1965 sought to fulfill the promise of the Fifteenth Amendment and secure nondiscrimination in voting for racial minorities. Previous research shows that the VRA largely succeeded in achieving its first-order goal of increasing Black political participation. The literature also finds that, by increasing Black political power, the VRA helped Black communities achieve more favorable 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 preferences for taxation, redistribution, and public goods more broadly. I find that places with greater non-white shares saw relative declines in revenues and taxation. Among Section 4 counties, places with a 10 percentage point increase in the non-white share experienced a 2.8% decrease in per capita general revenues, including a 4.9% decline in total taxes per capita and a 5.2% decline in property taxes per capita. I also find negative effects on various categories of expenditures, consistent with the decline in revenues, though estimates are generally noisier and less precisely estimated. Importantly, I find that a 10 percentage point increase in the non-white share leads to a 4.4% decline in education expenditures in Section 4 counties, though this estimate is not statistically significant.

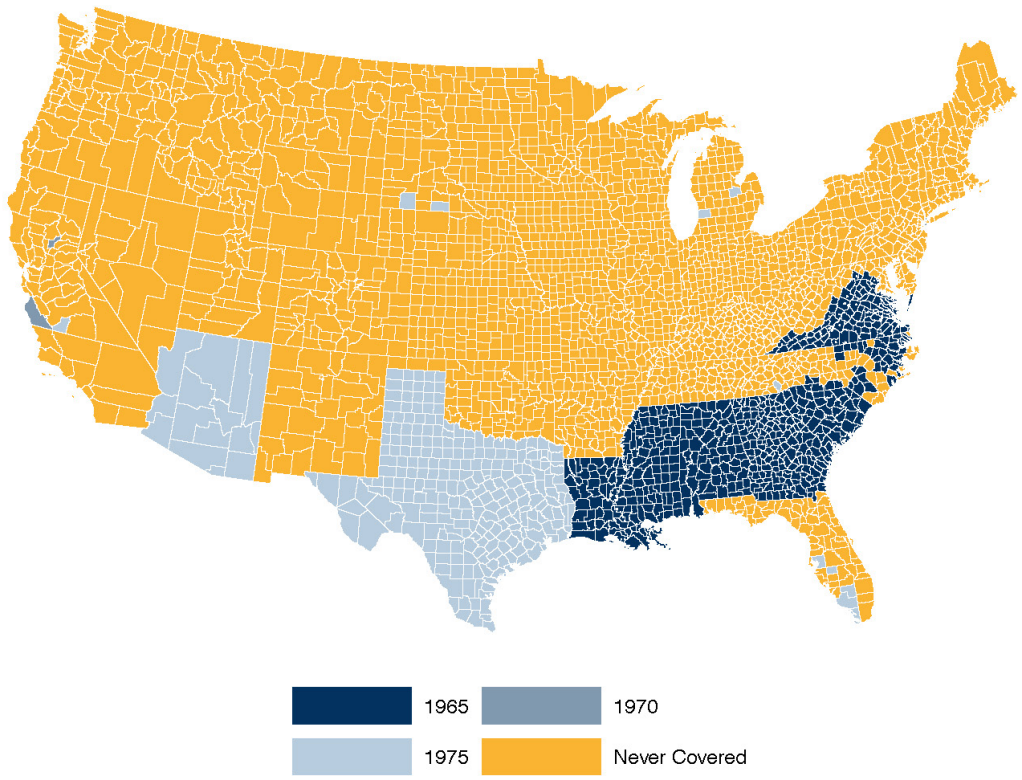
Further exploration of mechanisms suggests it is unlikely that these changes were simply mechanical responses to secular declines in various tax bases. Rather, the analysis in this paper shows that families in Section 4 counties may have responded by shifting their preferences from public to private goods, raising the possibility that local communities responded

to Section 4 coverage by demanding lower taxes as well. Consistent with this possibility, I find that communities in Section 4 counties see greater government fragmentation, consistent with an increase in incorporation, a strategy communities have historically used to block annexation and keep taxes low, and to preserve local control over resources more generally.

It is important to note that the results presented here do not imply that the VRA was 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 analogous changes in never covered counties. 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 is distributed more evenly, then it is possible that the VRA increased consumption even if the size of the pie shrunk. However, I do not assess this possibility here. Finally, the ultimate measure of the VRA lies not not 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 "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.

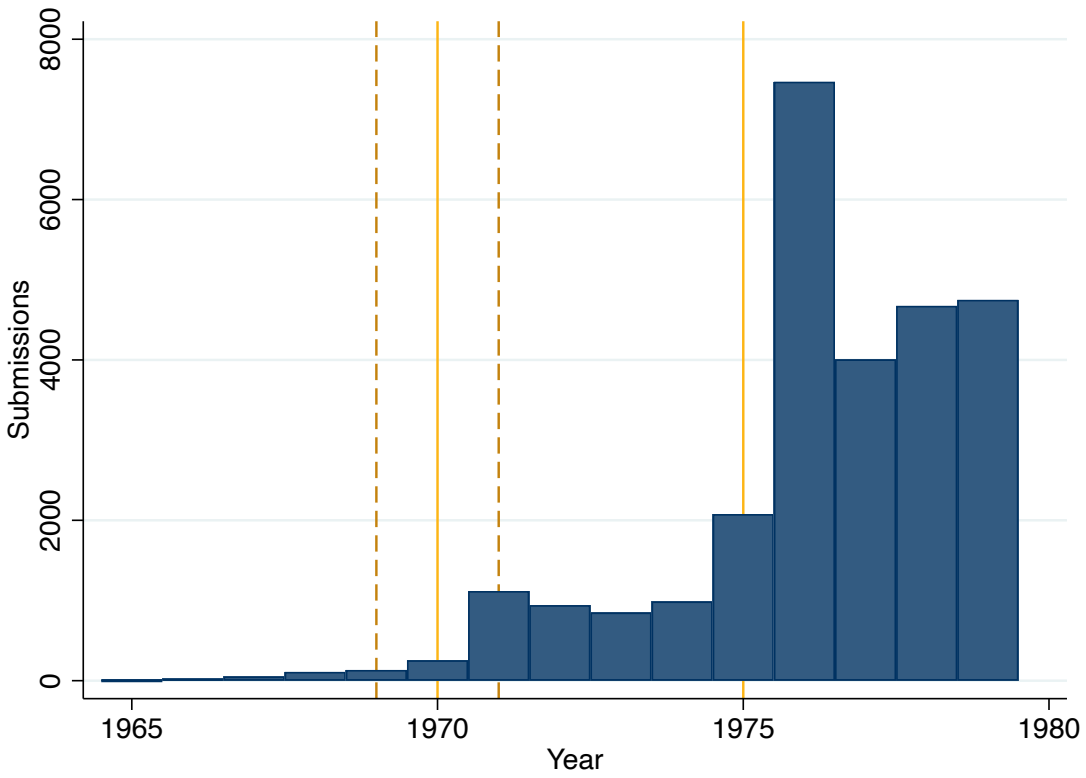
8 Figures and Tables

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



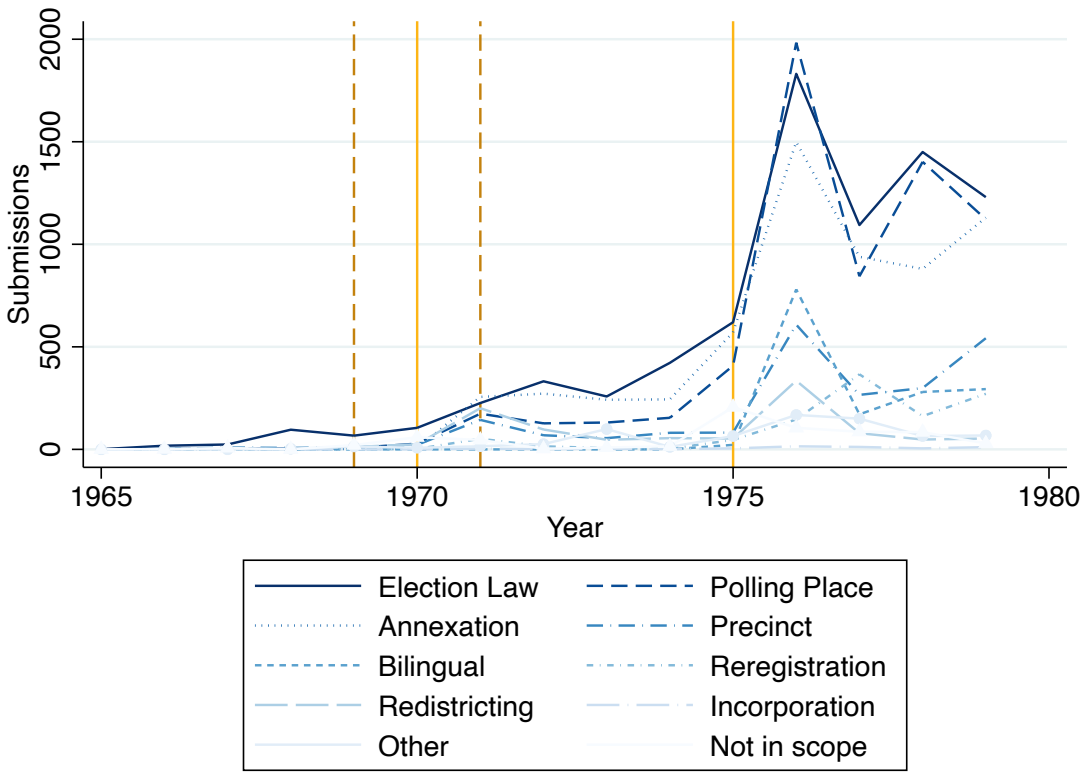
Source: Department of Justice

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˙compromised˙1982**.

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˙compromised˙1982**.

Figure 4: Counties Covered Under Section 4 and Their Neighbors

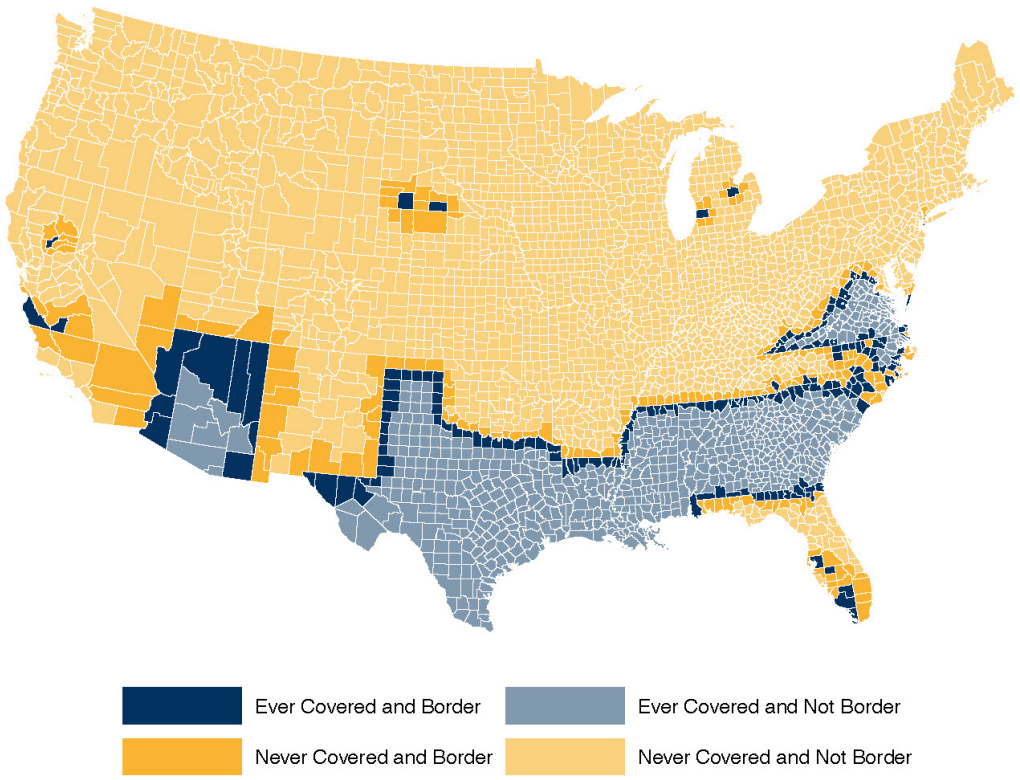
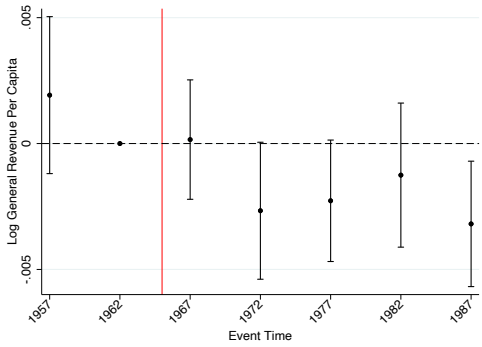
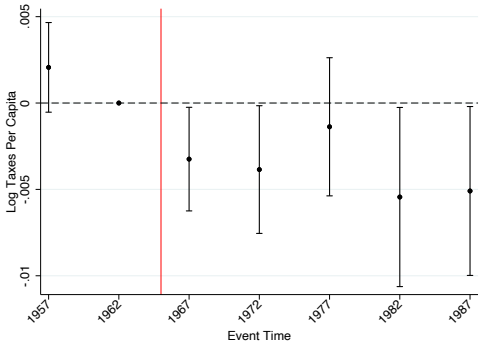


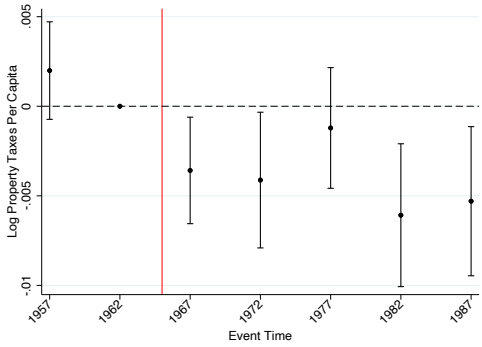
Figure 5: Effect of Section 4 Coverage on Revenues, 1965 Cohort



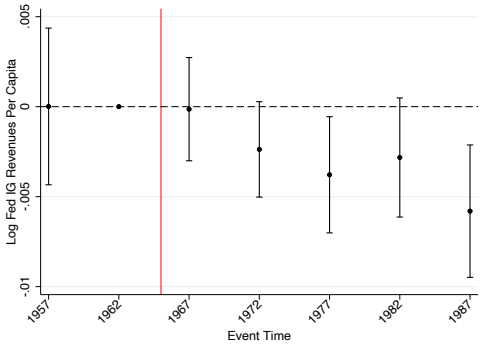
(a) General Revenues



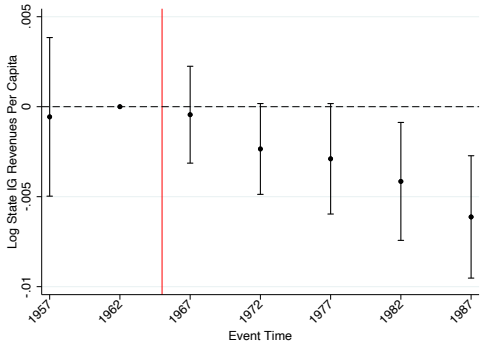
(b) Total Taxes



(c) Property Taxes



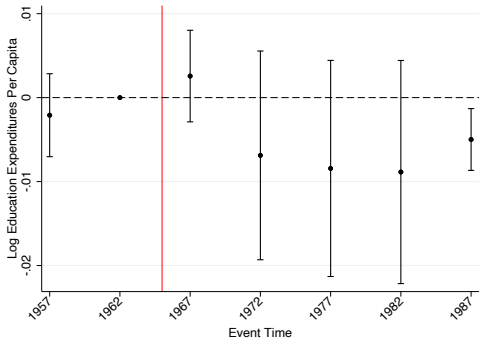
(d) Federal IGR



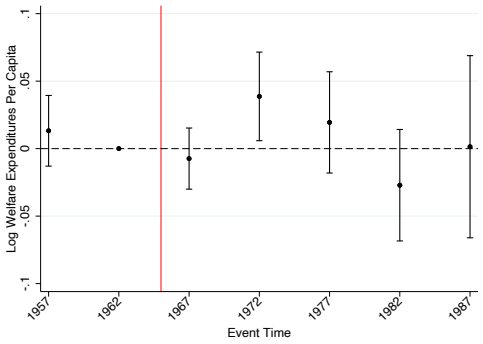
(e) State IGR

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.

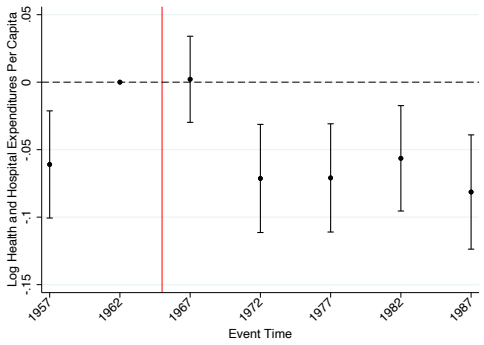
Figure 6: Effect of Section 4 Coverage on Expenditures, 1965 Cohort



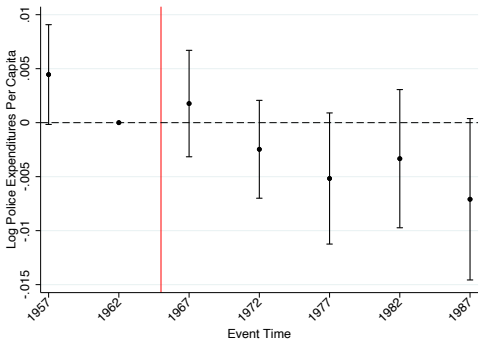
(a) Education



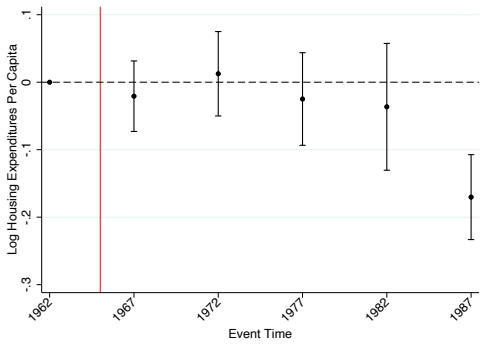
(b) Welfare



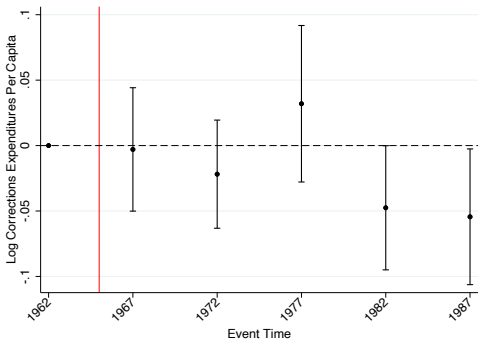
(c) Health and Hospitals



(d) Police



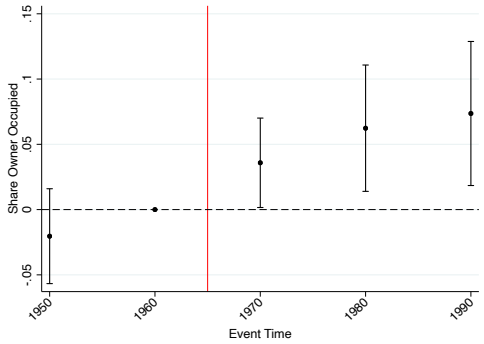
(e) Housing



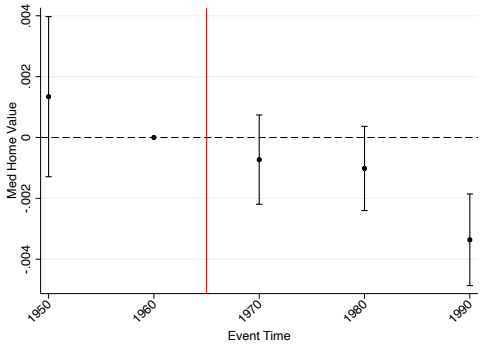
(f) Corrections

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. No data on housing or corrections expenditures in 1957. 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 Homeownership and Home Values, 1965 Cohort



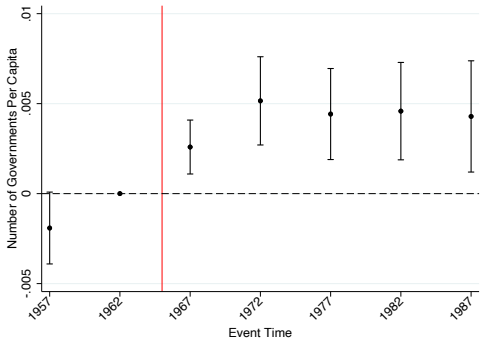
(a) Owner Occupied



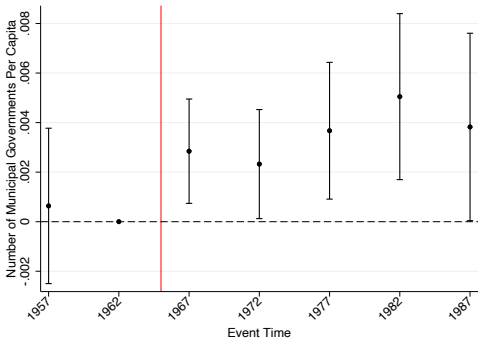
(b) Median Home Value

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. Median home value is log transformed. Standard errors are clustered at the county level. Vertical line represents date of initial passage of VRA.

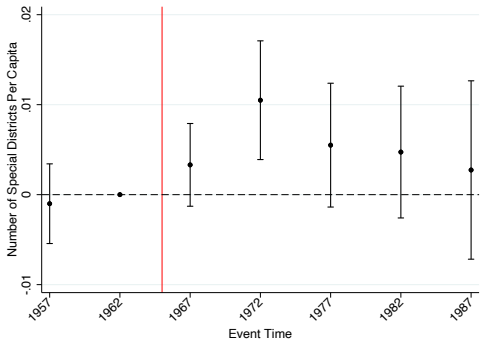
Figure 8: Effect of Section 4 Coverage on Fragmentation, 1965 Cohort



(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.

Table 1: 1960 County Characteristics by Section 4 Coverage Status

Characteristic	Ever Covered	Never Covered	Difference	p-value
A. CBP sample, 1965 cohort				
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		
B. CBP sample				
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. 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 counties in the contiguous border pair sample.
* p< .1 ** p< .05 *** p< .01

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

		Outcome	(1)	(2)	(3)	(4)	
General Revenue	-0.0024*	-0.0026*	-0.0026**				-0.0028***
	(0.0014)	(0.0013)	(0.0010)				(0.0010)
Total Taxes	-0.0043**	-0.0044**	-0.0044***				-0.0048***
	(0.0019)	(0.0019)	(0.0016)				(0.0015)
Property Taxes	-0.0040**	-0.0042**	-0.0046***				-0.0051***
	(0.0018)	(0.0017)	(0.0015)				(0.0013)
Federal IGR	-0.0024	-0.0025	-0.0029*				-0.0030*
	(0.0020)	(0.0020)	(0.0016)				(0.0016)
State IGR	-0.0016	-0.0017	-0.0024				-0.0029*
	(0.0020)	(0.0019)	(0.0016)				(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	3318	3318	3318				3318

Notes: The table reports coefficients on interactions between an indictator 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.
* p< .1 ** p< .05 *** p< .01

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

Outcome	(1)	(2)	(3)	(4)
Education	-0.0034	-0.0036	-0.0038	-0.0043
	(0.0041)	(0.0041)	(0.0043)	(0.0043)
Welfare	-0.0027	-0.0021	-0.0031	-0.0016
	(0.0114)	(0.0115)	(0.0137)	(0.0137)
Health and Hospitals	-0.0211	-0.0222	-0.0230**	-0.0251**
	(0.0143)	(0.0139)	(0.0116)	(0.0108)
Police	-0.0059**	-0.0064***	-0.0057**	-0.0055**
	(0.0027)	(0.0024)	(0.0025)	(0.0022)
Housing	-0.0228	-0.0208	-0.0464*	-0.0480*
	(0.0272)	(0.0262)	(0.0269)	(0.0269)
Corrections	-0.0099	-0.0104	-0.0134	-0.0190
	(0.0245)	(0.0244)	(0.0207)	(0.0207)
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	3318	3318	3318

Notes: The table reports coefficients on interactions between an indictator 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.
* p< .1 ** p< .05 *** p< .01

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

Outcome	(1)	(2)	(3)	(4)	
Owner Occupied (%)	0.0843**	0.0779**	0.0811***		0.0674***
	(0.0399)	(0.0353)	(0.0238)		(0.0215)
Med Home Value	-0.0015	-0.0015	-0.0019**		-0.0024***
	(0.0014)	(0.0014)	(0.0009)		(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	2361	2352		2352

Notes: The table reports coefficients on interactions between an indictator 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. Median home value is log transformed. Standard errors are clustered at the county level and are presented in parentheses.
* p< .1 ** p< .05 *** p< .01

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

Outcome	(1)	(2)	(3)	(4)	
Med Family Income	0.0000	-0.0000	0.0001		-0.0007
	(0.0013)	(0.0010)	(0.0009)		(0.0006)
Per Capita Income	-0.0005	-0.0006	-0.0003		-0.0009*
	(0.0012)	(0.0010)	(0.0007)		(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	1887	1886		1886

Notes: The table reports coefficients on interactions between an indictator 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.
* p< .1 ** p< .05 *** p< .01

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

Outcome	(1)	(2)	(3)	(4)	
Population	-0.0007	-0.0003	-0.0009		-0.0011
	(0.0016)	(0.0013)	(0.0008)		(0.0008)
White (%)	0.0003	0.0002	0.0002		0.0002*
	(0.0002)	(0.0002)	(0.0001)		(0.0001)
Black (%)	-0.0002	-0.0002	-0.0002		-0.0002
	(0.0002)	(0.0002)	(0.0001)		(0.0001)
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	4116	4112		4112

Notes: The table reports coefficients on interactions between an indictator 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.
* p< .1 ** p< .05 *** p< .01

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

Outcome	(1)	(2)	(3)	(4)	
# Local Govt	0.0048**	0.0046**	0.0050***		0.0051***
	(0.0021)	(0.0018)	(0.0012)		(0.0012)
# Municipalities	0.0018	0.0023	0.0029**		0.0032***
	(0.0020)	(0.0019)	(0.0012)		(0.0010)
# Special Districts	0.0089*	0.0084*	0.0056		0.0056*
	(0.0049)	(0.0049)	(0.0034)		(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	3318	3318		3318

Notes: The table reports coefficients on interactions between an indictator 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. Standard errors are clustered at the county level and are presented in parentheses.
* p< .1 ** p< .05 *** p< .01

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

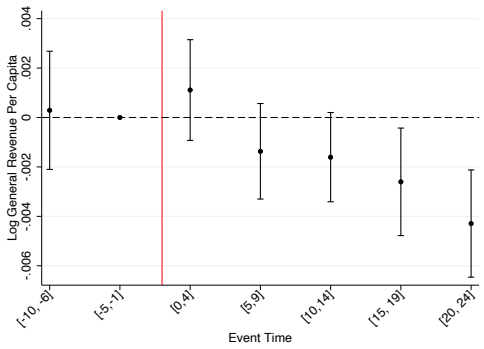
Outcome	(1)	(2)	(3)	(4)
Tot School-Age Pop				
1970	0.0003	0.0002	0.0001	0.0001
	(0.0004)	(0.0004)	(0.0003)	(0.0004)
1972	0.0005	0.0005	0.0001	0.0002
	(0.0008)	(0.0009)	(0.0007)	(0.0007)
White School-Age Pop				
1970	0.0002	0.0001	-0.0000	0.0000
	(0.0004)	(0.0005)	(0.0003)	(0.0003)
1972	0.0003	0.0003	-0.0000	0.0000
	(0.0009)	(0.0009)	(0.0006)	(0.0007)
Black School-Age Pop				
1970	0.0003	0.0003	0.0001	0.0002
	(0.0006)	(0.0006)	(0.0005)	(0.0005)
1972	0.0005	0.0006	0.0003	0.0004
	(0.0012)	(0.0011)	(0.0009)	(0.0009)
Tot Pupils				
1970	-0.0036	-0.0037	-0.0042	-0.0031
	(0.0024)	(0.0024)	(0.0025)	(0.0019)
1972	-0.0039	-0.0041	-0.0050	-0.0037
	(0.0032)	(0.0033)	(0.0036)	(0.0028)
White Pupils				
1970	-0.0102	-0.0102	-0.0100	-0.0100
	(0.0080)	(0.0078)	(0.0063)	(0.0064)
1972	-0.0038	-0.0038	-0.0038	-0.0039
	(0.0038)	(0.0036)	(0.0028)	(0.0029)
Black Pupils				
1970	-0.0027	-0.0027	-0.0028*	-0.0027*
	(0.0020)	(0.0019)	(0.0016)	(0.0016)
1972	-0.0024	-0.0023	-0.0027	-0.0027
	(0.0024)	(0.0022)	(0.0021)	(0.0021)
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

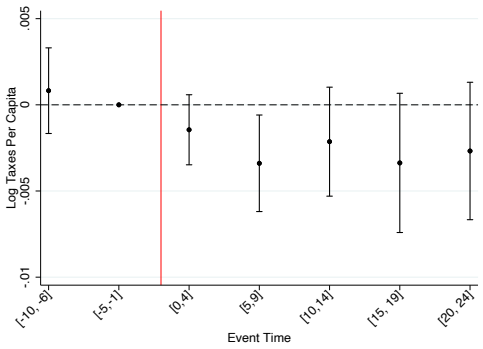
Notes: The table reports coefficients on interactions between an indictator for Section 4 coverage, an indicator for post-VRA, and non-white county share in 1960. Model is estimated using OLS and includes controls for county fixed effects, pair-year fixed effects, and 1960 county characteristics interacted with year. All outcomes are log transformed. Standard errors are clustered at the county level and are presented in parentheses.
* p< .1 ** p< .05 *** p< .01

A Additional Figures and Tables

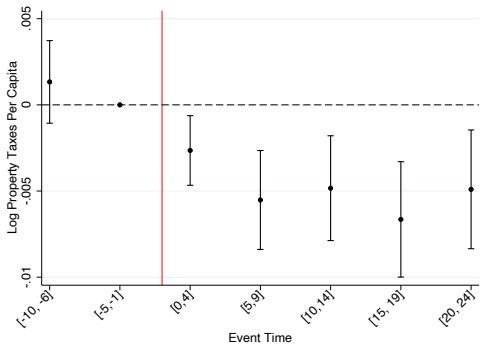
Figure A.1: Effect of Section 5 on Revenues, All Cohorts



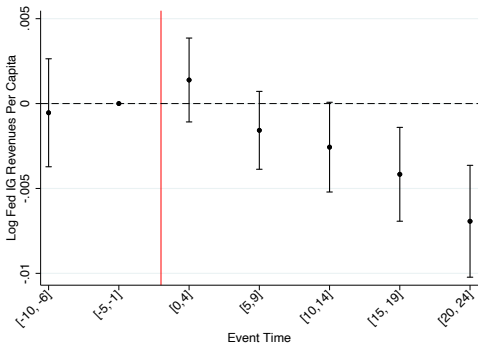
(a) General Revenues



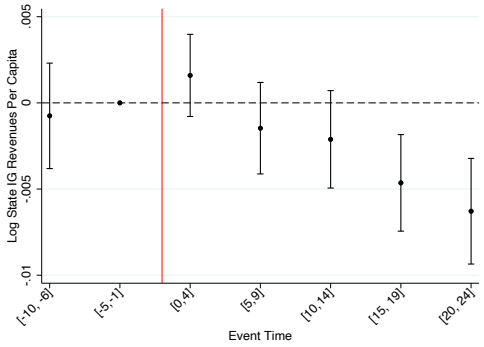
(b) Total Taxes



(c) Property Taxes



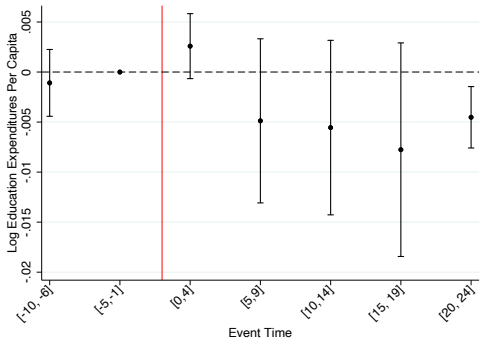
(d) Federal IGR



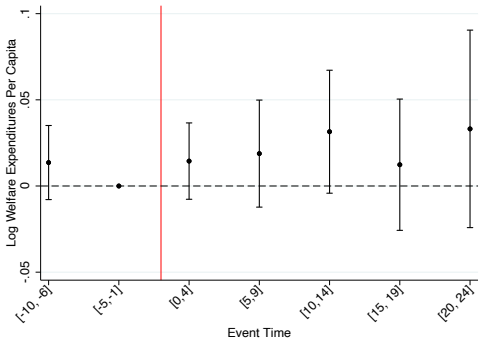
(e) State IGR

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.

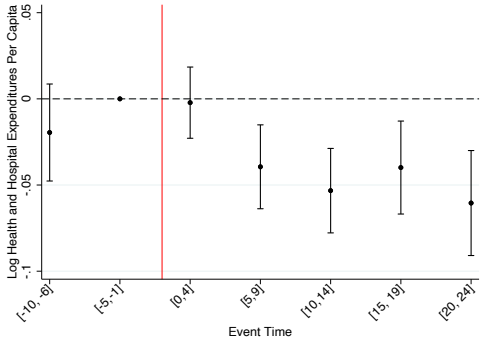
Figure A.2: Effect of Section 5 on Expenditures, All Cohorts



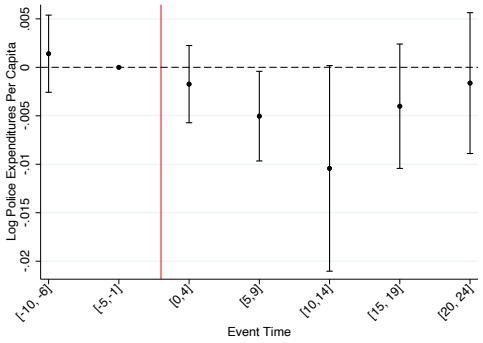
(a) Education



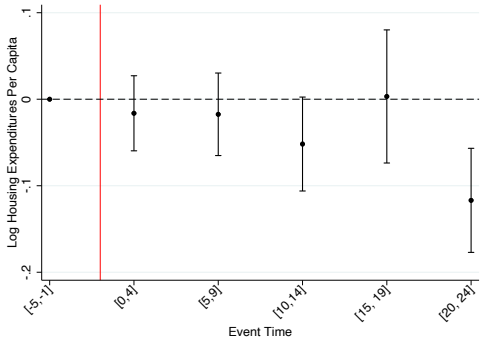
(b) Welfare



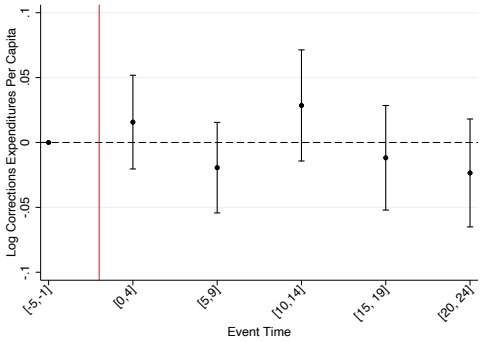
(c) Health and Hospitals



(d) Police



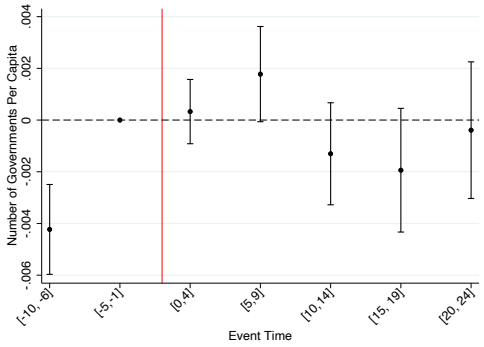
(e) Housing



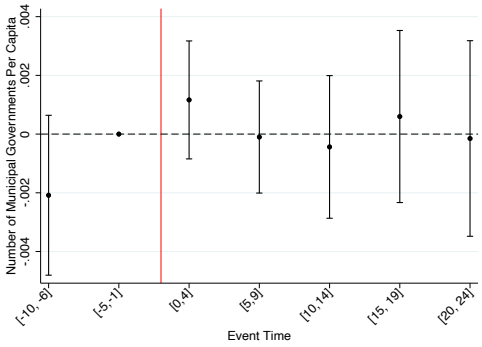
(f) Corrections

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. No data on housing or corrections expenditures in 1957. Standard errors are clustered at the county level. Vertical line represents relative time county was first covered by VRA.

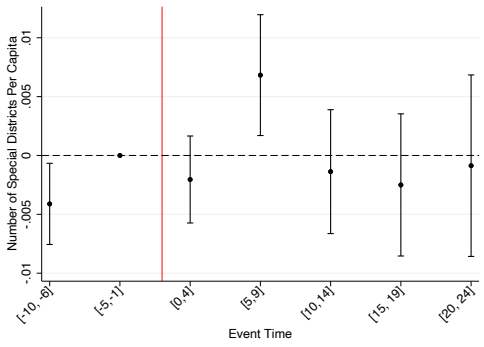
Figure A.3: Effect of Section 5 on Fragmentation, All Cohorts



(a) All Local Governments



(b) Municipalities



(c) Special Districts

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
A. Contiguous states, 1965 cohort				
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		
B. US counties				
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. 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.
* p< .1 ** p< .05 *** p< .01

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

		Outcome	(1)	(2)	(3)	(4)	
General Revenue	0.0011	0.0007	-0.0015				-0.0016*
	(0.0019)	(0.0017)	(0.0010)				(0.0009)
Total Taxes	-0.0041*	-0.0040*	-0.0031**				-0.0031**
	(0.0024)	(0.0023)	(0.0013)				(0.0013)
Property Taxes	-0.0059**	-0.0051**	-0.0058***				-0.0055***
	(0.0026)	(0.0024)	(0.0013)				(0.0012)
Federal IGR	0.0027	0.0018	-0.0010				-0.0019
	(0.0028)	(0.0025)	(0.0015)				(0.0013)
State IGR	0.0023	0.0012	-0.0005				-0.0016
	(0.0018)	(0.0016)	(0.0015)				(0.0013)
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	5320	5334				5306

Notes: The table reports coefficients on interactions between an indictator 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.
* p< .1 ** p< .05 *** p< .01

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

		p-values	
Outcome	Estimate	County	State
A. 1965 Cohort			
General Revenue	-0.0028	0.0058	0.3025
Taxes	-0.0048	0.0012	0.0375
Property Taxes	-0.0051	0.0001	0.0200
Fed IG	-0.0030	0.0653	0.5050
State IG	-0.0029	0.0643	0.4450
B. All Cohorts			
General Revenue	-0.0016	0.0900	0.4400
Taxes	-0.0031	0.0150	0.2425
Property Taxes	-0.0055	0.0000	0.0275
Fed IG	-0.0019	0.1568	0.4875
State IG	-0.0016	0.2310	0.4900

Notes: The table reports coefficients on interactions between an indictator 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 column presents p-values in models estimated clustering standard errors at the county level. The column presents p-values in models estimates clustering standard errors at the state level generated using the wild bootstrap method from **cameron˙bootstrap-based˙2008**.

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

Outcome	(1)	(2)	(3)	(4)
Education	0.0029	0.0006	-0.0006	-0.0029
	(0.0032)	(0.0026)	(0.0035)	(0.0030)
Welfare	-0.0037	-0.0017	0.0090	0.0132
	(0.0095)	(0.0099)	(0.0130)	(0.0123)
Health and Hospitals	-0.0190*	-0.0214**	-0.0268***	-0.0257***
	(0.0098)	(0.0097)	(0.0094)	(0.0088)
Police	-0.0111**	-0.0110**	-0.0073***	-0.0054***
	(0.0049)	(0.0047)	(0.0023)	(0.0019)
Housing	-0.0331*	-0.0350*	-0.0304	-0.0346
	(0.0185)	(0.0201)	(0.0222)	(0.0222)
Corrections	-0.0094	-0.0098	0.0057	-0.0035
	(0.0133)	(0.0142)	(0.0162)	(0.0158)
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	5320	5334	5306

Notes: The table reports coefficients on interactions between an indictator 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.
* p< .1 ** p< .05 *** p< .01

Table A.5: Comparison of Clustering on County or State, Expenditures

Outcome	Estimate	p-values	
		County	State
A. 1965 Cohort			
Education	-0.0043	0.3211	0.6675
Police	-0.0055	0.0116	0.2450
Health and Hospital	-0.0251	0.0213	0.0100
Welfare	-0.0016	0.9071	0.9625
Housing	-0.0480	0.0757	0.4050
Corrections	-0.0190	0.3602	0.6225
B. All Cohorts			
Education	-0.0029	0.3259	0.5800
Police	-0.0054	0.0050	0.0950
Health and Hospital	-0.0257	0.0036	0.0100
Welfare	0.0132	0.2835	0.7025
Housing	-0.0346	0.1200	0.2925
Corrections	-0.0035	0.8248	0.9175

Notes: The table reports coefficients on interactions between an indictator 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 column presents p-values in models estimated clustering standard errors at the county level. The column presents p-values in models estimates clustering standard errors at the state level generated using the wild bootstrap method from **cameron˙bootstrap-based˙2008**.

Table A.6: Effect of Section 4 on Fragmentation, All Cohorts

Outcome	(1)	(2)	(3)	(4)	
# Local Govt	-0.0012	-0.0022	0.0029***		0.0019**
	(0.0018)	(0.0017)	(0.0010)		(0.0009)
# Municipalities	-0.0005	-0.0004	0.0011		0.0012
	(0.0017)	(0.0017)	(0.0011)		(0.0009)
# Special Districts	0.0003	-0.0007	0.0034		0.0024
	(0.0033)	(0.0034)	(0.0025)		(0.0025)
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	5320	5334		5306

Notes: The table reports coefficients on interactions between an indictator 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. Standard errors are clustered at the county level and are presented in parentheses.
* p< .1 ** p< .05 *** p< .01

B Data Appendix

To Do